SCIENCE, LABOR AND SCIENTIFIC PROGRESS

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My dissertation introduces a new materialist theory of scientific progress built on a novel characterization of scientific work and an analysis of progress appropriate to it. Two questions, crucial for understanding scientific progress, are answered:

Why is it possible for scientists at a given time to have more epistemic abilities than scientists at an earlier time?

How can knowledge acquired in the past be used in on-going or future research?

I argue that these questions are best answered by analyzing science as a form of labor. The elements of the labor process, involving both intellectual and material means, provide a starting-point for the systematic study of how scientific abilities evolve.

As a unit of analysis, the labor process exposes features of the dynamics of knowledge accumulation that traditional analyses do not. I analyze historical cases from chemistry and the Scientific Revolution, attending carefully to how scientific work is conducted and conceived. First, I argue that scientific progress consists not just in the growth of theoretical or empirical knowledge, as in traditional philosophy of science, but also in the growth of know-how. The tools of science play a crucial role in determining the abilities scientists can and must have to do science. Tools also determine how scientists' abilities change over time, by enabling, but also constraining, the incorporation of knowledge into the labor process. I argue that an extremely important mechanism of progress in science consists of a feedback loop between the production of new knowledge and instrument construction. This process requires the integration, and transformation into material form, of different kinds of knowledge. As the process is repeated over the long term, scientific work is transformed because it becomes less dependent on native human epistemic abilities.

Second, the evolution of scientific abilities depends on ambient ideological conditions: Social attitudes towards different kinds of work are critical, as are notions about the proper object of science.

What results is a picture of scientific change involving the interactions of different kinds of knowledge and in which internal and external factors, as well as instrumental rationality, play a significant role.

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1.0 INTRODUCTION

What causes scientific progress? Progress is a kind of change, but it is not identical to change. As will be discussed in more detail in chapter 2, progress is a goal-relative change, where the goal can be either backward-looking or forward-looking, depending on whether it is at the beginning or the end of an activity. A traditional answer to what causes scientific progress is that science has a special method of inquiry, the “Scientific Method,” the application of which leads to progress, in the sense that scientists either move closer to the destination (knowledge, truth, approximate truth, or whatever) or farther from the starting-point (e.g., later theories are more effective at solving problems than earlier ones).

To ask what causes scientific progress is to ask a question about the *mechanism(s)* by which science makes progress. This is a different question than what *constitutes* scientific progress. The two questions are related, of course, because we need to know what progress is in order to identify the mechanisms causing it.

As I will discuss in more detail in chapter 2, the notion of progress adopted in this dissertation is that it is constituted by the growth of knowledge. The latter will be understood in a broad sense, involving not just theoretical knowledge, or propositional knowledge more generally, but also knowledge of how to do things, or what Ryle called ‘knowledge-how’ or ‘knowing how.’[1] I intend this project to be primarily explanatory and descriptive rather than normative. For that reason, I choose a conception of progress that is derived from scientific practice and that forecloses minimally on what is to count as ‘progress.’

Conversely, studying the mechanisms of change in science can reveal candidates for what might count as progress. Though this dissertation will be largely concerned with mechanisms responsible for progress, the study of such mechanisms will suggest novel candidates for what should constitute progress. The idea that progress may be constituted

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[1] Ryle (1949), chapter II.

by the transcendence of the limitations of native human epistemic abilities will be discussed in chapter 7.

This dissertation project starts from the view that whatever the "Scientific Method" amounts to, it involves a synthesis of manual and intellectual labor. This view is not original, and in fact it was the source of some controversy in mid-20th century debates on the origins of the Scientific Revolution. In what became known as the "Zilsel Thesis," Edgar Zilsel, a historian and philosopher of science and member of the Vienna Circle, claimed that "[s]cience was born when, with the progress of technology, the experimental method eventually overcame the social prejudice against manual labor and was adopted by rationally trained scholars."[2] On Zilsel's view, then, modern science results from a synthesis of manual and intellectual labor, and hence requires both manual and intellectual tools.

Not everyone in science studies has shared this view. The sociologist Joseph Ben-David wrote that "the subject matter of science is nature and the tools of science are systems of thought."[3] Alexandre Koyré, an influential historian of the Scientific Revolution, could not abide the idea that the development of technology or the rise of superior artisans had any significant impact on the rise of modern science:

> Their science [that of Galileo and Descartes] is made not by engineers or craftsmen, but by men who seldom built or made anything more real than a theory. The new ballistics was made not by artificers and gunners, but against them. And Galileo did not learn *his* business from people who toiled in the arsenals and shipyards of Venice. Quite the contrary: he taught them *theirs*. Moreover, this theory [of the influence of the crafts and technology on the Scientific Revolution] explains too much and too little. It explains the tremendous scientific progress of the seventeenth century by that of technology. And yet the latter was infinitely less conspicuous than the former. Besides, it forgets the technological achievements of the Middle Ages.[4]

He championed the arch-rationalist explanation that "it is thought, pure unadulterated thought, and not experience or sense-perception, as until then, that gives the basis for the 'new science' of Galileo Galilei."[5] Koyré and like-minded historians in the

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[2] Zilsel (2000 [1942]),), p. 544.

[3] Ben-David (1971), p. 1.

[4] Koyré (1968), p. 17.

[5] Koyré (1968), p. 13.

mid-20th century, such as A. Rupert Hall and Charles Schmitt,[6] were reacting to a recent trend in the history and historical sociology of science, exemplified by Zilsel and Leonardo Olschki among others, of explaining the Scientific Revolution by appealing to the role of artisans and technology.

In general, philosophers have been inclined to simply ignore the role of technology and the manual aspects of scientific work in their discussions of scientific progress. Perhaps one reason for this neglect is that the traditional philosophical focus has been on the justification of scientific theories. As a result of this focus on justification, philosophy of science has largely concerned itself with the intellectual aspects of scientific method. To the extent that progress is claimed to occur through theory change, the former consists of the fact that the successor theory is more justified than its predecessor. Given this focus, the cause of progress is the application of a method for producing better justified theories, where "method" is typically understood as a set of rules or procedures for choosing theories in light of evidence. A classic example of such an approach is Lakatos' (1970) rational reconstructions of the history of science. He argued that "philosophy of science provides normative methodologies in terms of which the historian reconstructs 'internal history' and thereby provides a rational explanation of the growth of objective knowledge."[7] Lakatos characterizes these "methodologies" as "a set of … rules for the *appraisal* of ready, articulated theories" and also as "theories of the rationality of scientific progress."[8] As Sankey and Nola (2007) note, this approach to the history of science makes methodology, so understood, "the main explanatory driver of scientific progress," with non-methodological factors being left to external history of science.[9]

More recently, even a practice-oriented philosopher of science like Hasok Chang has held that "if scientific knowledge is getting better, it must mean that our current scientific beliefs are more justified than our previous beliefs (or at least that we have a

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[6] Hall (1959); Schmitt (1969), pp. 89-90 and 94-95. For an overview of the reception of Zilsel's ideas by historians in the mid-20th century, see Conner (2005), pp. 276-281.

[7] Lakatos (1970), p. 91.

[8] Lakatos (1970), p. 92 and 105.

[9] Sankey & Nola (2007), p. 101. In their own book on *Theories of Scientific Method*, Sankey and Nola focus on justification as well (e.g., (2007), p. 20).

larger number of beliefs, which are as justified as the beliefs we used to hold)."[10] He explains scientific progress as the result of a process of "epistemic iteration" through successive standards of belief, with each succeeding standard building on, but also correcting, its predecessor. On this view, progress arises from the application of the appropriate criteria for accepting new standards of belief: the principle of respect for earlier standards, the greater precision of the later standards, the self-consistency of the later standards, their coherence with other things we believe, and the ability to coherently revise beliefs based on the earlier standards in light of the later ones. Progress comes about because scientists accept new standards for such reasons. The "mechanism of progress"[11] consists entirely in relating later standards of belief to earlier ones in this self-correcting and cumulative manner; the "technical infrastructure" of science has no place in the mechanism except as a possible object of belief.[12]

But science is as much about doing as about believing. Consider Chang's well-known account of the development of temperature standards. In *Inventing Temperature* (2004) and "Scientific progress: beyond foundationalism and coherentism" (2007), Chang describes a succession of increasingly precise instruments, starting with the hands and ending with the high-precision Beckmann thermometer, for estimating warmth.[13] Each instrument provided a standard for beliefs about the temperatures of bodies. Each instrument in the sequence also provided a standard for assessing the reliability of its successor. Once the successor was accepted, it in turn provided a standard for correcting measurements made with its predecessor. Thus the new beliefs formed with the successor

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[10] Chang (2007), p. 2.

[11] Chang (2007), p. 10.

[12] I here make use of Joseph C. Pitt's (2000, 2011) term 'technical infrastructure of science' which Pitt (2000, p. 136) defines as "*the historically defined set of mutually supporting sets of artifacts and structures without which the development and refinement of scientific knowledge is not possible*" (emphasis in original). Though Pitt's focus on the role of artifacts, institutions and division of labor in the development of scientific knowledge is similar to mine, Pitt appears to conceive of progress exclusively as theoretical progress, whereas I favor a broader conception, as will become clear in chapter 2.

[13] Chang (2004), pp. 47-48 summarizes the process of developing numerical thermometers starting from the senses; Chang (2007), pp. 9-11extends the analysis to Beckmann thermometers.

were more justified in virtue of the greater precision of the latter, and the old beliefs formed by means of the predecessor became more justified in virtue of the corrections. So there was overall progress in the justification of beliefs and the standards by which they were justified.

What this explanation leaves out are the conditions determining the invention, production, and use of the instruments themselves. Each new temperature standard had to be invented, produced and used, and each one of these activities was made possible by the conditions within which the scientists were working.[14] Here is a partial list of these conditions:

a. Goals
b. Means and objects of inquiry
c. Mental conceptions, including background knowledge and theory but also attitudes, cultural understandings and beliefs
d. Technological and organizational forms of production and exchange
e. Relations to nature, that is, the human-nature relationship and the instrument-nature relationship mediating it
f. The human-instrument relationship (e.g., is the instrument a manual implement or a machine?)
g. Institutional arrangements, e.g., publishing practices, credit systems and scientific societies.

All of these conditions are necessary for scientific action. Moreover, they are important for the growth of knowledge, because they determine what scientists are able to do towards acquiring knowledge. In other words, they determine scientific *abilities*. That is, they determine what experiments can be performed, what hypotheses can be tested, what kinds of phenomena can be discovered, what calculations can be performed, and so on.

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[14] To be fair, in subsequent writings (e.g., Chang 2012) Chang has broadened his analysis of scientific practice to include his well-known "systems of practice," which include "epistemic activities." I discuss these in chapter 2. That said, to my knowledge he has not updated his theory of epistemic iteration to incorporate systems of practice.

In short, philosophical theories of how knowledge grows tend to explain progress as the result of the application of a method consisting of rules of reasoning about new theories, standards, etc. This way of explaining the growth of knowledge is insufficient, however, because it neglects the conditions of scientific action.

For reasons I will provide shortly, this observation raises the following fundamental question for understanding how science makes progress:

1. Why is it possible for scientists at a given time to have more epistemic abilities than scientists at an earlier time?

For an epistemologically oriented explanation of scientific progress, a crucial ability to explain is how scientists are able to make use of background knowledge acquired in past research ((c) above). Scientific knowledge does not grow simply by the addition of one item of knowledge after another, but because scientists try to further their knowledge on the basis of what they have learned. This raises a second fundamental question:

2. How is it possible for knowledge acquired in the past to be used in on-going or future research?

Let me explain why these questions are important.

The first question concerns abilities. An ability is a power of an agent that relates the latter to an action.[15] So if we want to explain scientific actions, a complete explanation will include an account of why scientists are able to do what they do. For example, let's say we want to explain an event such as that chemist X analyzed chemical Y using technique Z. Clearly, part of the explanation would appeal to X's motives. But merely appealing to motives would be insufficient, for it leaves out of the explanation the factors enabling X to realize her motives. In order to understand why she has the ability to analyze Z, we need to understand the instrumentation, techniques, division of labor, background knowledge and beliefs that enabled her to carry out the analysis.

This example highlights the fact that focusing on abilities avoids excessively one-sided analysis in terms of either "objective" or "subjective" factors. For example, an ability depends on the means available for action, such as instruments or organizational structures. But it also depends on the properties of individual agents, such as their methods of

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[15] Maier (2014).

reasoning, beliefs, desires, attitudes, skills, etc. Put slightly differently, abilities connect the individual, the material and the social. Because it encompasses these factors, ‘ability’ is a broad enough category to explain scientific action and change.

This category may not be pleasing to empiricist historians and philosophers, who tend to be hostile to the use of modal notions. Better to focus on what scientists actually did, why they did it and how. Though defending the use of modal notions in historical explanations would require much more space than I can devote to it here, a short answer is that we want to be able to explain why scientists are *in a position* to do what they do, why the conditions are such that certain courses of action are feasible and others are not. Appealing to the motivations of the scientists, for example, is insufficient for explaining the particular course of action taken, since acting requires not just motivations but also the means to achieve them. Indeed, a motive that is concrete enough to explain an action has to be formulated in terms of the means available for satisfying it. It is not enough to want to analyze a chemical; one must want to use a particular technique in order to take an action towards analyzing it. But concretizing motives in this way requires an understanding of what can and cannot be done in given circumstances, and hence an understanding of what the agent is in a position to do.

From a historical perspective, explaining why scientists are in a position to act as they do provides insight into the long-term dynamics of scientific change, which is the time-scale with which this project is concerned. For example, if we want to know why discovery *A* was made at time *t* whereas discovery *B* was only made much later, then showing that scientists were in a position to discover *A* at *t* but were only in a position to discover *B* at a much later time provides a partial contrastive explanation of the sequence (partial because motivations would also play a role, of course). Another *explanandum* is the convergence of discoveries. Suppose two scientists, working independently, make the same discovery during a certain historical period. Showing that they were working under similar conditions—concerned with similar questions, using similar tools, presupposing similar background knowledge, etc.—helps to explain the convergence. Moreover, the identification of similar conditions may indicate that the science of the time was on a specific trajectory, in that the discovery would probably have been made during that period, even if these particular scientists had not made it themselves.

As historians and philosophers of science, however, we are not concerned with just any abilities, but with abilities that contribute to the production of knowledge. As I will discuss in more detail in chapter 2, an *epistemic* ability is the ability to engage in a mental or physical action that is intended to contribute to the production or improvement of knowledge in a particular way, and according to discernible rules. A striking characteristic of scientific change in the modern era has been that scientists have acquired a huge number of epistemic abilities that they did not have in the past, and this growth has been directly responsible for the growth of knowledge (and indeed might constitute such growth, on the view of knowledge adopted here). So if we want to explain the growth of knowledge, we will want an account of what makes the acquisition of these abilities possible.

This point holds even if the only knowledge we are concerned with is theoretical knowledge. In empirical science, the growth of theoretical knowledge depends on the acquisition of new empirical information, which in turn depends on scientists' abilities to acquire such. But theorizing itself also involves actions, and depends on the means available for theorizing, for example mathematical tools, calculation aids, and concepts. *A fortiori*, an account of what makes the acquisition of epistemic abilities possible is necessary if we want to explain the growth of knowledge in the broad sense invoked at the beginning of the chapter, which includes know-how. Indeed, abilities may be constitutive of the latter (I delay discussing this issue until the next chapter), in which case such an account is clearly needed if we are to explain the growth of that knowledge. Even if know-how turns out not to consist of abilities, or even to be propositional after all, as some philosophers believe, the relevance of the know-how for explaining scientific change (and therefore progress) depends on whether scientists are in a position to exercise it. For example, let us grant that a scientists knows how to build an instrument in the purely discursive sense of being able to provide a set of instructions for how to build it. If she is not in a position to actually build the instrument, the trajectory of her research will presumably be different from the trajectory when she is in such a position and acts on it. Thus, the relevance of the know-how for explaining scientific changes depends on whether scientists have the corresponding ability. It follows that abilities remain relevant, even if know-how is conceived independently of them.[16]

The second question concerns the application of prior knowledge. Another feature of scientific progress is that once knowledge is acquired, it does not necessarily lie idle on a shelf but is potentially useable for the purpose of acquiring new knowledge. Thus science is able to make use of what it has learned in order to learn more, and indeed the mechanisms by which it does so seem to be responsible for much of its progress. Among intellectual endeavors, science is perhaps uniquely able to incorporate what it learns not just into its thinking, but also into its conditions of action. Thus the two questions are interrelated, for the growth of knowledge does not remain external to the process by which knowledge is acquired but modifies it, enhancing scientists' epistemic abilities. Some mechanisms by which it does so will be the subject of chapter 7. As will be discussed in chapter 8, the application of prior knowledge is a major driver of changes in scientific practice. A complete explanation of scientific progress will need to provide an account of how this is possible.

The dissertation may be viewed as an application of the Zilsel thesis to the topic of scientific progress. My contention in this dissertation is that thinking about science as a labor process is a useful way of addressing these questions. Tools (including past results), activities, skills, division of labor, products, and "ideological" conditions (e.g., attitudes towards work) are all involved in doing science. In the labor process, the various conditions of scientific action are combined and enable the agents of the process to engage in production. So if we want to explain how and why the agents are able to produce what they produce, we need to understand science as labor. We also need this understanding to explain how prior knowledge can be used in on-going research. Rules of reasoning are important, of course, but so are these other aspects of scientific work that tend to be overlooked by philosophers of science.

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[16] For a discussion of some of the issues involved in applying the category of know-how to scientific practice, see Chang (2017). See Stanley & Williamson (2001) for an influential reductionist account of 'knowing how' in terms of knowledge-that.

I will provide support for these claims by examining some historical cases. The dissertation is structured as follows. Chapter 2 defends the labor process as a unit of analysis for studying the growth of scientific abilities, and relates that unit to the notion of scientific progress. At the core of the labor process is the relation of production between humans and instruments. This relationship is essential for understanding the agents' abilities, manifested in production, and for the evolution of those abilities over time. Moreover, it is essential for understanding how prior knowledge can be exploited in production, including in the production of knowledge. Chapter 3 is about how modern experimental science was constituted in the 17th century. It is argued that social attitudes towards different kinds of work, notions about the proper object of science, and beliefs about how instruments and human faculties are related were important factors in the emergence of an experimental science in the early modern period. Thus, what might be called 'ideological' conditions affecting the evolution of scientific abilities are identified. Chapters 4, 5 and 6 concern the Instrumental Revolution in chemistry, a period of rapid change in chemical instrumentation in the mid-20th century. In chapter 4, this episode is analyzed as a transformation of labor. It is argued that this episode was an instance of a distinct kind of revolution in science, involving radical changes in the means of production and corresponding changes in the cognitive aspects of chemical analysis. Chapter 5 explores some of the impacts of the transformation on the rate of progress of the field. The impact on the rate of progress is assessed by examining how heuristic features of chemical analysis were affected by the changes. It is argued that methods development in chemical analysis became more dynamic and that compound identification became more efficient. In chapter 6, some classical models of scientific change are applied to this case and critiqued. An alternative model is developed based on this critique. The model builds on Larry Laudan's (1984) "reticulated" model of scientific change, significantly augmenting it with new categories and modifying the old ones. It is argued that the new model is more descriptively accurate of scientific practice, and that it addresses questions (1) and (2) above better than Laudan's and the other models do, because it, unlike they, incorporates changes in instrumentation and systems of labor. Chapter 7 elaborates on one particular aspect of this model, the relationship between instruments and prior knowledge. A novel mechanism of progress, involving the interaction of instrument construction and prior knowledge, is identified that, I argue, suggests a novel way of understanding scientific progress. In chapter 8 I argue that the focus on abilities has the potential to transform our understanding of old philosophical issues. Directions for future research are also suggested.

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2.0 MODELS OF SCIENCE AND SCIENTIFIC PROGRESS

2.1 Introduction

The purpose of this chapter is threefold. First, I would like to situate the view of science as labor with respect to the larger science studies literature. I have two reasons for this. Though the view of science as labor is not original to me, as was pointed out in the previous chapter, it is nevertheless a heterodox view and will perhaps be unfamiliar to many readers trained in mainstream history and philosophy of science. Furthermore, situating this view with respect to the larger literature will help make both the proposed unit of analysis—the labor process—and the rationale for using it, clearer and more precise.

Second, I would like to elaborate on the two key concepts introduced in the previous chapter, scientific progress and epistemic abilities. Finally, I would like to show why a labor-process analysis is promising for answering the two questions concerning abilities and knowledge posed in the introduction.

This chapter is structured as follows. The second section will discuss contemporary philosophical views on the nature of scientific progress and argue for the adoption of one, recently proposed by Moti Mizrahi, for the purposes of this dissertation. In the third section, three models of how science has been thought about in science studies will be discussed. What one might call an "intellectualist" model will be considered, according to which the interesting products of science are ideas, and scientific method is cashed out in terms of cognitive procedures. This model has dominated traditional philosophy of science. Then the labor process model will be discussed, a model that aims to do justice to the obviously crucial roles of expertise and conceptual thought in science while simultaneously emphasizing the equally crucial importance and historicity of material practices therein. Finally,[17] there is also a "sociological" model, which focuses on interactions between

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[17] As far as this dissertation is concerned. I do not claim to provide an exhaustive list of models of science in science studies.

scientists or on institutions in order to explain the actions of scientists or the genesis of scientific facts and concepts.

Though the intellectualist model has dominated discussions of scientific progress in history and philosophy of science, it tends to underestimate the role of abilities and instruments in scientific change and progress. On the other hand, a concern with these elements of scientific practice falls naturally out of the labor process model. Among authors who have considered science as a form of labor, however, there has not been much interest in applying this model to the analysis of the causes and nature of scientific progress.

2.2 Scientific progress

Although the issue of scientific progress is not as central in the preoccupations of historians and philosophers of science today as it was in the 20th century, it continues to attract interest from philosophers for at least two reasons. First, it seems hard to deny that modern science makes rapid and significant progress, but it is very difficult to articulate exactly what this progress consists of. There is therefore a conceptual problem of articulating a satisfactory notion of scientific progress. Second, scientists (or at least natural scientists, who are the focus of this dissertation) seem to be able to make progress better than people in other realms of human intellectual endeavor. How does science differ from these other endeavors in such a way as to make better progress? There is therefore a problem of demarcating science from other intellectual fields in a way that is explanatory of the superior progress of the former.[18]

The focus of this section will be the first question, what is scientific progress? I need an answer to the conceptual question in order to assess whether progress occurred in

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[18] This problem is similar to, but distinct from, Popper's concern with demarcating science from pseudo-science. Popper was concerned with what makes a theory scientific or non-scientific. The question here is what features of science enable it to make better progress than other intellectual fields.

a given scientific episode. First, I will describe some general features of progress, summarizing Niiniluoto (2015). As he points out, it is important to distinguish progress from mere change or development. ‘Progress’ is an axiological concept, and therefore has a normative dimension lacking in neutral descriptive concepts like ‘change’ or ‘development.’ Progress is usually conceived as progress towards some goal, where the goal can be either backward-looking or forward-looking, depending on whether it refers to the starting point or to the destination point of an activity. In the philosophy of science, for example, Kuhn famously argued that scientific progress consisted in achieving better and better problem-solving ability, relative to earlier states of science, rather than in coming closer and closer to “one full, objective, true account of nature.”[19] As implied in the previous sentence, progress is also usually conceived as involving some notion of ‘better.’ According to Niiniluoto (2015),

> In general, to say that a step from stage *A* to stage *B* constitutes progress means that *B* is an *improvement* over *A* in some respect, i.e., *B* is *better* than *A* relative to some standards or criteria. In science, it is a normative demand that all contributions to research should yield some cognitive profit, and their success in this respect can be assessed before publication by referees (peer review) and after publication by colleagues. Hence, the theory of scientific progress is not merely a descriptive account of the patterns of developments that science has in fact followed. Rather, it should give a specification of the *values* or *aims* that can be used as the constitutive criteria for “good science.”

In addition to these formal characteristics of progress, a discussion of progress in any concrete case should specify the type of progress at issue. Given the material, social and epistemic dimensions of science, different types of progress can be defined. For example, increased funding of research may constitute economic progress, the rising social status of scientists and their institutions may constitute a kind of professional progress, and the increase of scientific knowledge constitutes cognitive progress. Here, I will focus on cognitive progress, construed as an increase of knowledge. However, it is worth pausing to note that this is not the only conception of cognitive progress that philosophers have entertained. A brief review of some varieties of scientific progress follows.

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[19] Kuhn (1996 [1962]), p. 171.

2.2.1 Varieties of scientific progress

Bird (2007, 2008) identifies three approaches to characterizing scientific progress: (i) an epistemic approach (E), (ii) a semantic approach (S), and (iii) a functional-internalist approach (FI). Bird characterizes each approach as a criterion for judging whether a scientific episode constitutes progress:

> (E) An episode constitutes scientific progress precisely when it shows the accumulation of scientific knowledge.
>
> (S) An episode constitutes scientific progress precisely when it either (a) shows the accumulation of true scientific belief, or (b) shows increasing approximation to true scientific belief.
>
> (FI) An episode shows scientific progress precisely when it achieves a specific goal of science, where that goal is such that its achievement can be determined by scientists at that time (e.g., solving scientific puzzles).

Proponents of (E), like Bird, believe that scientific progress requires the accumulation of justified beliefs, whereas proponents of (S), like Rowbottom (2008), Niiniluoto (1987), or Popper (1979) believe that it requires only the accumulation of true beliefs.

(FI) refers to views such as those of Kuhn (1962/1996) and Laudan (1977) according to which progress is a matter of success in fulfilling a function, that of solving problems or puzzles. For both Kuhn and Laudan, theoretical truth is unattainable. In Kuhn’s case, it is unattainable because evidence for the truth of a theory is always mediated by the theory itself and therefore presupposes its truth, or as Mizrahi & Buckwalter (2014) put it, Kuhn holds theoretical truth to be transcendent. In Laudan’s case, it is unattainable because the pessimistic meta-induction tells us that current successful theories are probably false, just like their discarded predecessors. On the other hand, puzzle solutions are attainable. For Kuhn, a puzzle is solved when a proposed solution is sufficiently similar to a relevant paradigmatic puzzle-solution. For Laudan, a problem phenomenon P is solved by a theory T when P can be deduced from T. T, however, need not be true, according to Laudan. For neither Kuhn nor Laudan does solving a puzzle involve knowledge, at least if knowledge is understood in the classical way as requiring truth. On the other hand, the achievement of a solution can be determined by scientists at the time of solution, by comparison to the paradigm in Kuhn's case or by demonstrating that it follows from an accepted theory in Laudan's.

In an interesting (2013) paper, Moti Mizrahi argued that none of these conceptions of progress fit the way scientists evaluate their own progress. He argues for a broader conception of progress that takes into account 'knowledge-how.' Since I think this broader conception is essential for understanding how the labor process contributes to scientific progress, I will now describe Mizrahi's view and how it illuminates the nature and dynamics of scientific progress.

2.2.2 The broad conception of scientific progress

Basing his argument on evidence from scientists' reflections on progress, Mizrahi argues that scientists employ a broad conception of progress that includes different kinds of knowledge. The four kinds he identifies are:

(EK) *Empirical Knowledge*: Empirical knowledge usually comes in the form of experimental and observational results.

(TK) *Theoretical Knowledge*: Theoretical knowledge usually comes in the form of well-confirmed hypotheses.

(PK) *Practical Knowledge*: Practical knowledge usually comes in the form of both immediate and long-term practical applications.

(MK) *Methodological Knowledge*: Methodological knowledge usually comes in the form of methods and techniques of learning about nature.[20]

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[20] Mizrahi (2013), p. 380.

An example of each kind is provided below, taken from Mizrahi's account of Ivan Pavlov's study of the physiology of digestion:

(EK) *knowing that* stimulation of gastric secretion of acid and pepsin and stimulation of pancreatic secretion of digestive enzymes starts with the anticipation of the ingestion of desirable food.

(TK) *knowing that* stimulation of pancreatic secretion of digestive enzymes is mediated by input to the stomach and pancreas from efferent nerves (i.e., nerves that carry impulses away from the brain or spinal cord) of the vagus; *knowing that* the stimulation of secretion induced by connecting environmental stimuli with appearance of tasty food is a conditioned reflex.

(PK) *knowing how to* treat peptic and duodenal ulcer disease with selective vagotomy (in selective vagotomy, the branches of the vagus nerve to the gall bladder and pancreas are left intact; usually performed to reduce secretion of acid and pepsin by the stomach to cure a peptic ulcer); *knowing how to* treat gastric acid-related disorders with selective muscarinic receptor antagonists.

(MK) *knowing how to* study the anatomy of conscious animals by using surgical techniques, such as the Pavlov gastric pouch.[21]

Mizrahi provides further examples from cosmology and Karl Landsteiner's discovery of blood groups.

Mizrahi draws on Ryle's (1946, 1949) distinction between "'knowing that *p*' (propositional knowledge) and 'knowing how to *p*' (knowledge of skills)."[22] He suggests that empirical knowledge and theoretical knowledge are kinds of "knowledge-that" whereas practical knowledge and methodological knowledge are forms of "knowledge-

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[21] Mizrahi (2013), p. 381.

[22] Though they do not refer to Ryle or his distinction, the view of Baird and Faust (1990) is consonant with Mizrahi's: "according to most philosophers, improved theories account for the progress of scientific knowledge. Technicians, engineers and experimenters […] are able to make devices work with reliability and subtlety when they can say very little true, or approximately true, about how their devices work. Only blind bias would say that such scientists do not *know anything* about nature. Their knowledge consists in the ability to *do* things with nature, not say things about Nature" (p. 147).

how." If this means, however, that no propositional knowledge is involved in PK and MK, then the identification of the latter two with Ryle's conception of know-how seems incorrect because, as I will discuss further in the section on the labor process model (2.3.2), methodological knowledge does involve propositions. For example, knowledge of how to build an instrument involves what Kletzl (2014) calls "engineer theory," which "contains propositional knowledge of how to manufacture an artifact."[23] Conversely, theoretical knowledge has a material form (e.g., equations, symbols, diagrams, etc.) that the theorist needs to know how to physically manipulate. So TK is not purely propositional, either.

It is beyond the scope of this dissertation to enter into the philosophical debates over whether knowledge-how is independent of knowledge-that, or what exactly knowledge-how consists of. Nevertheless, I will resolve the discrepancy noted above by observing that Mizrahi's characterizations and examples of the four kinds of knowledge suggest that, at least in the scientific context, knowledge-how is intrinsically linked to the goal of performing an action (treating ulcers, studying anatomy, etc.), whereas knowledge-that is not. True, the latter may describe actions or presuppose them, as in the appearance of tasty food in the Pavlovian example, but these actions need not be goals. In contrast, know-how necessarily involves an action as a goal.

So if we want to explain the growth of scientific abilities, it is reasonable to group PK and MK together as "know-how," and TK and EK as "knowledge-that," provided that we distinguish the two groups according to the action criterion rather than Ryle's proposition criterion. This grouping is consonant with what seems to me an intuitive and plausible understanding of knowledge-how, that it is a sort of ability, insofar as both abilities and knowledge-how, distinguished according to the action criterion, relate agents to actions.[24]

Abilities are both *explanans* and *explanandum* with respect to the growth of knowledge. The growth of epistemic abilities partially explains the growth of knowledge.

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[23] Kletzl (2014), p. 122. According to Kletzl, engineer theory can also contain pictorial representations of how to achieve the desired goal, e.g., flow charts of how to build an instrument.

[24] For a review of the philosophical debates on how "knowledge-how" and "knowledge-that are related, as well as on the subtleties involved in conceiving of know-how as ability, see Fantl (2008).

But the growth of knowledge results in the growth of abilities, insofar as the knowledge is exercised in the labor process. This circularity is as it should be, for it is one of the ways in which science builds on what it has learned, as noted in the introduction.

Mizrahi summarizes the broad conception thus:

> Granted that PK and MK both count as scientific knowledge, and hence that their accumulation counts as scientific progress, my proposed account of scientific progress is the view that scientific progress is constituted by the accumulation of scientific knowledge, where scientific knowledge consists of each the [*sic*] following: EK, TK, PK, and MK. Each of these counts as scientific knowledge; the accumulation of each advances science.[25]

This conception presents a multi-dimensional picture of scientific progress, where TK, EK, MK and PK each represent a dimension of knowledge within which progress can be made. A given research episode can make progress along any one, or several, of the dimensions. So, progress in TK, or what we might call 'theoretical progress,' is constituted by theoretical advances, for example the accumulation of additional well-confirmed hypotheses or, perhaps, the ruling out of erroneous ones. Progress in EK, or what we might call 'empirical progress,' consists in the accumulation of experimental and observational results. Progress in MK, or what we might call 'methodological progress,' is constituted by the accumulation of methods and techniques for studying nature. And progress in PK, or what we might call 'practical progress,' is achieved by discovering practical applications.

What I would like to add to Mizrahi's conception is the consideration that when we observe how these forms of knowledge are *produced*, this neat picture gets more complicated, for progress in one kind of knowledge cannot be explained separately from progress in the other kinds. Scientific instruments are a good illustration of this interdependence. Scientific instruments are frequently constructed with the aid of all four forms of knowledge, though philosophers have focused mostly on the theoretical knowledge (TK) embodied in them. The goal of construction is usually know-how, especially methodological knowledge (MK), since scientists must know how to study nature in order to acquire TK or EK about it. Successful construction affords scientists the ability to study this or that natural phenomenon.

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[25] Mizrahi (2013), p. 383.

The improvement of scientific instruments constitutes progress in MK.[26] A sketch of the telescope's development will provide an example.[27] For most of human history, the only way to study distant objects was by using the eyes. Reading lenses became available in Italy in the thirteenth century. In the early seventeenth century, spectacle-makers in the Netherlands invented the spyglass. Basing his work on this model, Galileo constructed several research-level telescopes of varying quality but was only able to observe all the phenomena reported in *Siderius Nuncius* with the best of them (Spelda 2017). In the following decades, astronomers inserted cross hairs, facilitating precise alignment of telescopes on objects, and micrometers, to measure small angular distances and diameters. They also developed stable, precise mountings and large arcs with precisely divided and marked scales against which the telescope's alignment could be noted when pointed at celestial objects. In 1757, John Dollond was able to correct chromatic aberration in the refracting telescope by inventing the achromat, a combination of glass lenses that enabled more precise measurements of positions of faint stars. In the late 19th century, the telescope was coupled with photography, and the photographic reflecting telescope became the basic instrument of astronomy. This instrument allowed far larger quantities of data to be collected than with previous telescopes predicated on ocular observation. Space telescopes allow traditional optical telescopes to escape the interference of the Earth's atmosphere. The development of non-optical telescopes has allowed forms of electromagnetic radiation, such as radio, infrared, gamma or X-ray, to be detected that are completely inaccessible to human vision, allowing ever more kinds of objects at ever greater distances to be detected.

Our knowledge of how to study distant objects, make more precise measurements, and handle large amounts of data was massively increased, which means that our MK was massively increased. But progress in MK had consequences for EK and TK as well. The application of that MK in astronomical observations led to a massive increase in observational results, which means that our EK was massively increased. This EK then led

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[26] In section 2.4.2, I will refine this picture by introducing the category of "instrumental knowledge" (IK), which is distinct from MK. Nevertheless, in the scientific context, improvements in instruments usually afford MK as well as IK.

[27] The following information is provided in Hetherington (2003).

to theoretical advances, for example in gravitation theory and big-bang cosmology, and hence TK.

Importantly, all four forms of knowledge were required in the development of the telescope. PK was involved, for instance in glass-making and lens-crafting techniques that were used to make spectacles and spyglasses and which provided the basis for producing research-grade lenses, or in the photographic techniques that were combined with telescopy. MK was involved, for example in the achromat, the invention of which represented knowledge of how to make more precise measurements of faint stars. EK was involved, for example in the results from earlier generations of telescopes that provided calibration for later ones. So, of course, was TK, for example in the optical and electromagnetic theories that were employed in the design of new telescopes.

In short, what this brief example of the telescope suggests is that progress in MK is dependent on progress in the other dimensions of knowledge. The same holds for the other forms of knowledge. The role of theory in the development of technology (PK) is well-known, and leads to the view of the latter as "applied science." Theory isn't always required for PK: for example, spectacles were invented long before there was any good theory of how they corrected vision (indeed, before Kepler they were not considered to correct vision at all but rather to magnify images).[28] But only when Kepler's optics came around was there a convincing explanation of their effects, which probably had consequences for the development of ophthalmology.

Even purely theoretical developments presuppose MK. For example, Einstein's development of the theory of general relativity depended on the availability of the tensor calculus, for the awareness and understanding of which Einstein was indebted to his mathematician friend Marcel Grossmann (Norton 1984). In contrast with the infinitesimal calculus, tensor calculus allows physical equations to be presented in a form that is independent of the choice of coordinates on the space-time manifold. Thus we can say that, having learned the tensor calculus, Einstein knew how to derive equations that he did not know how to derive with the infinitesimal calculus. Moreover, confirmation of the theory

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[28] The explanation of corrective vision offered before and by Kepler is discussed in more detail in section 3.3.

depended on progress in EK, for example the discovery of the precession of the perihelion of Mercury or Eddington's observation of the solar eclipse in 1919.

Thus when we combine the broad conception of progress and knowledge with the labor-process view's focus on the process of production and the means of production, an interdependence of the different forms of knowledge becomes evident that is relegated to the background, if not completely ignored, when we take a more static approach or when we focus primarily on theory. Let us return to question (2) posed in chapter 1:

2. How is it possible for knowledge acquired in the past to be used in on-going or future research?

From the foregoing, it is clear that the answer will depend on how the different forms of knowledge are integrated, and this integration depends crucially on the mediation provided by the means available for scientific work. This idea will be the main theme of chapter 7.

This all sounds very holistic, raising the question to what extent errors in one dimension of knowledge undermine not just the progress made therein, but the progress made in the other dimensions. For example, will a theory that turns out to be false undermine the methodological progress predicated on it?

Though I will not try to settle the question here, I will provide grounds for thinking the holism might not be as threatening as it may seem. The reason is that this conception of knowledge and progress introduces a heterogeneity in scientific knowledge. The evaluative criteria appropriate to TK and EK are different from those appropriate to MK and PK. TK and EK are forms of propositional knowledge, and so are evaluated according to their truth and falsity. In contrast, as forms of knowledge-how MK and PK are evaluated according to the success and failure of actions enabled by them. Moreover, whereas the truth-value of a proposition is binary in standard logic, success and failure admit of continuous degrees.

This heterogeneity can affect the dynamics of progress. For example, astronomers employing Newtonian physics sought to know the trajectories of planets. They had to learn how to compute them, which involved knowing how to solve the equations of Newtonian physics. Newton and his 18th century successors, like Euler, had shown how to come up with such equations in the first place. Then the precession of the perihelion of Mercury was discovered, and Einstein came along and showed that Newtonian physics was false and should be replaced by general relativity.

In this example, the MK involved in computing Newtonian planetary trajectories did not suffer the same fate as the TK. Though the TK was shown not to be knowledge after all, the MK—the abilities to compute the trajectories, to solve the equations, or to represent Newton's laws using the calculus—remained. Moreover, this MK was not made redundant by the downfall of the theory, but continues to be used in applications involving static, weak gravitational fields.[29] Thus, the PK based on classical physics was partially preserved.

This example suggests that the heterogeneity between the different forms of knowledge may block holistic upheaval. Much more would have to be said to establish this point; I leave this as a question for future research.[30]

In any case, the holism of the broad conception of progress seems appropriate for capturing the dynamics of scientific change. This conception is therefore, in my view, descriptively accurate. One may object that even if it is, it does not follow that from a *normative* point of view, we are obliged to accept this conception. One may dispute

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[29] One might object that the applicability of the MK is due to the fact the Newton's theory of gravity *holds* in the static, weak-field limit of Einstein's theory. If it did not, the MK would be inapplicable. Therefore, the MK depends on the approximate truth of Newton's theory. In reply, I will point out that the claim about holding in the limit is, of course, true in the sense that the relativistic equations *converge* on the classical ones in that limit, but whether the convergence preserves the *truth* or approximate truth of Newton's theory seems like a complicated question that I cannot address here. From a conceptual point of view, in any case, the world according to general relativity is a very different place than according to classical physics. Moreover, this objection would at best undermine the *relevance* of the MK, not whether we still know how to do those things.

[30] Douglas (2014), p. 62 makes a similar point when she observes that "[t]heories or paradigms may come and go, but the ability to intervene in the world, or at least predict it, has staying power. We can think of explanatory frameworks and understandings lost in paradigm change (e.g. an intuitive grasp of what light is, a sense of place in the universe, a clear grasp of what makes something a species), but we are hard pressed to think of a predictive or manipulative capacity that has been lost." I note that she is here identifying a form of progress that consists of an accumulation of abilities, which is consonant with the progress in knowledge-how that has been under discussion in section 2.2.2.

whether some of these kinds of scientific change constitute progress, or even whether progress should be conceived in terms of an accumulation of knowledge rather than, say, truths. In an example I will discuss in detail in chapters 4-6, the adoption of powerful spectrometric techniques by chemists in the 20th century caused progress in the theoretical knowledge of molecular structures, but virtually halted progress in the empirical knowledge of the reactions of natural products. Now, from a normative point of view it might be the case (for example) that in the end we will want to exclude empirical knowledge from the definition of progress (though I doubt it). For the purpose of exploring and applying the labor process model in particular case studies, however, I think it is more prudent not to foreclose on the possibilities for progress offered by the situations we will be examining.

Returning to (S), (E) and (FI), Mizrahi (2013) argues that neither (S) nor (E) satisfies the broad conception of progress. Methodological know-how and practical applications do not require truth. (E) is broader than (S), since reliable methods and justification are necessary for the accumulation of knowledge. On the other hand, to the extent that knowledge is understood as a kind of belief, (E) would seem to exclude know-how, and hence MK and PK, assuming these are not reducible to propositional knowledge.

On the Laudan and Kuhn versions of (FI), it is clear that the broad conception does not fit since these versions exclude the accumulation of TK. Perhaps their views could accommodate accumulation of knowledge-how, say in the form of an ever-greater ability to solve problems. But an ability is not itself a solution to a problem or puzzle, though it can be useful for solving a problem or puzzle. More importantly, not all scientific knowledge is knowledge of the solution to a puzzle.[31] Much EK is like this. For example, accumulating and cataloguing observations, say anatomical data or maps of the night sky, may involve problem-solving and may help to solve problems, but a catalogue of observations does not seem like the sort of thing that is the solution to a problem or puzzle. Serendipitous discoveries result in progress, but they are unsought and therefore not solutions to a pre-existing puzzle.[32]

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[31] As pointed out by Bird (2007), p. 68.

[32] Van Andel (1994) defines serendipity as “the art of making unsought findings.”

In summary, the conception of scientific progress I will be working with in this dissertation is as follows. Following Mizrahi, knowledge is conceived as consisting of four forms, theoretical, empirical, methodological and practical. Progress consists in the accumulation of knowledge, where the knowledge can be of any one of these forms. As I have argued above, it follows from this conception that progress is made holistically, with achievements in the different dimensions of knowledge being played off against each other. The holistic dynamics are tempered, however, by the heterogeneity of the components with respect to the evaluative criteria used to assess progress in the different dimensions of knowledge. This imparts some measure of stability to the overall accumulation of knowledge.

Thus far in this chapter, we have been largely concerned with the topic of scientific progress. In the following section, I turn to the other side of the coin, the nature of the activity that achieves that progress.

2.3 Three models of science

2.3.1 The intellectualist model

In general, what I am calling "intellectualist" views tend to reduce the content of science to ideas and logical relations between ideas, the history of science to the history of ideas,[33] and the method of science to cognitive operations. Textbooks frequently adopt an intellectualist stance. For example, Neil Campbell's classic textbook on biology states that "[w]hat really advances science ... is a new idea."[34] Samir Okasha's (2002) introduction to

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[33] Laudan's (1977) book on *Progress and its Problems* states that the history of science "at a first approximation, can be regarded as the chronologically ordered class of beliefs of former scientists" (158). Later, he writes that "[i]f the philosopher would learn something from history, he must make himself a servant to it—at least to the extent of dealing with actual cases and actual beliefs" (170).

[34] Campbell (1993), p. 18.

the philosophy of science furnishes an example. The first chapter, “What is science?” opens by asking what common feature is possessed by all the activities uncontroversially recognized as science—e.g., physics, chemistry, and biology—and that distinguishes them from non-scientific intellectual activities like art or theology. He considers several candidate common features throughout the chapter, for example: the attempt to understand, explain and predict our world; the method of inquiry; or the construction and nature of scientific theories. Okasha then concludes by casting doubt on the very project of seeking “some common feature shared by all the things we call ‘science,’ and not shared by anything else.” He suggests that the sciences are related to each in the same way as games are related to each other according to Wittgenstein: though there is a loose cluster of features most of which are possessed by most sciences, any particular science may lack any of the features in the cluster and still be a science. What is not in doubt, however, is that aspect of science that is relevant to discussing the question of what science is: the question is answered by appealing to the history of modern scientific ideas, starting with Copernicus’ heliocentric model of the universe, through Darwin’s theory of evolution, and ending with contemporary cognitive science based on the idea that the human mind is similar to a computer. The chapter on “Scientific change and scientific revolutions” is similarly idea-centric: though “scientific change” could involve myriad aspects of science, the first sentence clarifies what is at issue: “Scientific ideas change fast.”[35]

Carl Hempel’s 1966 textbook, *Philosophy of Natural Science*, has been credited with defining the philosophy of the natural sciences for generations of students.[36] The table of contents indicates that the book is concerned with logical relations of support between evidence and hypotheses and with the nature of scientific theories:

> 1—Scope and Aim of This Book
> 2—Scientific Inquiry: Invention and Test
> 3—The Test of a Hypothesis: Its Logic and Its Force
> 4—Criteria of Confirmation and Acceptability
> 5—Laws and Their Role in Scientific Explanation

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[35] Okasha (2002), p. 77.

[36] Salmon (1992), p. 3.

6—Theories and Theoretical Explanation
7—Concept Formation
8—Theoretical Reduction

The 1992 textbook *Introduction to the Philosophy of Science*, authored by members of the History and Philosophy of Science department at the University of Pittsburgh, is framed in part as a response to Hempel's failure to take account of the bearing of history of science on the philosophy of science. Despite more emphasis on particular sciences and incorporating a chapter on scientific change, however, the book shares Hempel's theory-centric orientation, as indicated by the table of contents:

PART ONE: GENERAL TOPICS IN THE PHILOSOPHY OF SCIENCE
1—SCIENTIFIC EXPLANATION
2—THE CONFIRMATION OF SCIENTIFIC HYPOTHESES
3—REALISM AND THE NATURE OF THEORIES
4—SCIENTIFIC CHANGE: PERSPECTIVES AND PROPOSALS

PART TWO: PHILOSOPHY OF THE PHYSICAL SCIENCES
5—PHILOSOPHY OF SPACE AND TIME
6—DETERMINISM IN THE PHYSICAL SCIENCES

Six more chapters on particular sciences follow, focusing largely on theoretical issues within each science.

There is also intellectualism about the scientific method. For example, Steven Gimbel begins his (2011) textbook *Exploring the Scientific Method* by asking "[w]hat actually is the scientific method?" He then proposes to answer the question by first answering three interrelated question about science:

(1) WHAT IS A SCIENTIFIC THEORY?
⋮
(2) HOW DO SCIENTISTS COME UP WITH THEIR THEORIES?

⋮

(3) WHAT MAKES SOME THEORIES BETTER THAN OTHERS?[37]

Two of the three questions are about theories. The second question, it turns out, largely concerns the cognitive operations involved in coming up with a theory:

> Is it [the theory] the result of a strict logic applied to undeniable first truths or to observations made in the lab or in the world around us? Is it a matter of creativity and insight where any old idea can be introduced as a scientific hypothesis no matter how outlandish and bizarre? What role do politics and the biases of the times play?[38]

How scientists come up with their theories (laws, to be exact) is also the primary concern of Langley, Simon, Bradshaw & Zytkows' 1987 book *Scientific Discovery: Computational Explorations of the Creative Process.* Their goal is to simulate the human thought processes involved in the discovery of scientific laws by developing artificial-intelligence programs to do the same. Key to their approach is "the hypothesis that scientific discovery is a species of normal problem-solving." One of the virtues of this hypothesis, according to the authors, is that "[i]t preserves a framework in which all forms of serious human thought—in science, in the arts, in the professions, in school, in personal life—may reveal their commonalities."[39] A running example of problem-solving in the first chapter is the missionaries-and-cannibals puzzle:

> Three missionaries and three cannibals are trying to cross a river in a boat that holds no more than two persons. Everyone knows how to row. Missionaries may never be left on either bank with a larger number of cannibals, because the missionaries will then be eaten. How can the party cross the river?

By reducing scientific discovery to problem-solving, these authors eliminate important differences between the former and logical puzzles like the above.[40] The role of

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[37] Gimbel (2011), p. ix-x.

[38] Gimbel (2011), p. ix.

[39] Langley et al. (1987), p. 6.

[40] Jutta Schickore 's (2018) article on scientific discovery indicates that the philosophical study of discovery tends to reduce the latter to a reasoning process: "There are three main lines of response to the disciplinary distinction tied to the context distinction [between discovery and justification]. Each of these lines of response opens up a philosophical perspective on discovery ... Discovery is conceived as an analyzable reasoning process, not just as a creative leap by which novel ideas spring

data production in discovery, for example, or the tacit knowledge that may be presupposed in the judgment that something new has been discovered, have no place here. This last assertion is not intended as a criticism of Langley et al.'s approach, but merely to point out that such an approach leaves out important features of scientific practice. The point generalizes: though the intellectualist model is appropriate for addressing many important questions about science, it leaves out other important features, and it overreaches to the extent that it is taken to grasp the "essence" of science.

2.3.2 The labor process model

The model of science as a labor process conceptualizes science as a synthesis of intellectual and manual labor. It posits a closer relationship between scientific work and ordinary material labor than views of science that understand it as a primarily intellectual and creative activity. Skills, know-how and material practices are important for understanding how science works, what kinds of change are possible, and what kinds of change are significant. The emphasis is on the performance of science rather than its results. Scientific change is viewed as resulting from changes in the labor process, which again has both intellectual and manual (or material) components.

To some extent, this last point can be explained in terms of the four forms of knowledge described in section 2.2.2. Scientists draw on TK and EK to do their work, but they also draw on MK and even PK. Chemists, for example, draw on their knowledge of molecular composition and structure, but they also need to know how to analyze complex organic substances in the lab. The changes in chemistry described in chapters 4, 5 and 6 resulted from changes in the TK and EK used by chemists to determine molecular structure and composition, for example the introduction of quantum theory or the use of spectroscopic data. But it also resulted from changes in the MK and PK employed, for

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into being fully formed ... All of these responses can be described as theories of problem solving, whose ultimate goal is to make the generation of new ideas and theories more efficient."

example the switch from wet-chemical to physical methods of data production or the application of new materials in the equipment (e.g., superconductors rather than glass).

I wrote “to some extent,” because the four forms of knowledge each have intellectual and material aspects. A theory isn’t just an abstract idea, but has a material form (e.g., equations, diagrams, etc.). A method isn’t just a set of instruments and manual operations, but often involves principles and rules describing how to do something, as well as other kinds of background propositional knowledge.

The same holds for ordinary manual labor. Carpentering, for example, is not just about manipulating tools on wood, but involves background knowledge such as the different kinds of wood that are appropriate for a given purpose. Thus carpentering involves MK, knowing how to wield the tools of the craft, as well as EK, knowledge of kinds of wood and their properties.[41] Conversely, intellectual work relies on propositions and equations, but also on the ability to manipulate material means of representation, for example symbol systems.[42]

Thus, the distinction between TK and EK on the one hand, and MK and EK on the other, is not the same as the distinction between intellectual and manual labor. This latter distinction is not as straightforward as it might seem either. One might try to distinguish between them by focusing on the organs that are used in the labor. What I am calling ‘intellectual’ labor is sometimes called ‘mental’ labor, and might be identified with work carried out primarily in the brain. The sociologist Harry Braverman defined it thus.[43] Examples of mental labor include thinking, discussing, reading, writing, calculating, arguing, describing, and explaining. In ‘manual’ labor, on the other hand, labor is carried

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[41] Chazal (2002), pp. 125-132 (« Qu’est-ce que l’habileté manuelle ? ») contains an excellent discussion of the kinds of knowledge involved in manual work: « *Car les gestes que l’habileté manuelle suppose ne sont jamais nus. Nous voulons dire qu’ils s’accompagnent d’une mise en œuvre de savoirs, de capacités d’abstraction, de capacités de se projeter dans l’espace et le temps. En effet, l’adaptation des gestes demande de toute façon une connaissance des matériaux, une connaissance minimum de la géométrie, une connaissance de l’algorithme qu’il s’agit d’intérioriser. Toute technique se double d’une technologie aussi fruste soit-elle.* »

[42] Damerow (1996).

[43] Braverman (1998 [1974]), p. 79.

out primarily with other parts of the body, especially the limbs and the torso. Examples of manual activities are carrying, hammering, sawing, and walking. Tentatively and informally, perhaps one could put the distinction as follows: in intellectual work, the bulk of the effort is thinking, whereas in manual work, the bulk of the effort is physical (involving bodily movements). There is obvious overlap between the two, since intellectual work always requires some physical activities and vice-versa. Thinking is often based on the manipulation of symbols, for example, and even simple physical activities require purposiveness, and hence intention and thought, if they are to be labor.

On the other hand, notions like ‘primarily’ and ‘bulk’ are vague, and suggest a quantitative distinction that is probably hard to make precise. How much thinking is necessary before an activity counts as intellectual? Writing is an undeniably physical activity, as the bodily aches and occupational injuries associated with writing-intensive jobs attest, yet a philosopher writing a book is certainly performing intellectual labor. Moreover, conscious physical activities always include a mental component, regardless of how “manual” they are. Even simple manual activities like shovelling or hammering require attention, purpose and dexterity (and hence, some thought about how to approach the task).

Perhaps one can distinguish between intellectual and manual labor on functional grounds. Manual labor is labor in which cognition is aimed at manipulating the body in order to achieve some productive, physical effect, tool use for example. Intellectual labor is labor in which cognition is aimed at manipulating ideas. Such a view is expressed by Damerow & Lefèvre (1981):

> Mental labor is an immaterial activity dealing with a mental object and employing material tools. The object appears only as the meaning of signs, symbols, or other material representations, and the mental activity is the transformation of such meanings by objectively manipulating their material representations. (397)

As the reference to material tools and representations indicates, body manipulation and tool use enter into intellectual labor, but only as secondary aims, conditional on the first. Conversely, ideas enter into manual labor, but subordinated to the primary aim of achieving a productive, physical effect.

Whatever the case may be, it is clear that the distinction between the kinds of knowledge and that between the kinds of labor are not identical. Nevertheless, focusing on manual labor tends to reveal the roles of MK and PK better than focusing on intellectual labor. By concentrating on the latter, one can be all too easily be absorbed by the products of the work, TK and EK, and lose sight of the various abilities that were brought to bear in the work itself. In manual labor, on the other hand, the roles of action, and hence abilities and MK and PK, are evident.

Thus, conceptualizing science as a synthesis of intellectual and manual labor is an intentionally holistic way of conceptualizing scientific work that can provide insight into the holistic nature of scientific knowledge and progress. As should be obvious from the foregoing, this view does not exclude ideas from playing an important role in scientific change, but it does emphasize the constraints on what can be achieved by means of purely intellectual activity.

In this section, I will do two things. First, I will discuss what is involved in the labor process model of science in more detail than I have up to now. Then I will review three bodies of literature that can be viewed as falling under the rubric of science as labor. First, I focus on the views of the *Radical Science Journal* (*RSJ*) collective, which for a time in the late 1970s and early 1980s explicitly applied the labor process model to the study of science and technology. Second, I will discuss the theory of Edgar Zilsel, who explained the Scientific Revolution in terms of the merging of intellectual and manual labor. Third, I examine a better known and longer-lived tradition that emerged in roughly the same period as the *RSJ*, according to which science is a practice. This tradition focuses on the performative aspects of scientific work and on the processes by which individual performances give rise to stability at the community level.

2.3.2.1 Excursus on the labor process

If science is a labor process, then it is not a fundamentally different kind of activity from other forms of labor. Consequently, a reasonable starting-point for the analysis of science as labor is an understanding of the labor process in general. All authors that I am aware of who have self-consciously applied this category to science have started from Marx's discussion of the labor process in chapter 7 of *Capital*, and I will follow them in this regard. I will supplement Marx's discussion with Harry Braverman's clarifications in *Labor and Monopoly Capital* (1974), a work that sparked renewed interest in the evolution of labor processes under capitalism and directly inspired the *Radical Science Journal* collective.

According to Marx, labor is

> a process between man and nature, a process by which man, through his own actions, mediates, regulates and controls the metabolism between himself and nature. He sets in motion the natural forces which belong to his own body, his arms, legs, head and hands, in order to appropriate the materials of nature in a form adapted to his own needs.[44]

He identifies three "simple elements" of the labor process: "(1) purposeful activity (*zweckmässige Tätigkeit*), that is work itself, (2) the object on which that work is performed, and (3) the instruments of that work."[45] This analysis subdivides the process into three basic categories: purposeful activity, object of labor, instrument of labor. Which category a thing falls under is determined by its specific function in the labor process. As its function changes, so does the category to which it belongs. The same thing may function as raw material or instrument of labor, as for example cattle may serve as raw material for the production of food or as instruments for the production of manure.[46] Similarly, the category of purposeful activity need not be exclusively attached to individual humans. Work may be carried out by teams of workers, for instance, or by machines.

Marx characterizes an instrument of labor as "a thing, or a complex of things, which the worker interposes between himself and the object of his labour and which serves as a conductor, directing his activity onto that object." In doing so, the worker avails himself of "the mechanical, physical and chemical properties of some substances in order to set them to work on other substances as instruments of his power, and in accordance with his purposes." Though it might seem as if the worker's purposeful activity was restricted to his own actions, not the actions of the substances on each other, Marx views the entire process as subordinated to the worker's aims, a point he makes by way of quoting Hegel's *Logic* on the "cunning of reason":

> Reason is as cunning as it is powerful. Cunning may be said to lie in the intermediative action which, while it permits the objects to follow their own bent and act upon another till they

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[44] Marx (1976), p. 283.

[45] Marx (1976), p. 284. For the original, see Marx (1959), p. 193.

[46] Marx (1976), pp. 288-289.

> waste away, and does not itself directly interfere in the process, is nevertheless only working out its own aims.[47]

This point is important for understanding the role of *designers* in the labor process, for their purposes may be realized in the labor process even though they are absent.

Though in this chapter Marx writes as if labor were the activity of individual humans, he later explains that it is not necessarily so. The fundamental reason is that labor requires different functions the fulfillment of which need not all be carried out by the same individual:

> In so far as the labour process is purely individual, the same worker unites in himself all the functions that later on become separated. When an individual appropriates natural objects for his own livelihood, he alone supervises his own activity. Later on he is supervised by others. The solitary man cannot operate upon nature without calling his own muscles into play under the control of his own brain. Just as head and hand belong together in the system of nature, so in the labour process mental and physical labour are united. Later on they become separate; and this separation develops into a hostile antagonism. The product is transformed from the direct product of the individual producer into a social product, the joint product of a collective labourer, i.e., a combination of workers, each of whom stands at a different distance from the actual manipulation of the object of labour.[48]

Though initially all the functions required for work may be united in a single individual, later on they can be divided among different individuals. As I will discuss in greater detail below, the idea that some of these functions can also be delegated to instruments will play an important role in this dissertation. For the time-being, it is worth noting that two kinds of division are mentioned in this passage. There is the basic division of functions, whatever their nature, among different workers (the “collective labourer”). Then there is the separation of mental and physical labor as a result of which functions of conception, design and supervision may become the purview of different workers than those who execute the physical operations necessary to produce the product.

It may be worth pausing here to ask whether human labor differs significantly from the activities of other living beings. As Braverman points out, “the human species shares with others the activity of acting upon nature in a manner which changes its forms to make

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[47] Quoted in Marx (1976), p. 285.

[48] Marx (1976), p. 643.

them more suitable for its needs."[49] So is there any difference between humans and other species? In a famous passage, Marx answers the question by suggesting a distinction between instinct-guided labor and conception-guided labor:

> We are not dealing here with those first instinctive forms of labour which remain on the animal level … We presuppose labour in a form in which it is an exclusively human characteristic. A spider conducts operations which resemble those of the weaver, and a bee would put many a human architect to shame by the construction of its honeycomb cells. But what distinguishes the worst architect from the best of bees is that the architect builds the cell in his mind before he constructs it in wax. At the end of every labour process, a result emerges which had already been conceived by the worker at the beginning, hence already existed ideally. Man not only effects a change of form in the materials of nature; he also realizes his own purpose in those materials. And this is a purpose he is conscious of, it determines the mode of his activity with the rigidity of a law, and he must subordinate his will to it.[50]

Following Braverman, one might say that the "directing mechanism" of work in animals is instinct, whereas the corresponding mechanism in humans is the "power of conceptual thought."[51] Due to their exceptionally large brain, humans have a capacity for doing work that is "well-conceptualized in advance and independent of the guidance of instinct."[52] Though there are cases of animals learning and using tools, the difference between those rudimentary abilities and the human capacities for learning, conception and tool-use are great enough to warrant the distinction between labor as an activity of living beings in general and specifically human labor.

To conclude this section, I will emphasize three features of labor that will be especially relevant for the analyses of the following chapters:

1. The labor process presupposes a system of functional relations between agents on the one hand and instruments and objects of labor on the other.
2. Labor essentially involves the transformation of existing materials by agents, who carry out the transformation by means of existing means of production, to produce new products.

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[49] Braverman (1974),

[50] Marx (1976), p. 284.

[51] Braverman (1974), p. 32. Italicized in the original.

[52] Braverman (1974), p. 33.

3. This transformation is carried out in order to obtain a product that is not readily available in the environment.
4. The instruments are hybrid, in that they have both social and material properties.

(1), of course, reflects Marx's tripartite analysis of the labor process. It also reflects the fact that any labor process involves a division of functions among the agents and between the agents and artifacts.

(2) differentiates labor from other practices. There must be materials and means of production existing outside the agents, and a product likewise at the end of the process. This differentiates labor from e.g., recreational activities, which need leave behind no product,[53] and practices involving largely human-human interactions, like negotiations and more basic communicative interactions like the expression of thoughts and feelings. The materials, instruments and products need not be material objects. For example, Ravetz (1971) holds that scientists investigate the properties of classes of intellectually constructed things and events. A chemical substance, for example, is a class defined intensionally by certain properties of its members.[54] The goal of the investigation is the establishment of new properties of these classes. If the investigation is successful, the object itself is transformed, precisely because the class is defined in terms of its properties. Althusser (1963, 1965) likewise maintained that scientists use existing theories to transform existing concepts into new, scientific concepts. That said, scientific work also depends on straightforwardly material transformation, as when a sample of a chemical substance is produced synthetically or some physical phenomenon is produced in the laboratory.

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[53] One might object that there are some kinds of work, like teaching or music-playing, that leave behind no product. In reply, I would claim that the notion of "product" should be understood liberally, to include not just physical objects but useful effects in general, which includes things like skills and experiences. So understood, it makes sense to view teaching as productive, since it produces bearers of skills and knowledge. Music-playing produces the experience of listening to music. It should also be noted that the highlighted features are not intended to be necessary and sufficient conditions for what counts as 'labor.' So, for example, in some contexts we might want to consider music-playing as work and in others as recreation.

[54] Ravetz (1971), p. 111.

(3), the necessity of transformation, distinguishes labor from mere use of the natural endowment: "to seize upon the materials of nature ready-made is not work; work is an activity that alters these materials from their natural state to improve their usefulness."[55] The necessity of transformation *in science* arises from the fact that new knowledge of the sort scientists care about is not readily available in the social and natural environment but must be produced through alteration of the existing stock of knowledge and means of production. The production of a phenomenon in the laboratory that cannot be observed in its "pure" state in nature is an example. So might elaborating a theory and producing evidence for it. On the other hand, certain kinds of work, like attending a conference to report on research results, would not count as labor according to (3), unless the interactions at the conference were somehow important for the result itself. If they were not, then it might be best to categorize them as what the sociologist Elihu Gerson calls 'metawork,' work devoted to organizing work that is to be done[56] (or, the conference example suggests, disclosing work that has already been done).

Finally, (4) reminds us of nature's essential role in the labor process. For example, if the instrument is a hammer and the objects are loose nails and pieces of wood, then what the agent does in the process is determined by his or her own social and material properties—e.g., his physical strength, his assigned task, etc.—as well as the social and material properties of the hammer, nails, and wood—that hammers are, in his society, used for knocking together pieces of wood, that the nail is harder than the wood, etc. Since both social and material properties are subject to change, the labor process also has a historical character.

This historical character makes progress possible. So does the fact that the process is purpose-driven, a point driven home by Marx's analogy of the bee and the architect. Since labor is purposive, progress can be assessed relative to the purpose(s) of the producers, or relative to alternative purposive employments of the means of production. But those purposes themselves must be formulated in terms of the means available (a point to which I will return in section 2.3.2.3) which gives progress a certain path-dependence. I

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[55] Braverman (1974), p. 31.

[56] Gerson (2013).

use the latter term in the sense of Peacock (2009), p. 107, according to which it means that evolution is conceived as a path: “As with any path, one’s position on it is determined by the course that path has taken, and one cannot simply ‘undo’ the path one has trodden nor efface one’s current position on it.” In the case of labor, for example, in general one cannot switch methods of production at will—one is to some extent stuck with the instruments that the past history of development has bequeathed. This dependence of later developments on earlier ones imparts temporal directedness to the overall developmental process. Thus the historical character of the labor process both makes progress possible, but also constrains its direction.

Moreover, since the labor process involves both conception and execution, progress in the development of labor depends on both intellectual and material factors, some of which go beyond mere “purposes.” Ideology, for example, plays an important role in shaping worker’s attitudes towards their work, and hence how they produce, what they produce, and for whom they produce it. This fact will be important in chapter 3. Furthermore, the labor process involves not just a relation of production between workers, but also between the workers and their instruments. In chapters 4 and 5, it will be demonstrated on a historical case that this relationship can play a critical role in scientific progress.

Finally, as a unit of analysis the labor process gives us conceptual elements for explaining the abilities displayed by the agents in production. Means of labor, object of labor, purposes, division of labor and ideology are all so many conditions of action that contribute to explaining the agents’ abilities.

With this excursus complete, I now turn to the literature review, starting with the *Radical Science Journal* collective.

2.3.2.2 The Radical Science Journal collective

The key insight of the RSJ collective is that particular labor processes are not neutral with respect to purposes and values:

> Capitalist production and reproduction presents its own form of social organization as due to objective necessity, be it natural or technical. The subversive potential of labour process analyses

> lies in undermining that apparent necessity by revealing the social relations built into the concepts, techniques and technologies that mediate human labour.[57]
>
> The concept of the labor process and its sub-categories offer a framework that encourages us at every step to grasp the human purposes at the heart of science, technology, medicine, or any practice. … A labor process perspective offers the possibility of recovering the intentions already embodied in facts, theories and artifacts.[58]

Based on the contributions collected in the two volumes put out by the collective on *Science, Technology and the Labour Process* in 1981 and 1985, the participants appear to have been mainly interested in how capitalist purposes and values structured the ways in which science and technology are applied in ordinary material production. A few of the contributions, however, apply the labor process model to "pure" science. For example, Yoxen (1981) develops a labor-process account of the history of molecular biology:

> … I want to draw out some more implications of the labour process perspective on molecular biology, by considering the significance of the integration of concepts like 'program,' 'message' and 'code' into a reductionist biological framework. What lies behind the idea of a genetic program? Again I should say stress [*sic*] that I am not saying that the idea that organisms are programmed is a biased or deformed account of reality, a misconception occasioned by capitalism. What I am asking is why it should be that we attempt to apprehend nature as programmed? Why do we frame and analyse the manifold of nature in this way? What is it that leads us to this way of formulating questions?[59]

As these quotations indicate, the collective was concerned with identifying the contingent aspects of scientific work and thought, in particular those aspects that might be caused by the social relations within which scientific work takes place. Yoxen, for example, in answering the questions at the end of the quoted passage emphasizes the interdependence of scientific concepts and ideas, forms of scientific organization, and economic forces.

The perspective of the *RSJ* collective may be related to the "cunning of reason" Marx sees at work in the instrument of labor. Does the fact, if it is a fact, that artifacts embody intentions entail that human agency is still in some sense present, even in the absence of direct intervention by humans? Moreover, if instruments embody or determine

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[57] Levidow & Young (1985), p. 4.

[58] Young (1985), p. 208.

[59] Yoxen (1985), p. 102.

certain goals and not others, then the choice of an instrument to achieve independently given goals may not be not neutral with respect to them, and may in practice encourage goal revision. This topic will come up in the chapters on the Instrumental Revolution.

Another contribution in *Science, Technology and the Labour Process* that will be relevant in the third chapter is Nathan Rosenberg's "Marx as a Student of Technology." The author asks the question, "What are the characteristics of technologies which make it possible to apply scientific knowledge to the productive sphere?"[60] Rosenberg points out that not all technologies permit the application of scientific knowledge to production to the same extent. On his reading of Marx's account of the Industrial Revolution, the decisive step in the latter was not the discovery of new power sources, as is commonly thought, but rather

> the development of a machine technology which was not heavily dependent upon human skills or volitions, where the productive process was broken down into a series of separately analyzable steps. The historic importance of the manufacturing system was that it had provided just such a breakdown. The historic importance of Modern Industry was that it incorporated these separate steps into machine processes to which scientific knowledge and principles could now be routinely applied. … When this stage has been reached, Marx argues, technology become, for the first time, capable of indefinite improvement.[61]

Rosenberg's answer to his question involves a specific division of functions between humans and machines, one involving the marginalization of human skills and volitions in the labor process.[62] In chapter 4, I will ask a question analogous to Rosenberg's, but with respect to science: what are the characteristics of scientific instrumentation that make it possible to apply scientific knowledge to science itself? Moreover, the idea that

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[60] Rosenberg (1981), p. 15.

[61] Rosenberg (1981), p. 17.

[62] Though coming from a different intellectual tradition, the philosopher Etienne Balibar had earlier made a similar point in the structural Marxist classic *Reading Capital* (2009 [1965]):

> The machine-tool makes the organization of production completely independent of the characteristics of human labour-power … This separation makes possible the constitution of a completely different type of unity, *the unity of the means of labour and the object of labour*. … This unity is expressed in the emergence of technology, i.e., the application of the natural sciences to the techniques of production. But this application is only possible on the existing basis provided by the objective unity of the *means of production* (means and object of labour) in the labour process. (p. 268)

instruments tend to allow an indefinite sequence of improvements over human abilities will be a *leitmotif* in this dissertation, as explained in section 2.4.2 below.

2.3.2.3 Edgar Zilsel and the merging of intellectual and manual labor

In his classic 1942 article on "The Sociological Roots of Science," Zilsel asks why modern science emerged when and where it did. Why the modern period and not antiquity? Why Europe and not China? Though elements of modern science could be found in antiquity and non-European cultures, modern science emerged only under the conditions of early capitalism. Zilsel asks, what sociological process made the emergence possible?

His answer involves four background conditions, all related to the rise of capitalism. First, the centers of culture shifted from manors and monasteries to towns. Second, machines were increasingly used for the production of goods and for warfare. Third, competition undermined the traditionalism of the Middle Ages in favor of an individualistic spirit. Fourth, feudal traditions and customs were replaced by the more rational methods of early capitalism.

Against this background, Zilsel identifies three strata of intellectual activity in the period from 1300 to 1600: the universities, humanism, and labor. The universities were dominated by scholasticism. Though rationally trained, university scholars endeavored to explain "the ends and meanings of the phenomena" rather than investigate causes or discover physical laws. They remained largely aloof of technological developments. The humanists were a class of literati that appeared in Italian cities in the 14th century. They depended on the upper classes for patronage and employment. Though they developed the methods of scientific philology, the humanists, like the scholastics, neglected causal research, physical laws and quantitative investigation.

A common and central characteristic of the university scholars and humanist literati was that they despised manual labor. They distinguished between the liberal and mechanical arts, and considered only the former worthy of well-bred men.

Meanwhile, various types of manual worker emerged. Economic competition stimulated them to inventions. Thus the artisans, the mariners, the shipbuilders, carpenters, foundrymen and miners pioneered empirical observation, experimentation and inquiry into causes. The 15th century saw the rise of "superior artisans" in the arts, engineering, surgery, musical instrument-making, the production of measuring-instruments, surveying and navigation. These superior artisans included famous Renaissance figures like da Vinci, Cellini and Dürer. Their measuring-instruments were forerunners of modern physical instrumentation, and their quantitative rules of thumb anticipated physical laws. What the superior artisans lacked and the scholars and literati had, however, was a "methodological training of intellect," which involved the ability to carry out research in a systematic fashion.

The key break-through for the emergence of modern science was the collapse of the social barrier between the academically trained scholars and the superior craftsmen, which occurred when the former adopted the methods of the latter. This happened around 1600 with the work of Gilbert, Galileo and Bacon.[63]

It is worth noting that for Zilsel, science is a kind of labor, but one that involves an essential intellectual and theoretical component. In effect, Zilsel offers a genealogy of the modern scientific method: The academically trained scholars contributed one component in the form of logical training, learning, and an interest in theory, whereas the manual workers contributed an interest in causation, experimentation, measurement, quantitative rules of operation, disregard of authority, and cooperation. On his view, the scientific method consists of two skill sets: training in systematic and logical inquiry on the one hand, skills for the manipulation and control of the natural world on the other. Technology is important in his story, because it is through the development of technology that the latter skill set was acquired over generations. The development of technology was accompanied

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[63] Rossi (1970) concurs that there was a change in attitudes towards labor among philosophers in the Renaissance and early modern period: "A new view of labor, of the function of technical knowledge, and of the significance of artificial processes through which nature was altered and transformed clearly makes its way into the work of artists and experimentalists of the fifteenth century and into the treatises of engineers and technicians of the sixteenth century. This trend is discernible even at the level of philosophy: in those social groups which were much taken up with problems of this type, there emerged an appraisal of the arts that was quite different from the one which had traditionally prevailed. It was now argued that some of the methods employed by technicians and artisans to modify and alter nature might also be useful for acquiring a real knowledge of natural reality" (p. x).

by the development of a class of superior artisans. Once these skill sets were combined, the stage was set for the successes of modern science.

The quote from Zilsel in the introduction, according to which modern science was born when the experimental method was adopted by scholars, might appear to equate all science with experimental science. Zilsel himself was aware of this problem, and in an unpublished manuscript he acknowledged that the astronomy of the solar system was highly successful without any experiment.[64] He explains this success as a matter of luck, that we happen to live in a solar system where "superimposed effects belong to very different orders of magnitude and therefore can be separated comparatively easily." Were it not for this "extraordinary fact," "Copernicus, Kepler, and Newton would not have achieved much." In my view, however, Zilsel does not go far enough with his own mode of explanation. As the examples from cosmology and chaos theory discussed in the appendix suggest, and as my discussion of Kepler's attempt to improve the quality of ocular astronomical observation in chapter 2 will reinforce, the development of technology and the correlative skills have been important in non-experimental sciences as well.

A view conceptually related to Zilsel's is Jerome Ravetz's (1971) depiction of science as a kind of craft. He describes scientific work as having a "peculiar" character, "as a special sort of craft work operating on intellectually constructed objects."[65] This view leads him to stress the craft component in scientific method, the universality of pitfalls, the uncertain nature of criteria of adequacy in scientific assessment, and the interpersonal nature of some components of scientific communication. The scientific use of tools is doubly "craft-like:" first, because the use of particular tools requires a craftsman's competence, and second, because the tools chosen at a given time influence the direction of future work. The scientist must therefore be able to assess the sorts of problems that the use of particular tools would allow him and his colleagues to deal with and whether these problems are optimal for progress in his field, and this assessment involves the sort of uncertainty that can only be navigated with a craftsman's expertise and tacit knowledge.

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[64] See his "Problems of Empiricism" in Zilsel, Krohn & Cohen (2000), p. 176.

[65] Ravetz (1971), p. 146.

Ravetz applies the model of science as craft labor to construct a historical argument to the effect that whereas, from the 17th to the 19th century, science was conducted in accordance with its craft nature, it increasingly adopted industrialized methods of production as it entered the 20th century. Where the craft worker worked alone, or with a few apprentices, industrialized science requires substantial capital, a large group of scientists, a clear division of labor between them, and common goals to be set and managed by a scientific director. Ravetz argues that this industrialization of science has made it subservient to the state and to industry. In order to avoid the resulting “debasement” and “corruption” of science and the use of its results toward socially and ecologically catastrophic ends, the nature of science as a craft must be recognized and taken into account in the planning of science.

I mention Ravetz in order to highlight an important feature of the labor-process model. Ravetz is an essentialist about science. He believes that science has an optimal developmental pathway that reflects its nature as a special craft. Now, the labor-process model emphasizes the role of purposes in determining the nature of the labor process. In his defense of the Hessen-Grossman thesis, to be discussed in the next section, Freudenthal & Mclaughlin (2009) give a useful account of how to conceive of purposes in labor-process analyses. A purpose is a need that can explain an action. In order to explain an action, needs must be formulated in terms of the means available. For example, in order to explain why a person is eating, it is not sufficient to point to their need for nourishment. The need must be concretized in terms of the type of food available to the person at the time and place of eating. A purpose is a need formulated in terms of the means available for satisfying it. The means available, however, are not up to the sole choice of the scientist, but depend on contextual factors like the level of technological development in the broader society, the division of labor the scientists can lean on, etc.

It follows that the activity of the scientist—the kind of labor he or she performs—is indeed determined by his or her judgments of the needs of research, as Ravetz points out, but only once those needs are concretized in terms of the means available. These means may in turn dictate different ways of working. Galileo formulated his need to study the heavens in terms of the recently invented spyglass. Since there were no professional instrument-makers of research-grade telescopes at the time, Galileo did indeed adopt a craft-style of working, both because of the skills he needed to make his telescopes and because both the telescopes of the time, and the instruments required to make them, were adapted to craft labor.[66] By way of contrast, in the 1880s the Paris Observatory director Ernest Mouchez formulated his need to study the heavens in terms of recently developed photographic techniques. Since the individual astronomer, even assisted by apprentices, was incapable of processing the large amounts of data thereby obtained, Mouchez imitated mass production techniques by assigning data processing to large teams of human computers who were not professional astronomers. In a further move imitating mass production, the processing of the data was standardized in order to produce a comprehensive star catalogue comprising the massive quantities of observations gathered from the other photographic observatories around the world. Funding for all this work was obtained from the French government by appealing to nationalistic sentiments.[67]

My point is that the style of labor, craft or industrial, in the two cases was partially determined by the interplay between needs and the means available. This claim militates against an essentialist view of scientific labor, because it entails that the nature of that labor necessarily depends on historically contingent factors outside the scientist's control. In the excursus, I noted that this dependence makes progress path-dependent, because the goals of the enterprise will depend on the means available to the scientists and not just on their own volition. This path-dependence is generated by the dialectic of means and ends inherent in the labor process.

2.3.2.4 Science as practice

The view of science as labor naturally raises questions about the skills of the worker and how those skills are deployed in the practice of science, or in other words about science as a practice. Thus it is not surprising to read constructivists explicitly drawing on the science-as-craft-labor tradition: "The third line of work in the constructivist tradition

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[66] On the absence of professional scientific instrument makers at the time of Galileo's pioneering telescopic work, see van Helden (1983). On the craft character of Galileo's work in general, and its dependence on the instruments available to him, see Lefèvre (2005).

[67] See Bigg (2000) for an account of this episode.

broadly conceived has focused on scientific practice in a variety of ways, usually described as taking off from Kuhn's (1962; 1977) and Ravetz's (1971) pioneering discussions of science as craft work."[68] 'Practice' is usually understood in the sense of "regularities or commonalities in the performance or presuppositions of some community of human agents."[69] Practice-oriented researchers therefore focus on the elements of skilled improvisation necessary for many individual performances of science, and on the question of how these performances can give rise to regularities across the scientific community. Clarke & Fujimura (1991) identify several activities involved in scientific practice that span the distance between the improvisation required at the individual level and the stability observed at the community level, including "constructing doable problems; crafting, tinkering and making ad hoc arrangements; and standardizing and stabilizing the elements in the situation, including collective actions as disciplining tools to achieve continuity."

Tools are important elements in stabilization. For example, Clarke (1987) illustrates how the recalcitrance of tools (in her case, opossums in embryological research) can prevent the emergence of a research tool that is reliable across research situations. Hacking (1992) and Pickering (1995) have argued that *coherence* of the elements of an experimental practice has to be achieved in order to stabilize it. On their view, the resistance of the instrument is an important obstacle in the achievement of coherence, say, with theory. As mentioned above, Ravetz (1971) has argued that tools impart what might be called a certain "path-dependence" on scientific research, and so the proper selection of tools requires expert judgment.[70]

It should be noted that the sense of practice as a regularity is not the only one that has been endorsed in the literature. Rouse (2002) defends a normative conception of practice according to which

> [the] constituent performances [of the practice] are appropriately regarded as answerable to norms of correctness or incorrectness. Not all practitioners perform the same actions or have the

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[68] Clarke & Fujimura (1991), p. 4.

[69] Rouse (2002), p. 161. See also Landecker (in press).

[70] See Peacock (2009) for a discussion of path-dependence in the production of scientific knowledge.

> same presuppositions, but practitioners and other constituents of a practice are accountable for performances or presuppositions that are inappropriate or otherwise incorrect.[71]

On the normative conception, a practice is defined by the norms of correctness or incorrectness to which its constituent performances are answerable, rather than by an underlying regularity of action or belief.

Chang (2011) articulates an activity-based view of scientific practice that seems to fall under the normative conception:

> A serious study of scientific practice must be concerned with what it is that we actually do in scientific work … This way of thinking leads into the analysis of scientific practice in terms of epistemic activities. An *epistemic activity* is a coherent set of mental or physical actions (or operations) that are intended to contribute to the production or improvement of knowledge in a particular way, in accordance with some discernible rules (though the rules may be unarticulated). Because activities are rule-bound systems of actions, they are inherently normative in the sense that the actions within an activity are continually evaluated in terms of their conformity to the rules.[72]

This activity-based view is intended to encompass both intellectual and manual aspects of scientific work: "everything from calculating to smelling, from glassblowing to computer simulation, from synthesizing specific pharmaceuticals to explaining the structure of the universe." Epistemic activities are generally practiced in relation to others, forming what Chang (2012) calls a 'system of practice.' A set of epistemic activities forms such a system when they are performed with a view to achieving certain aims. The system is coherent when the constituent activities combine effectively to achieve the aims of the system; this kind of coherence differs from the logical notion of coherence as consistency between propositions.[73]

Chang recommends using activity-based analysis to refresh topics in the philosophy of science that have traditionally been understood as being about the propositional aspects of scientific work. In this dissertation I aim to do something akin to Chang's recommendation with respect to questions about scientific progress, in particular what it consists of, how it is achieved and what distinguishes it from the forms of progress exhibited by other intellectual endeavors.

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[71] Rouse (2002), p. 169.

[72] Chang (2011), p. 209.

[73] Chang (2012), p. 16.

Lefèvre (2005) worries that conceptualizing science as a social practice tends to reduce science to mutual interactions between people, which interactions are not conceived as conditioned by non-social factors. (By "non-social factors" Lefèvre has in mind the material character of the means of scientific work). "The popular references to scientists' 'negotiations' about observations, conjectures, conclusions, or concepts are indicative of this understanding of the social relations among scientists. However, is it really plausible to take the relations of tradesmen on the market place as standard for the social relations of scientists?"[74] Lefèvre argues that it is not, because such a conception ignores the "hybrid character of the production process," the fact that the material means of labor blend social and natural properties. That a given object serves as a tool is determined in part by its human users, but their use of it is in turn constrained by the natural properties of the object. Lefèvre emphasizes that the material means of production react back on the social organization of labor and the forms of cooperation and communication.

This dependence of the social on the material holds not only for ordinary material labor processes but also for science, as the history of science shows.[75] The material means of science include not only things that resemble, or in fact are, production apparatuses, like certain observational instruments or, say, distillation apparatuses. They also include "material means of scientific thinking" like diagrammatic representations or numerical notations. These material means of thinking "delineate a horizon of what results scientists can achieve and even what results are conceivable or probable."[76] The application of the material means of science generates a surplus of knowledge, which arises from "the simple but rarely sufficiently acknowledged fact that humans can gain more knowledge from the use of a means than was needed to invent it in the first place."[77]

The dependence of science on material means informs the perspective of this dissertation. Though Lefèvre alludes to a "surplus of knowledge" that is created from the application of material means, he does not inquire into the details of how this surplus is

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[74] Lefèvre (2005), p. 205.

[75] Lefèvre (2005), pp. 211-214.

[76] Lefèvre (2005), p. 217.

[77] Lefèvre (2005), p. 215.

generated in science, or into how the development of the material means influences scientific progress. I will develop the connection between surplus knowledge and scientific progress in chapter 7.

2.3.3 The sociological model[78]

Joseph Ben-David's (1971) study of *The Scientist's Role in Society* offers a helpful categorization of sociological approaches to the study of science.[79] Ben-David identifies two basic kinds of sociological *explananda*. A sociological theory may explain the behavior of scientists and scientific activity, or it may seek to explain the basic concepts and the logical structure of science (or both). He also identifies two basic kinds of *explanans*. On the one hand, the explanatory variables can be predominantly interactional, involving the way scientists interact, for example the division and coordination of scientific work, citation patterns, and "habits of consultation." On the other hand, the variables can be predominantly institutional, involving things individual scientists have no control over, for example the scientists' socially specified roles in a given country, the size and structure of scientific organizations, and different features of the economy, the political system, religion and ideology. This categorization entails four approaches to the sociology of science: "an interactional study of either scientific activity or the conceptual and logical structure of science, and an institutional study of the same two aspects."

Though for the purposes of this dissertation I think it is unnecessary to catalogue the many sociological approaches that have been applied to the study of science, a few approaches are worth mentioning due to their similarity to the labor process model discussed in the last subsection. An example of the institutional approach is the classic work of Boris Hessen and Henryk Grossmann in the 1930s, which has recently been defended by Freudenthal & McLaughlin (2009). Their work overlaps with the labor process

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[78] Though some of the approaches considered in the previous subsection may be considered "sociological," I have chosen to exclude them from this subsection because of their focus on the craft aspects of science.

[79] Ben-David (1971), p. 2.

model because they conceptualize science as one kind of labor within the system of social production. On Freudenthal and McLaughlin's reconstruction, Hessen's "The Social and Economic Roots of Newton's *Principia*" (1931) advanced three theses, the first of which was independently proposed by Grossmann and the second of which Grossmann also later assented to.

> —The first thesis concerns the relation of economic and technological developments in the early modern period and the relation of these two to the emergence of modern science: Theoretical mechanics developed in the study of machine technology.
>
> —The second thesis draws the converse conclusion: In those areas where seventeenth-century scientists could not draw on an existing technology (heat engines, electric motors and generators) the corresponding disciplines of physics (thermodynamics, electrodynamics) did not develop.
>
> —The third thesis concerns the ideological constraints placed on science in England at the time of the "class compromise" or "Glorious Revolution" (1688): Because of this compromise Newton drew back from fully endorsing the mechanization of the world picture and adapted his concept of matter so as to be able to introduce God into the material world.[80]

The first and second theses are the ones of interest here. They exemplify Ben-David's institutional approach in that the level of development of technology is not something an individual scientist can control. For example, the impact of technology on scientific concept formation is significant for understanding scientific change because it explains, according to Hessen and Grossmann, why certain abstract concepts arise when they do. As summarized by Freudenthal & McLaughlin:

> When (1) various different kinds of labor have been separated from the motive power applied in performing them, then motive power could also be conceptualized separately, and when (2) various kinds of the motion (circular, straight) produced by various motive powers (e.g., water, animal, man) could also be transformed one into the other by appropriate transmission machines, then it also made sense to form concepts of abstract motion and force …[81]

The Hessen-Grossmann theses differ from the labor process model, however, insofar as the role of technology in science is limited to providing observational material to stimulate concept formation and theorizing. The labor process model, in contrast, emphasizes the use of technology *in* science.

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[80] Freudenthal & McLaughlin (2009), pp. 2-3.

[81] Freudenthal & McLaughlin (2009), p. 14.

A classic example of an interactional approach that emphasizes the use of technology in science is Latour and Woolgar's (1986) social constructivism. On their view, theories and facts are constituted by social constructs. Scientific change arises through scientists constructing facts in the lab by means of communication, persuasion and the use of apparatuses. Latour and Woolgar explain laboratory dynamics in terms of a model of competition: the personality, institutional affiliation, and rank of the researcher, the nature of the research, and its potential for future investigations count as much as strength of argument, cogency of evidence or style of reasoning.

Their view resembles the model of science as a labor process in that it focuses on the necessary intervention of human labor in the production of knowledge and especially on the role of instruments in that process. As is well-known, Latour and Woolgar (1986) treat scientific instruments as "inscription devices," apparatuses that provide some sort of symbolic output. An inscription device is "any item of apparatus or particular configuration of such items which can transform a material substance into a figure or diagram which is directly usable by one of the members of the office space."[82] So a scale on an apparatus is an "inscription device" if it provides information about a new compound, a machine if it weighs something, a checking device when it is used to verify an operation. In short, an apparatus is used as an "inscription device" when it is used in an argument such as that involved in the construction of a bioassay profile.

The labor process model, however, does not entail the more controversial epistemic claims they make, such as that "the phenomena *are thoroughly constituted by* the material setting of the laboratory. The artificial reality, which participants describe in terms of an objective entity, has in fact been constructed by the use of inscription devices."[83] If such claims amount to the view that warranted scientific belief is in the end about the activities of scientists and not about states-of-affairs in nature, that is not the view that will be defended here: in my view, the labor process model is compatible with the view that scientists do in fact learn about phenomena other than their own activities. Indeed, as suggested by the role of the means of production in labor and the hybrid character of these

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[82] Latour & Woolgar (1986), p. 51.

[83] Latour & Woolgar (1986), p. 64.

mentioned in the excursus and stressed by Lefèvre, nature plays an essential role in the labor process. This role is presumably reflected in the content of scientific knowledge, though I will not try to work out exactly how here.

The claim that the production process is hybrid, both social and natural, may be contrasted with Latour and Woolgar's view that "[i]t would be wrong to contrast the material with conceptual components of laboratory activity."

> The inscription devices, skills, and machines which are now current have often featured in the past literature of *another field*. Thus each sequence of actions and each routinized assay has at some stage featured as the object of debate in another field and has been the focus of several published papers. The apparatus and craft skills present in one field thus embody the end results of debate and controversy in some other field and make these results available within the walls of the laboratory.[84]

That apparatus "embody the end results of debate and controversy" is only true for certain aspects of the apparatus. As Davis Baird has emphasized, the materiality of instruments exerts its own constraints on their construction.[85] This materiality is not always theorized or conceptualized, and is not always the subject of social processes like debate or controversy. For example, in their account of the construction of the first cyclotron Baird and Faust write that

> The information … in the practices which are passed from teacher to apprentice, at laboratories such as the Berkeley Radiation Laboratory, constitutes some of the *resources* for an instrument builder. Pyrex has proved to be a good material for constructing vacuum systems. Some unrecorded engineer figured out the spring system for converting linear motion into circular motion; now this small problem is solved. Experience too has taught us about the use of moving metal parts in a vacuum, and how to use acetone to detect leaks … We do not expect there to be a general theory for the conversion of linear motion into circular motion; instead there are techniques for doing so. There are fairly general theories about materials such as Pyrex, but no such theory would have as a consequence the fact that Pyrex is commonly a good material for the construction of vacuum systems. Such a consequence depends too directly on the specific contingencies of how experimental practice evolved. Still, the use of Pyrex does serve as an important technique in vacuum system construction.[86]

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[84] Latour & Woolgar (1986), p. 66.

[85] See Baird & Faust (1990), Baird (2004).

[86] Baird & Faust (1990), p. 171.

The "fact" that Pyrex is a good material for generating a vacuum was not theorized, according to them, and was presumably not the outcome of debate or controversy but rather determined empirically, by constructing a vacuum system with Pyrex and measuring the vacuum. True, the measurement technique might have been the outcome of debate, but the fact that Pyrex and not window pane or brass was found to be the best material for the vacuum system constitutes a material "remainder" over and above what can be obtained from theory and debate.[87]

The claim by Latour and Woolgar that phenomena are constituted by the material setting has a consequence for scientific change: a change in how facts are constructed entails a change in the phenomena. Latour and Woolgar write that "[i]t follows that if our observer [the anthropologist in the laboratory] was to imagine the removal of certain items of equipment from the laboratory, this would entail the removal of at least one object of reality from discussion." For example, they claim that "[t]he molecular weight of proteins could hardly be said to exist except by virtue of the ultracentrifuge."[88] Though Latour and Woolgar do not discuss the topic of scientific "progress," given what they say about the construction of facts it would seem that traditional philosophical notions of scientific progress in terms of the accumulation of truths or knowledge ought to be unavailable to them.[89] On the other hand, it seems reasonable to suppose that they could entertain a notion of progress in terms of the improvement of the persuasive power of the material techniques available for producing inscriptions, of the acceleration of fact construction through better methods for conducting and ending debate and controversy over the facts, or of reforms of the institutional structures shaping the interpersonal dynamics within the laboratory and between laboratories. In other words, for Latour and Woolgar the notion of the scientific progress would have to concern the social characteristics of science, and not the semantic and epistemic characteristics philosophers have traditionally been concerned with.

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[87] See Baird & Faust (1990), sections 2 and 3 for the details of the cyclotron's construction.

[88] Latour & Woolgar (1986), pp. 64-65.

[89] For example, they consider "the tendency to think of the inscription [produced by an inscription device] in terms of confirmation, or evidence for or against, particular ideas, concepts, or theories" to be part of the "mythology" of the laboratory, a view that suggests agnosticism (at least) about the possibility of accumulating justified beliefs (p. 63).

The views of Hessen and Grossmann also have implications for scientific change and progress. According to the Hessen-Grossmann thesis, the rate of development of major new theories depends on the rate at which new technologies are developed. Moreover, the development of new technologies leads to the formation of more abstract concepts to cover the increasing diversity of technologically-produced phenomena. More abstract concepts expand the explanatory scope of theories, which may be considered a form of progress. The notion that technology both constrains and makes possible scientific achievement is similar to Lefèvre's emphasis on the material means as conditions of possibility of scientific work. The effect of technology on the scale and rate of knowledge production will be discussed in the context of the Instrumental Revolution in chemistry in chapters 4 and 5.

Summarizing the bearing of the sociological approach on the question of scientific change and progress, it may be said that this approach focuses on social variables that explain change and that are not taken into account by the intellectualist approach. The latter's focus on the history of ideas tends to be blind to variables such as the social characteristics of science or the kinds of technology existing in the broader society. The sociological approach does overlap with the labor process model, for example in the emphasis on the technological dependency of science and on science as a form of labor. This overlap is as it should be, for in recent decades there has been increasing interest among philosophers of science in the social practices of scientists and the epistemic effects of these practices (see section 8.2.3 on social epistemology).

On the other hand, social variables are not always central in an analysis based on the labor process model. Examples of social variables are the technology available in the broader society, social prejudices concerning intellectual and manual labor, the value attached to science by society, the uses made of science or of scientific activity in general, the organization of research, or the modes of transmission and diffusion of scientific knowledge. Though social variables are probably always relevant to the labor process, they are not necessarily central. This can be seen even in Marx's work, where the category is introduced as a universal condition of human existence before its particular form under capitalism is treated.

What *is* central is an understanding of science as the activity of embodied beings who rely on tools to acquire knowledge of the world. This understanding can be taken in either a sociological direction or a philosophical one. In this dissertation, for example, I have tried to take the latter direction by asking questions concerning scientific progress, understood as an interdependent accumulation of different forms of knowledge, that are raised by this understanding of science.

2.4 Scientific progress and the labor process model

The labor-process perspective focuses attention on two elements that are critical for answering the two questions posed in the introduction:

1. Why is it possible for scientists at a given time to have more epistemic abilities than scientists at an earlier time?
2. How is it possible for knowledge acquired in the past to be used in on-going or future research?

Those elements are abilities and instruments. In this section, I will do two things. First, I will discuss the nature of abilities, in particular what I called ‘epistemic abilities’ in the introduction. Second, I will discuss how instruments are related to the acquisition of abilities and the use of prior knowledge.

2.4.1 Abilities in philosophy of action and in philosophy of science

As stated in the introduction, an ability is a kind of power.[90] Powers, at first pass, are all and only those properties that (i) are possessed by agents and (ii) are typically expressed by the modal auxiliary ‘can.’ A power is an *ability* if and only if it relates an agent to an action. An ‘agent’ will here mean an entity capable of purposive action. Not all

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[90] Since I am not a philosopher of action, I rely here on John Maier’s (2014) review of abilities in the philosophy of action.

powers are related to actions: for example, understanding a sentence is not typically an action, whereas uttering one typically is. In this taxonomy, I have the power to understand English sentences, but not the ability. But I do have the ability to utter them.

What exactly is an ability? According to Maier (2014), a widely used, approximate analysis of 'ability' is the following:

(CA) *S* has the ability to *A* if and only if *S* would *A* if *S* tried to *A*.

This conditional analysis serves as something of a default analysis of abilities in the literature on the subject. However, it has been subjected to some criticism, and there appears to be little consensus on what the true analysis of abilities is. It is beyond the scope of this dissertation to try to solve this problem. For our purposes, the intuitive idea that abilities are powers that relate agents to actions will suffice.

As noted in section 2.2, whether or not ability should be identified with know-how appears to be an open question in the philosophy of action and epistemology. Some philosophers of science have identified the two.[91] In general, abilities have not seen much explicit discussion in the philosophy of science. They are, however, essential to both Kuhn's and Laudan's conceptions of scientific progress as increasing problem-solving ability (see section 2.2.). For example, Laudan (1977) claimed that "progress can occur if and only if the succession of scientific theories in any domain shows an increasing degree of problem-solving effectiveness." In his 1969 post-script to *The Structure of Scientific Revolutions*, Kuhn held that "the demonstrated ability to set up and to solve puzzles presented by nature is, in case of value conflict, the dominant criterion for most members of a scientific group" (205). The high value accorded to puzzle-solving ability in the natural sciences has for consequence that "later scientific theories are better than earlier ones for solving puzzles in the often quite different environments to which they are applied" (206). Abilities also underlie the versions of realism defended by Ian Hacking in his (1983) and Hasok Chang in his (2012). Hacking (1983) memorably proclaimed that "*if you can spray them* [electrons and positrons] *then they are real*" (24; italics in original). In a similar vein, Chang (2012) bases his 'active realism' on the view that "at least when considering knowledge as it exists embedded in a system of practice, we can gain new and better

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[91] E.g., Baird & Faust (1990), p. 147; Mizrahi (2013), p. 385; Chang (2017), p. 2.

insights by thinking of knowledge not as consisting in belief but in ability—an ability to do certain things reliably as intended, without being foiled by resistance from reality" (215).

Chang's notion of a 'system of practice' will help us narrow the category of ability to properly scientific abilities, or what I called *epistemic abilities* in the introductory chapter. A system of practice, in science, is a coherent set of epistemic activities performed with a view to achieving certain aims.[92] An 'epistemic activity' is "a coherent set of mental or physical actions (or operations) that are intended to contribute to the production or improvement of knowledge in a particular way, in accordance with some discernible rules."[93] These rules need not be articulated. Among epistemic activities, Chang includes classic scientific activities like explaining, hypothesizing, testing and observing, but also more unusual or contemporary ones like smelling, glassblowing, and computer simulation (provided these are performed in order to acquire scientific knowledge, of course).

In Chang's account, the difference between an action and an activity seems to be one of level of description. What might be considered an action at one scale, might be considered an activity on a lower level of description. Consider Chang's analysis of the combustion-analysis of chemical substance:

> The structure of actions and processes is not atomistic in a reductive way, unlike the structure of things and statements. Each epistemic activity can itself be analyzed as a system of activities, but the "component" activities are not necessarily simpler than the "whole" activity in an absolute sense, and the analysis can go on indefinitely. For example, take the combustion-analysis of a chemical substance. This can be analyzed as consisting of various other activities: burning the target substance; absorbing the combustion-products using other chemicals; weighing with a balance; making percentage-calculations; etc. And those component activities in themselves consist of other activities; for example, the activity of weighing with a balance consists in placing samples and weights on balance-pans, reading the number off the scale, etc. Now it may seem that we are getting to simpler and simpler activities as we continue in our analysis of actions, hopefully to reach a rock-bottom of atomic operations ... [But] [t]here is no lowest level of description, and no clear

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[92] Chang (2012), p. 16.

[93] Chang (2011), p. 209.

> end to the process of activity-analysis. Rather, the analysis should be carried out wherever, and as far as, it is productive.[94]

Chang uses "activity" and "action" interchangeably, and some of the activities he mentions, like reading the scale or smelling, could just as easily be called actions. Perhaps one way of putting Chang's point concerning levels of description is this: *A* is an activity if it can be analyzed into constituent actions, and *A* is an action if it is the (always provisional, according to Chang) end-point of an analysis of an activity. Thus *A* can be both an activity and an action, depending on its position in the analysis.

If we grant that an epistemic activity involves a set of actions, the successful performance of the activity will require a set of abilities relating the agent to the actions. Each of these abilities is what I call an 'epistemic ability.' Following Chang's lead for epistemic activity, I here define an epistemic ability as the ability to engage in a mental or physical action that is intended to contribute to the production or improvement of knowledge in a particular way, and according to discernible rules. When one has acquired the ability to engage in each of the actions required for an epistemic activity, and when one has acquired the "second-order" ability to perform these actions in the coherent, rule-bound fashion necessary for the success of the activity, then one has also acquired the ability to perform that activity.

For example, scientific observation typically involves several distinct actions or subsets of actions: sample preparation, manipulation of instruments, reading of the instruments, data reduction, and various other actions involved in data processing. When one has acquired the various abilities required for these actions, and can perform them coherently and according to the rules necessary for a successful outcome, then one has acquired the ability to observe. Scientific training largely involves the acquisition of a massive variety of epistemic abilities.

Abilities are intimately connected with instruments, because many actions can only be performed by means of the latter. In the next subsection, then, I will discuss how instruments are related to the acquisition of abilities and the use of prior knowledge.

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[94] Chang (2012), p. 17.

2.4.2 Abilities, instruments, and scientific progress

The conditions of action determine the abilities an agent can exercise. Thus my physical make-up enables me to jump two feet vertically, but not four. Galileo’s telescope enabled him to observe the moon and other objects in the solar system, but not objects outside it. The conditions of action thus delimit a horizon of possible actions for the agent. By ‘horizon’ I mean that the conditions simultaneously make possible, but also limit, actions for the agent. Tools allow humans to expand their abilities beyond what is permitted by their natural endowment. For example, whereas I am incapable of constructing a lengthy text simply by exercising my memorial abilities, with writing instruments in hand I can write hundreds of pages.

Even simple manual tools expand the horizon of possible actions well beyond what humans can do in virtue of their natural endowment. Nevertheless, the expansion remains limited because actions by means of manual tools are still highly dependent on native human abilities.[95] Machines, on the other hand, introduce a qualitative shift by virtually emancipating the horizon of possible actions from the constraints imposed by native human abilities. Machines are not simply complex tools. At least since the Industrial Revolution, they have tended to replace and displace human labor, which can have significant effects on the potential for action and the potential for progress of the labor processes in which they are incorporated.

These considerations on tools and abilities hold for scientific labor as well. Consider again the example of the telescope sketched in section 2.2.2. Every stage in the sketch resulted in new possibilities for action, for example:

- Observing details of planetary surfaces
- More precise aligning of telescopes on objects
- Measuring small angular distances and diameters
- Noting the telescope’s alignment more precisely
- Measuring the positions of faint stars

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[95] The notion of a native human ability is discussed in some detail in chapter 7.

- Collecting larger quantities of data
- Processing larger quantities of data
- Escaping the interference of the Earth's atmosphere
- Detecting non-visible radiation

Every stage expanded the horizon of possible actions accessible to humans to study distant objects. But this expansion was only possible because the developmental process came increasingly to depend on the capabilities of artifacts to do things that humans cannot do. It began with native human abilities to see and compute and ended with today's high-tech detecting and computing telescope systems.

Rather than focus on the human aspects of science as labor, as do Zilsel and Ravetz, I will focus on the human-instrument relationship that mediates the relationship between the inquirer and nature. The human-instrument relationship falls out of the labor process model as an essential component of scientific activity. The instruments of science play a crucial role, not just in the production of knowledge, but in the historical transformation of the production process itself. This transformation can be caused by extra-scientific forces, for example commercial instrument-makers seeking to expand their markets. But as I will elaborate in the following paragraphs, there is a mechanism internal to science that promotes this transformation: instruments provide a means for applying scientific knowledge to scientific work. This can be seen by considering a well-known topic in the philosophy of science, the relation between theory and instruments, in the light of the labor process model.

Before moving to that topic, and given the role of instruments in this dissertation, I think it appropriate to offer a working definition of a 'scientific instrument:'

> (SI) A scientific instrument is a material thing, or complex of things, that the scientist interposes between him- or herself and the object of knowledge and that serves to acquire knowledge of the object.

I write "complex of things" because some instruments are complex assemblages of devices, each one of which carries out one or more functions. Thus an instrument might consist of, say, a source of radiation, a sample handler, a detector, devices for controlling the instrument and processing the signal, etc. Scientific instruments can vary greatly in complexity, ranging from a chemist's beaker to a massive particle collider.

"Interposes" can be understood in causal terms. The instrument serves as an intermediate in a causal chain linking the scientist and the object of knowledge. If we want to gain knowledge of a system *S*, we construct *S* or otherwise causally interact with *S* and observe what happens. This is the purpose of experiments, but also applies to observational sciences, as when telescopes are used in astronomy. This causal interpretation becomes problematic, however, when we consider instruments that are used for theorizing, as when computer simulations are carried out to compute properties of natural systems. The computer is not an intermediate in a causal chain including the natural system, and so might not count as a scientific instrument according to (SI).

One way to respond to this problem is to note that uses of the computer in science, like simulations or data processing, are *derivative* of causal chains including the natural system, in the sense that they transform knowledge of the world already gained by means of experimentation and observation. This knowledge can take two forms. It can take the form of data, which are fed to the computer in digitized form for processing. Or it can take the form of mathematical descriptions of real physical systems, descriptions that are presumably derived from experience. Either way, what the computer does presupposes causal interactions upstream of it.

On the other hand, this response seems to preclude more speculative theorizing, which may be only distantly informed by experiment or observation, from counting as science. This restriction may be too much for some. Moreover, the causal interpretation ignores what is perhaps most significant about instruments like computers and other thinking aids, namely that they are used for purposes of *cognition* about natural systems. It seems more pertinent to say, in the simulation case, that the scientist interposes the computer between herself and the target system *in order to help her think about the system* than that she interposes it in order to mediate some causal interaction with the system. The computer serves as an intermediate in an inferential chain, rather than a causal one, leading to knowledge of the system.[96] Similarly, the use of compass and ruler in Greek geometry

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[96] For computer simulations as inferences, see Beisbart & Norton (2012).

allowed geometers to gain insight into the regularities of constructions that can be accomplished by means of them.[97]

So I would prefer to leave ‘interposes’ polysemous, signifying either causal or cognitive mediation. This vagueness is as it should be, given that scientists both interact with nature and reason about it (this duality will be a theme of the next chapter). Indeed, even instruments that interact causally with the object of knowledge can be understood as cognitive mediators, since the causal interactions are valued precisely because they help scientists learn about their domains.

Given the four-fold conception of progress I am adopting, and the fact that ‘knowledge’ appears in my definition of ‘scientific instrument,’ it is in order to say what kinds of knowledge scientific instruments can afford. An obvious use is to acquire observational and experimental knowledge, EK. But they can also be used to acquire theoretical knowledge, as discussed above. Arguably, PK might also be gained by means of scientific instruments. For example, forensic science makes use of various instruments, like mass spectrometers, breatholyzers, DNA sequencers and computers, to obtain knowledge relevant to police work.

On the other hand, MK does not seem like the sort of knowledge that can be the ultimate goal of scientific instrument use. Scientists use instruments in order to learn about nature, not in order to learn techniques or methods for learning about nature. One exception to this is that instruments can sometimes be used to improve the means themselves, as when an instrument is used for calibration, either of itself or of another instrument. More generally, instruments are used in methods development. But the latter is not the ultimate goal of that use of instruments, for the methods thus developed are merely ways of employing the instruments in order to acquire TK, EK or PK.

A familiar question in the philosophy of science is whether instruments are embodied theory, or theories are merely disembodied tools.[98] The familiar debates on the theory-ladenness of observation tend to be concerned with the first disjunct, whereas other familiar debates on the boundary between science and technology tend to be concerned

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[97] Lefèvre (2005), p. 218; Netz (1999), pp. 14-19 and 34-35..

[98] For an overview of this topic, see Gooday (2000).

with the second. In this dissertation, I will focus on instruments as embodied knowledge. I use ‘knowledge’ rather than ‘theory’ to take into account certain craft aspects of scientific instrumentation. When philosophers talk about instruments as embodied theory, what they typically have in mind is an ‘explanatory theory.’ An explanatory theory is, roughly, a theory that explains the outputs of an instrument in theoretical terms. This theory corresponds to TK in the classification of section 2.2.2. The construction of a sophisticated instrument, however, also requires a huge amount of PK. First, it requires an ‘engineer theory,’ already mentioned in 2.2.2, i.e. a systematic set of propositions and pictorial representations connected with engineering skill. Such a theory contains propositional knowledge of how to construct an artifact and is a highly developed form of the propositional side of PK noted in 2.2.2. There are also the many abilities needed to put the theory into action. Perusal of a textbook like Moore, Davis and Coplan’s *Building Scientific Apparatus* (2009) suggests a plethora of abilities, such as the ability to work with materials; to draw; to blow glass; to use hand tools; to master techniques for manipulating light; to troubleshoot and identify the sources of problems in a welter of apparatus; mathematical abilities for calculating signal-to-noise ratios, etc. Some knowledge has to be acquired through experience: many abilities have to be acquired through practice, and some EK has to be discovered by trial and error, for example, the appropriateness of different materials for different constructions (Baird & Faust 1990). Finally, in some cases replacements have to be found for the decision-making processes and subjective judgments of human experts.[99]

In focusing on instruments as embodied knowledge, however, I am not interested in the relation between instruments and knowledge as a static logical relationship, but rather as an *achievement*. Here I am hearkening back to an earlier stage in the appreciation of the role of instruments in science, that of the 17th century scientific revolutionaries. Whereas nowadays the possibility that instruments could embody knowledge is considered

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[99] For examples of automating these aspects of scientific work, see: Schmidt & Güntert (2015) for NMR spectroscopy, Morris et al. (2003) for X-ray crystallography, and Perlin & Sinelnikov (2009) for forensic DNA analysis. Galison (1997) and Perovic (2011) discuss debates in microphysics on the value of automating high-energy experiments.

mundane, this was not the case for the likes of Descartes, Huygens or Kepler. As noted by Spelda (2017),

> The presentation of optical devices as resulting from effectively applied theory was very important for the new natural philosophy. Telescopes and microscopes showed that by the use of rational and technological approaches another relationship could be established between the investigative mind and nature, which in its fundamental elements refuses to show itself spontaneously to the human vision. The telescope and the microscope embodied the rational and methodical surpassing of natural sensory experience with the help of the technical expansion.
>
> Theoretical comprehension of the qualities of lenses opened the way to the methodical and planned improvement of optical instruments in the context of the limited technological possibilities of the time. Therefore the extent of scientific experience too is not given and unchangeable but created technically and transforms itself over time.[100]

The "other relationship" alluded to in the first paragraph is the unmediated relationship between human senses and nature favored by Aristotelian natural philosophy. The latter tended to discourage the use of instruments for scientific observation, because of the Aristotelian view that the human eye was set up as it is to provide reason with adequate images of the constitutive elements of nature—instruments were superfluous or even harmful for accurate observation, and the attempt to know about natural phenomena that could not be sensed was discouraged.[101] For the new natural philosophers, however, the possibility of embodying knowledge in instruments paved the way for greater progress in knowledge than would have been possible without it.

Why might this possibility exist? The usual answer is that instrumentation provides access to objects of inquiry that are inaccessible by means of our native human abilities.[102] A complementary, but less obvious, answer that more directly affects the temporal

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[100] Spelda (2017), pp. 11-12.

[101] Hutchison (1982) uses primary sources to argue that the Scholastics restricted science to entities and properties that could be observed through the senses, deeming the restriction "a major epistemological impasse not surmounted until the seventeenth century." Smith (1981, 1990) details the privileged role of the senses for cognition in the Scholastic world-view. Eamon (1994) provides several examples of 17th-century natural philosophers calling for the use of instruments to go beyond the realm of the senses.

[102] The notion of a native human ability, as well as the answer sketched here to the question of progress through instrument development, are discussed in greater detail in chapter 7.

characteristics of scientific research is that the instrument's contribution is not necessarily fixed once and for all but can be enhanced over time, more so than the native human abilities. This is due to what might be called a "second-order" capability, what I will call 'plasticity.' By 'plasticity' I intend the capability of being made to assume a desired form. Technology is more plastic than humans. The degree to which the latter are plastic is constrained fundamentally by human biology. In contrast, the plasticity of instruments is, in principle, only constrained by the laws of nature, though in practice it must be adapted to human users.

The plasticity of instruments is extremely important for scientific progress, because the ability of instruments to significantly extend the range of experience is often not given in the early prototypes, but must be achieved through a sometimes lengthy and difficult process of development. For example, the earliest telescopes were only capable of two or three-fold magnification; much optimization was required to develop telescopes useful for astronomical research. Indeed, van Helden (1977) goes so far as to claim that the improvability of the telescope is part of what makes it a scientific instrument.

The plasticity of instruments as a factor in scientific progress is generally neglected in recent philosophy of science. Though Rescher (1978) makes technological innovation the basis of scientific progress, he does not inquire into the structural features of technology that make this innovation possible. Robert Ackermann's (1985) theory of scientific progress is based on progress in instrumentation. For example, he writes that

> It will be argued that the history of instrumentation provides an unidirectional explanation of progress, in that later, more refined instruments are uniformly preferable to earlier instruments directed toward obtaining data in the same domain, and that this fact is essential to understanding the creation of what will be called data domains for scientific theory.[103]

To my knowledge, however, he does not focus explicitly on plasticity, preferring to focus on the relationship between theory and data. Humphreys (2004, 2011) makes similar technological dependence claims to Rescher, and hints at the contrasting plasticities of humans and instruments in the following passage:

> The situation with the concept of computational tractability is in some ways the inverse image of the situation regarding what counts as observable. Minimalist empiricists had a very

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[103] Ackermann (1985), p. 34.

> narrow conception of what is observable, whereas the domain of what is considered here as observable with the aid of technological advances is much larger. With the concept of computational tractability the wide domain of recursive functions is drastically reduced in size to obtain the set of functions that are tractable with the aid of contemporary computational science. But within the domain of this technologically enhanced conception there remains a parallel, for what is considered computable goes far beyond what is actually calculable by the limited powers of humans, and it is not a permanent boundary but one that is a function of technological progress.[104]

This passage has the additional virtue of pointing out that theorizing in the mathematical sciences is not just dependent on improvements in observational technology but also on the computational technology available for theorizing itself.

Spelda (2017) gives many examples, from Hooke, Gassendi, Huyghens, Galileo and others, showing that the optimism of the new natural philosophers was based on the plasticity of instruments.[105] For example, in his major work *Dialogue Concerning the Two Chief World Systems* (1632) Galileo has Sagredo ask whether the new observations and discoveries made with the telescope will ever cease, to which Salviati replies that "if its (i.e., the instrument's) progress follows the course of other great inventions, one may hope that in time (*progresso del tempo)* things will be seen that we cannot even imagine at present."[106]

Salviati's reply above suggests that plasticity is a condition for progress in instrumentation, since it is what makes the hope of seeing unimaginable things rational. In order to understand this kind of instrumental progress, it may be helpful here to refine our notion of methodological progress. As described in section 2.2.2., methodological progress results from the accumulation of methological knowledge. In order to understand the relationship between abilities, instruments and progress, we need a conception of methodological progress that focuses more explicitly on abilities and instruments. Here I will draw on Kitcher (1993), where Kitcher points out that

> instruments and experimental techniques are valued because they enable us to answer significant questions. One instrument (or technique) may do everything another does and more

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[104] Humphreys (2004), p. 124.

[105] Spelda (2017), pp. 2, 6, 8, 10, 11, 12.

[106] Galileo (1967 [1632]), p. 67. Quoted in Spelda (2017), p. 2.

> besides. If so, then we make *instrumental* (or *experimental*) progress by adopting a practice in which the former instrument (technique) replaces the latter.[107]

Put in terms of capabilities (a more passive term than ‘ability,’ which I prefer to reserve for agents), we might define ‘instrumental progress’ as follows:

> (I) An episode constitutes scientific progress when it shows the adoption of a practice in which an instrument (technique) with more capabilities replaces one with fewer.

Instrumental progress improves our ability to learn about nature, and therefore contributes to methodological progress. Outside the context of inquiry, it might also contribute to progress in PK since, of course, increasing instrument capability adds to the range of applications that can be achieved by the use of instruments. For example, microwave technology can be used for studying chemical reactions but also for cooking.

By analogy with epistemic abilities, I will define an epistemic *capability* as the capability of an instrument to engage in an operation that is intended (not by the instrument, obviously) to contribute to the production or improvement of knowledge. The epistemic abilities of agents are dependent on the epistemic capabilities of their instruments. For example, a scientist’s ability to observe distant objects will depend on the capabilities of her observational equipment. Returning to the telescope example, if astronomers have the ability to observe objects that do not emit in the visible spectrum, they have it because the telescope they are using has the capability to detect non-visible radiation. If it does not, then they will not.

It follows that plasticity is a condition for instrumental progress. The importance placed on the presentation of optical devices as applied theory suggests that the new natural philosophers perceived a new *mechanism of progress* made possible by the mediation of instruments in the human-nature relationship. MK, PK, TK or EK acquired at one stage of science could be embodied in instruments, thereby adding to their capabilities. This instrumental progress would then allow more knowledge to be acquired at a subsequent stage. The process could be repeated, leading to cumulative and cyclical progress in the

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[107] Kitcher (1993), p. 117.

long term. In the words of a physicist writing in 1945, instruments provide an avenue for "*the application of science to science itself*."[108]

Does instrumental progress *constitute* methodological progress? If that were the case, the invention of a new instrument would amount to a form of MK. However, the fact that instruments can sometimes contribute to both MK or PK, depending on the context, suggests that it may be useful to add a fifth kind of knowledge to Mizrahi's original four, one that focuses more narrowly on instruments, abilities (or capabilities) and actions and that is independent of its potential contribution to MK or PK. Hence I will propose *instrumental knowledge* as a kind of knowledge:

> (IK) *Instrumental Knowledge*. Instrumental knowledge usually comes in the form of instruments or techniques for carrying out operations or actions.

For comparison, recall Mizrahi's MK:

> (MK) *Methodological Knowledge*: Methodological knowledge usually comes in the form of methods and techniques of learning about nature.

The main change, of course, is that "learning about nature" has been replaced by "carrying out operations or actions" in order to account for the possibility of non-scientific contexts of use. Just like MK, PK, TK and EK, IK is derived from scientific practice and is intended to be descriptively accurate. Moreover, it would seem to be a form of knowledge-how, for, as defined, IK constitutes knowledge of how to carry out certain operations or actions.

In summary, here is a sketch of how the various ideas proposed in section 2.4 work together. My interest in instruments as embodied knowledge is fundamentally pragmatic, for I view the embodiment of knowledge as a method for exploiting prior knowledge in the acquisition of new knowledge. The "embodiment of knowledge" here refers to the

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[108] Klopsteg (1945), p. 572 (emphasis in original). In the text, the application is in fact what Klopsteg calls "instrumental technology," or "the science and art of applying instruments" to scientific problems.

construction and development of instruments. This construction and development illustrates the interdependence of different forms of progress. Moreover, the possible extent of construction and development depends on the plasticity of the technology in question. When a new instrument is developed, an item of instrumental knowledge has been gained. If the instrument has more capabilities than one it replaces, then such an episode counts as instrumental progress. The latter contributes to methodological and practical progress (and may eventually to theoretical and empirical progress as well). The augmented instrumental capabilities increase the epistemic abilities of the agents who use the instruments, since the abilities of the former depend on the capabilities of the latter.

Viewing science as a labor process, and putting it in relation to scientific progress, has thus suggested several new notions pertaining to instruments, abilities and progress: plasticity, the interdependence of different forms of progress, instrumental progress, epistemic abilities, and instrumental knowledge. Let us now return to our two guiding questions:

1. Why is it possible for scientists at a given time to have more epistemic abilities than scientists at an earlier time?
2. How is it possible for knowledge acquired in the past be used in on-going or future research?

We see that the answer to the first question, sketched in the previous paragraph, is epistemologically richer than one might have thought at the outset. Moreover, the answer to the first question depends on the answer to the second, for the increase in abilities depends on the possibility of exploiting prior knowledge.

In the next chapter, we will examine another feature of labor, namely the ideological conditions under which it takes place. An interesting result is that the onset of the dynamic of instrumental progress that started during the Scientific Revolution may have been heavily influenced by such conditions.

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3.0 HISTORICAL-EPISTEMOLOGICAL CONSIDERATIONS ON THE ORIGINS OF THE SCIENTIFIC METHOD

3.1 Introduction

In chapter 1, I claimed that the scientific method involves a synthesis of mental and manual labor. In this chapter, I will consider the origins of modern science in the 16th and 17th centuries. I will provide grounds for thinking that attitudes towards labor played a critical role in the emergence of modern science. These attitudes concerned the possible role of manual labor in science; the means of scientific labor, i.e., the means by which knowledge of the natural world was to be acquired; and the proper object of science.

In light of Spelda's study, one historical question one can ask is, why did the embodiment of knowledge in instruments become a conscious and valued strategy for progress by scientists when it did? What impediments to this strategy existed beforehand, and how did the natural philosophers of the early modern period overcome them? In this chapter, I will examine some texts from the history and historiography of the Scientific Revolution and suggest some hypotheses as to why the embodiment of knowledge in instruments became a conscious and valued strategy when it did. In doing so, I will show the utility of the conception of science as labor. As observed above, the labor process involves a number of heterogeneous elements, including tools, activities, skills, division of labor, raw materials and products, and "ideological" conditions (e.g., attitudes towards work). The focus of chapter 3 is on the latter conditions. It will be argued that social attitudes towards different kinds of work, notions about the proper object of science, and beliefs about how instruments and human faculties are related were important factors in the emergence of an experimental science in the early modern period. These ideological factors constituted so many conditions affecting the evolution of scientific abilities.

The first part of the chapter will discuss attitudes towards manual labor and the role of "maker's knowledge" in science. I will argue that Aristotelian views of natural philosophy and the arts tended to discourage the full-blown acceptance of manual labor and maker's knowledge as components of scientific method.

The second part of the chapter will concern Kepler's optical theory as developed in the *Ad Vitellionem paralipomena (Supplement to Witelo)* of 1604.[109] I will argue that his treatment of the eye as an instrument is a solution to a different kind of problem than the one that animated his Perspectivist predecessors, who accepted the Aristotelian theory of perception and cognition. Superficially, both Kepler and the Perspectivists appear to be concerned with figuring out how vision works. But whereas the latter were concerned with how to achieve certainty in perception, the former is concerned with how to achieve certainty in measurement. These two problems involve different relations to the means of acquiring knowledge. The problem of the certainty of our perceptions is *anthropocentric* in the sense that it concerns the certainty of the knowledge acquired by means of the human perceptual apparatus. In contrast, the problem of certainty in measurement is *non-anthropocentric* in the sense that the human perceptual apparatus is not required to produce a measurement. The eye just happened to be the main instrument of astronomical measurement at the time, and Kepler treats it as such. I conclude that Kepler's theory of vision, and his use of it in the *Optics*, suggests an egalitarian attitude towards the means of observation and helped to prepare the conceptual ground for the instrumentalization of scientific observation.

3.2 The Scholastics, manual labor and the role of maker's knowledge in science

3.2.1 Introduction

In "The Development of Scientific Method in the School of Padua" (1940), John Herman Randall famously claimed that Scholastic philosophers, largely based at the

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[109] I will use *Ad Vitellionem* to refer to the original Latin in the *Gesammelte Werke* and *Optics* to refer to William Donohue's translation.

University of Padua, developed what would later become known as “the scientific method” through a cooperative critical effort, spanning several generations, directed at the Aristotelian texts on logic. With the exception of mathematization, the basic method used by the great scientists of the seventeenth century emerged complete from this effort. What became known as the “Randall thesis” was seminal because it challenged the orthodox view that the scientific method had been developed *against* the methods of the scholastics, supposedly more interested in disputation than the discovery of novelty.

In section 3.2, I propose to assess the plausibility of Randall’s claim. I argue that as a matter of empirical fact, the claim is implausible. I further argue that there are conceptual reasons for thinking it unlikely that such a method could have emerged from within a purely Aristotelian framework. My reason for revisiting the Randall thesis is that it is an appropriate foil for the view of science as labor, for an implication of the thesis is that the human-instrument relationship, or the notion of production, did not play an important role in the emergence of the modern scientific method. By exploring why these elements were missing from the scholastic methodological development, we will learn something about the nature of modern science and the historical conditions for its emergence.

The section is organized as follows. I describe Randall’s main theses in the following subsection. In section 3.2.3, I review the contributions the Scholastics made to the development of scientific method, as described in Randall’s paper. In section 3.2.4, I discuss empirical arguments by Charles Schmitt that cast doubt on the claim in question. In section 3.2.5, I discuss the relationship between experimental science and Aristotelian philosophy of science. I argue that some sort of rupture with the latter was necessary for an experimental science to emerge, because the Renaissance Aristotelians took an essentially passive attitude towards observation, relying on the world to act on the observer in order to have experiences rather than producing the experiences themselves. In my view, experiments perform an essential function in the modern scientific method: the production of observational situations not readily available in the socio-natural environment. I then discuss Edgar Zilsel’s equally famous thesis, from the 1940s as well, that the merging of artisanal and intellectual traditions in the early modern period produced the modern scientific method, suggesting that it provides a plausible explanation for how forms of know-how essential for experimentation made their way into the scientific method. In section 3.2.6, I consider two conceptual paths by which an Aristotelian experimental science could have emerged. First, I suggest that the conception of logic as an instrument, defended by Jacopo Zabarella and Paulus Vallius, might have produced such a rupture from within Scholasticism, had the tradition not fallen into decline. I then examine a recent (2004) argument by William R. Newman that the medieval alchemists developed an Aristotelian experimental science on the basis of Aristotle's concept of perfective art. I argue that though the concept clearly allows for a more permissive attitude to experimentation than has generally been thought compatible with Aristotelianism, the license to experiment is restricted by the teleological framework of the latter. I offer concluding remarks in the final section.

3.2.2 Randall's two theses

Randall claims that "the basic idea of an experimentally grounded science of the mathematical structure of nature appeared as soon as Europeans began to explore the wisdom of the ancients … the idea of such a science, and much of its method and concepts, were in the possession of Europeans from the twelfth century on" (179). Though Randall mentions in passing Augustine, Arabic versions of Alexandrian science, and Archimedes, the influence that does most of the work in Randall's account of the development of scientific method is Aristotle. Randall argues that the basic idea of experimental science was developed by Scholastic philosophers and some physicians, largely in Padua, through a process of constructive criticism of the Aristotelian texts on scientific method, especially the *Posterior Analytics*.

> From the beginning of the fourteenth century … there set in a persistent and searching reconstruction of the Aristotelian tradition, which, when directed to the *Physics*, led by gradual stages to the mechanical and mathematical problems of the Galilean age, and when directed to the *Logic* led to the precise formulation of the method and structure of science acclaimed by all the seventeenth-century scientists.

With the exception of the mathematical element, the elaboration of the method that was eventually adopted by the great scientists of the 17th century was essentially complete with the work of Jacopo Zabarella, who lived from 1533 to 1589.

This claim supports two related, but distinct, theses. First, there is the well-known thesis of continuity or “Randall thesis” according to which 17th century science took over a method ready-made from the Scholastics. The only thing remaining to be added was the mathematical component, a task accomplished by Galileo. Second, there is also what I will call the Internalist Thesis, according to which the “idea of an experimentally grounded science” was developed by means of an internal critique of Aristotelian ideas on causal demonstration. By an “internal critique” I mean that the system of concepts of the Aristotelian theory of causal demonstration—effect, cause,[110] reason, fact, experience, syllogism, demonstration ‘of the fact’ and ‘of the reasoned fact’—provided the framework within which an extended debate took place over the nature of the inferential relationships between cause and effect that are suitable for scientific demonstration and discovery. According to the Internalist Thesis, this critique was sufficient to produce an experimentally grounded scientific method, minus the math. It is this thesis, rather than the continuity thesis, that I will examine and critique here.

An experimentally grounded scientific method involves (at least) two components (not including mathematical methods). One is a method for analyzing experiences in order to infer claims that reach beyond those experiences. According to Randall, the Paduan Scholastics of the late Middle Ages and Renaissance developed a method for inferring causal relationships from observation. This component is primarily a way of organizing one’s thinking about what experience is telling us about the world. The other component is an experimental method that allows one to make the observations required in order to solve scientific problems. This component involves thought, of course, but it also crucially involves acting on the world in such a way as to produce the requisite experiences.

In order to satisfy the Internalist Thesis, then, the school of Padua must have developed both of these methodological components. The question I will address in the

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[110] In this context ‘cause’ (from the Greek *aitia*) means any of the four Aristotelian causes: final, material, formal and efficient.

next section is, what grounds has Randall given us for thinking that the thesis was in fact satisfied?

3.2.3 The Scholastics' contribution to scientific method according to Randall

According to Randall, the Scholastics inherited from Aristotle the idea that scientific knowledge is obtained by the use of syllogisms to demonstrate a causal relationship between the premises and the conclusion. In the *Posterior Analytics* I.13, Aristotle distinguished between two types of demonstration, demonstration *tou hoti* and *tou dioti*, which were translated in the medieval Latin commentaries as demonstration "of the fact" and "of the reasoned fact." The latter kind of demonstration aimed to prove the reason why the given fact obtains. The Scholastics called these two forms of demonstration *demonstratio quia* and *demonstratio propter quid,* respectively. Whereas the former proves the cause of an effect, the latter proves effects through their causes. A famous example of such proofs, provided by Aristotle, is the pair of syllogisms connecting the fact that the planets do not twinkle with the fact that they are near the Earth. One member of this pair goes as follows:

a. The planets do not twinkle.
b. What does not twinkle is near the Earth.
c. Therefore, the planets are near the Earth.

This syllogism is an example of *demonstratio quia*, for it starts from the effect, that the planets do not twinkle, and concludes with the cause, that the planets are near the Earth. In order to connect these two facts through a *demonstratio propter quid*, the major and middle of the proof must be reversed:

d. What is near the Earth does not twinkle.
e. The planets are near the Earth.
f. Therefore, the planets do not twinkle.

Here the cause, that the planets are near the Earth, is the minor premise of the argument and the effect is the argument's conclusion. According to Aristotle, the proof of the reasoned fact is superior to the proof of the fact because it proceeds by way of explanation, showing how the effect results from the cause.[111]

For Aristotle, then, the theory of scientific demonstration was a theory of proof. According to Randall, the great service of Paduan Scholasticism was to transform this theory of proof into a theory of discovery. The Scholastic debate over the proper method for identifying causes was initiated by Pietro d'Abano in 1310 (185-188). D'Abano identified the two kinds of demonstration with two ways of teaching: demonstration *quia* was the "resolutive" way of teaching, whereas demonstration *propter quid* was the "compositive" way of teaching. Doing so modified the Aristotelian theory in two ways. First, it changed the question from a purely logical one of how causes and effects should be related in syllogisms to a methodological one of how to teach science. Second, d'Abano considered the resolutive way to have a legitimate claim to being science, albeit only because the weakness of the human mind required it to start from experienced effects in order to grasp causes.

The physician Jacopo da Forlì was the first to connect the two kinds of demonstration with the scientific discovery process. In 1475, he showed that the method of medical diagnosis was nothing other than a way of resolving effects into their causes (188-189). The medical teacher Hugo of Siena went a step further in 1489 by arguing that any complete science requires a double procedure to attain knowledge of causal relationships (189-190). To obtain knowledge of causes, one must discover them through their effects. Conversely, effects are known by relating them to causes as the consequences of the latter. The idea that the resolutive and compositive methods were merely successive phases of one method raised the question of whether the method was circular, since it appeared to infer effects from causes that had themselves been inferred from those same effects. From the end of the 15th century on, Paul of Venice, Agostino Nifo, Zabarella and others defended the method against the charge of circularity. Nifo introduced an important distinction between two kinds of knowledge of the effect. The first kind is obtained through sensory observation. Knowledge of the effect by the senses is the starting point from which the cause is inferred. Once the cause is inferred, a second kind of knowledge of the effect

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[111] *Posterior Analytics* I. 13.

can be obtained by deducing it from the cause. Since the two ways of knowing the effect are different, there is no circularity in the method. Nifo also introduced the idea that hidden causes can only be conjectured, and that knowledge of such causes is always less certain than their effects, since the latter are known by the senses (193-194). Randall views this idea as the original formulation of the method of hypothesis (194).

According to Randall, Nifo's formulation of the method marks a significant break with the theory of the *Posterior Analytics* and the tradition of theological scholasticism that stayed close to it. The break occurs because Nifo makes the truth of the causes inferred from sense-experience dependent on that experience, whereas previously their truth had been grasped by "sheer intellectual vision" (194).

Though Randall mentions several more contributors, the most important for my purposes is Jacopo Zabarella. For Zabarella, logic was not a science in its own right, but rather an instrument for "producing knowledge of the unknown from the known" (197). The sciences themselves were nothing other than applied logic. The role of scientific method was to reveal the necessary connections between things. Since all such connections are causal, according to Zabarella, the role of method was to establish causal relations between things. Zabarella had a relatively complex understanding of the method. First, he identified a resolutive method, which infers an unknown cause from a known effect. At this stage, both the effect and the cause inferred from it are known only confusedly. A "mental consideration" of the cause, however, allows it to be known distinctly. After this second stage, the effect can be demonstrated from the cause, so allowing the effect to be known distinctly now as well.

An important feature of Zabarella's resolutive method is that it allows two kinds of causal inference. Demonstration *a signo* allows us to infer causes whose instances cannot be observed by the senses. "Induction," on the other hand, allows us to infer causes whose instances *can* be so observed (198). For example, we can observe instances of the laws of motion simply by observing moving bodies. In induction, the analysis of instances yields knowledge of the universals they instantiate. In contrast, no instances of first matter can be observed, and so the latter must be inferred by demonstration *a signo*. Randall thinks the distinction between the two kinds of cause anticipates that of Newton between the mathematical principles of physical theory and the specific forces acting on bodies (198).

He emphasizes the importance of Zabarella's induction, which allows the first principles of science to be discovered. These had formerly been taken to be indemonstrable, for it was generally assumed that something could only be demonstrated through something else on which it depends, an obvious impossibility with first principles. Zabarella's originality lay in showing that the first principles could be inferred by grasping the observed effects as particulars instantiating a universal, intelligible structure like the laws of motion (198-199). He thus set the stage for the mathematization of natural philosophy, though he left that final step to Galileo.

Randall provides plausible grounds for thinking that the Paduan Scholastics made significant contributions to the development of modern science. The theory of proof in the *Posterior Analytics* was transformed into a methodology of discovery that anticipates the hypothetico-deductive method. Different ways of knowing were distinguished, in particular the distinction between the knowledge afforded by the senses and that afforded by causal inference. Different kinds of causes were identified, and the valid inferential relationships between cause and effect were elucidated. Distinguishing features of scientific experience vis-à-vis ordinary observation were established. Nevertheless, one is entitled to ask whether these contributions amounted to the experimentally grounded scientific method characteristic of modern science. For a curious feature of Randall's article is that, despite all of the contributions documented in it, no mention whatsoever is made of how the effects so carefully analyzed by Zabarella and his colleagues are to be produced. For Randall's Scholastics, scientific method is solely an affair of the head, what happens *after* the effect is observed. Where exactly is the experimental part of the "experimentally grounded scientific method" bequeathed by them to be found?

3.2.4 Schmitt on experience and experiment in Zabarella

Charles Schmitt picks up on this question, raised by Randall's article, in his own (1969) piece "Experience and Experiment: A Comparison of Zabarella's View With Galileo's in *De Motu*." In the course of assessing Randall's continuity thesis, Schmitt analyzes Zabarella's texts on natural philosophy in order to see how the terms *experientia* and *experimentum* are used there. He finds that whereas *experientia* figures prominently in Zabarella's natural philosophy, *experimentum* seldom appears, and when it does it is not at all clear that it means the same thing as the modern term "experiment." His main conclusion is that "*experimentum* does not function as a central technical term in his [Zabarella's] philosophy in the way in which *experientia* might be said to do."[112] According to Schmitt, for Zabarella *experientia* appeared to mean "intelligent personal experience or observation of the external world," and he used it frequently to resolve disputes in natural philosophy. Schmitt argues that Zabarella was a careful and avid observer of technological processes and natural phenomena. Nevertheless, though Zabarella made use of information gained from previous experiences or observations to solve scientific problems, he did not "consciously, and with forethought, attempt to test a particular theory or hypothesis by devising a specific experiment or observational situation by which to resolve the question."[113] Schmitt distinguishes between a science based on experience of the natural world and one based on experimentation:

> In the first case, the experiential aspect, which is utilized, is derived from what has been observed to have occurred previously and is hence unplanned; in the second case, the experience which is considered to be relevant has been planned out beforehand. Consequently, in the second case, one decides the question at hand on the basis of the results of the chosen observational experience. In short, experiment necessarily involves foresight and planning; experience does not.[114]

Schmitt concludes that though Zabarella qualifies as an empiricist, he does not qualify as an experimentalist.

As will be made clear from what follows, I think Schmitt's distinction between experience-based and experiment-based science is quite illuminating with regard to the possibility of a purely internal development of an experimental science within the Aristotelian conception of science. For the time-being, suffice it to say that if Schmitt is correct, it follows that Zabarella probably did not use or conceive of an experimentally grounded scientific method. Since Randall presents Zabarella as the culmination of the Paduan Scholastics' contribution to scientific method, it therefore seems unlikely that their

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[112] Schmitt (1969), p. 105.

[113] Schmitt (1969), p.105.

[114] Schmitt (1969), p. 106.

method had an experimental component. Schmitt's analysis suggests, then, that as a matter of empirical fact the Internalist Thesis is probably incorrect.

Schmitt does not try to explain the absence of experiment in Zabarella's methodology. In the following section, the question I will turn to is whether an experimental science was possible, given the constraints imposed by a purely internal development of the Aristotelian conceptual system.

3.2.5 The experimental method as production

In this section I will suggest that the experimental method is centered around the concept of production. Some quotations from Schmitt will illustrate the naturalness of describing experimentation in terms of production:

> Implicit in all of Galileo's uses of *experientia* [in the *De motu*] is the assumption that the observer plays merely a passive role: he does not *produce* an experience, but he has one. He is a mere observer in a world which can act upon him in a variety of ways. These actions of the physical world upon the receptive observer result in experience.[115]
>
> Here [in the *Discorso intorno alle cose che stanno in su l'acqua o che in quella si muovono*] for the first time the mathematical-Archimedean approach developed in the *De motu* is coupled with a more positive attitude toward experimentation and the manual ability to actually carry out the necessary experimental procedures.

Production, or productive activity, is the key concept in experimentation because the scientist must produce an observational situation that is not given to him by the natural or social worlds. To put it in economic terms: the experimentalist is responding to a situation of scarcity. By "productive activity" I intend an activity that produces an object of use external to the producer. The experimenter must design such situations, select and make use of artificial boundary conditions, control variables, construct and use instruments, make measurements, and so on. The goal is to produce a situation that is itself capable of producing an outcome of interest for solving the problem at hand. This goal is given discursive form by the formulation of what might be called a "when-then" proposition, in which the antecedent describes an experimental situation and the

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[115] Schmitt (1969), p. 114. Italics in the original.

consequent, a prediction of what will occur whenever that situation is realized.[116] An example of such a proposition is Huygens' isochronism theorem: "if a body descends along a path described by a cycloid, then the time of descent is the same regardless of the point along the path from which its descent begins."[117]

The experimental method is centered around the concept of production rather than causation. The latter concept is perhaps important for explaining why the experimental situation affords the outcome it does, but the activity of experimentation itself must be conceived as productive activity.[118] Experimentation has all the basic elements of any labor process: a purpose, expressed as a when-then proposition; instruments of labor, such as measuring instruments, mathematical techniques, boundary conditions, etc.; and an object of labor, either the natural objects on which the instruments are employed or, perhaps, the interaction between the objects and the instruments.[119] The chief difference with ordinary labor processes is that experimentation presupposes much greater freedom of exploration: "successful research needs to have the freedom to pursue unforeseen results and traces, to shift the focus of attention to unexpected by-products of the process under investigation, in other words, to yield to the drive of promising digressions that emerge in the course of the work."[120] It is also important for the practitioners to conceive of their own activity as productive, since the conscious element is obviously essential in the formulation of when-then propositions, the design of the experiment and selection of instruments, the ability to recognize unanticipated opportunities for research, and every other step in the process.

According to the view of experimentation that I have just sketched, the activity of producing effects is part and parcel of what it is to do science. There are grounds for thinking that such a view would have been difficult to accept within an Aristotelian framework. To my knowledge, there is very little evidence of Aristotle having performed experiments, though he appealed to experience quite often, at least in his biological

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[116] I borrow the notion of a "when-then" proposition from Smith (2002), p. 142.

[117] Smith (2002), p. 142.

[118] I write "perhaps" so as not to foreclose on the views of those who do not think causation is an essential component of explanation.

[119] For a defense of the concept of science as labor, see Lefèvre (2005).

[120] Lefèvre (2005), p. 218.

works.[121] In his philosophy of science, Aristotle distinguished between three kinds of science: (i) theoretical science, which includes natural philosophy, first philosophy, and mathematics; (ii) practical science, which includes politics and ethics; and (iii) productive science, which includes crafts like medicine, agriculture, ship-building, etc.[122] The key difference between the theoretical sciences and the productive sciences is that the former seek knowledge for its own sake, whereas the latter are aimed at the production of beautiful or useful objects. Based on this division, it seems reasonable to suppose that an Aristotelian could view productive activity as a means for producing objects that would further the goal of seeking knowledge for its own sake. But it does not seem reasonable to suppose that he could view it as an autonomous component *within* theoretical science. To echo Ian Hacking, experimentation could not have a "life of its own" within theoretical science, but would remain an external practice, though it might be enlisted to support the aims of theoretical science if the requisite experiences could not be obtained in other ways.

A passage in Zabarella suggests that he retained Aristotle's distinction between the theoretical and the practical or productive sciences. In *On Methods*, Zabarella argues that the "arts" are not a form of scientific knowledge:

> when we speak about methods, we give regard to the contemplative sciences, whose end is to know scientifically. For methods bring forth scientific knowledge of things unknown and so are the instruments of speculative sciences. Scientific knowledge, said properly, has a place in them but does not in the arts and all the other practical disciplines, which are concerned with contingent things and seek after action or bringing about an effect, not scientific knowledge … Scientific knowledge, therefore, properly speaking, does not have a place there; [what does have a place is] some sort of knowledge of something not necessary (unless from supposition of an end) and not itself inquired after for its own sake but for the sake of practical activity.[123]

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[121] Lloyd (1987).

[122] Shields (2015).

[123] "Pro huius dubii solutione sciendum est quòd quando de methodis loquimur, scientias contemplativas respicimus, quarum finis est scire, methodi namque scientiam rerum ignotarum pariunt, quare scientiarum speculativarum instrumenta sunt, in his enim scientia propriè dicta locum habet, non in artibus et aliis omnibus operatricibus disciplinis, quae in rebus contingentibus versantur et actionem, vel effectionem, non scientiam quaerunt. In his igitur propriè dicta methodus non datur, sicuti neque propriè dicta scientia neque vera necessitas. Si quam enim necessitatem habent, eam tantùm habent, quae dicitur ex suppositione finis, ut si hic homo sanandus sit,

On this view, learning how to, say, build and operate a device that produces concentrated beams of polarized electrons would count as "bringing about an effect," and therefore would not count as scientific knowledge, but using observations of the effects produced by that device to demonstrate violations in weak neutral current interactions would (to use Hacking's example).[124] The builders would not be engaging in scientific activity as builders of the device, but only as analyzers of the data produced by it. Hence Zabarella's assertion that scientific knowledge "said properly" has a place only in the *contemplative* sciences. "Knowledge for its own sake" appears to exclude knowledge of how to manipulate the natural world, no matter how involved the latter knowledge is, because it is conditioned by an end: instruments are built in order to test theoretical claims, bring about effects, or acquire data. In the context of natural philosophy, "knowledge for its own sake" appears to be restricted to causal relations, since these are necessary connections between things.[125] The know-how itself required to discover the necessary connections is not part of the science.

It is to the credit of Edgar Zilsel and other researchers, focused on the possible contributions the crafts and technology might have made to the emergence of modern science, to have challenged conceptions of science based on strict divisions between theoretical and productive activities. Zilsel was a contemporary of Randall's whose well-known article "Sociological Roots of Science" appeared in 1942, just two years after Randall published his own article on the school of Padua. Though Zilsel does not mention Randall, in some respects his article can be read as an externalist reply to just the sort of argument Randall is making. Zilsel emphasizes the importance of skills learned in the period of early capitalist production for the development of an experimental scientific method. His thesis is that such a method arose from the merging of the methods of superior artisans with those of university scholars during the Renaissance and early modern period. According to Zilsel, public intellectual life during the Renaissance was dominated by the

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necessarium est talibus remediis uti, simpliciter autem iis uti non est necessarium. Scientia igitur propriè dicta ibi locum non habet, sed cognitio quaedam rei non necessariae, nisi ex suppositione finis et ipsa non per se quaesita, sed propter operationem." Zabarella (2013 [1578]), pp. 156-159.

[124] Hacking (1983), ch. 16.

[125] Zabarella (2013), p. 137.

Scholastics and the humanists. The Scholastics dominated at the universities, whereas the humanists received patronage and employment from the upper classes. A common and central characteristic of both strata of intellectuals was that they despised manual labor. They distinguished sharply between liberal and mechanical arts, and considered only the former to be worthy of the "well-bred man".[126]

As capitalism developed, however, a class of superior artisans emerged that included artist-engineers (Brunelleschi, da Vinci, Cellini, etc.), surgeons, musical instrument-makers, makers of measuring instruments (such as compasses, astrolabes and cross-staffs), surveyors and navigators. This class developed technology and pioneered the use of empirical observation, experimentation and what Zilsel calls "causal research." The measuring instruments these artisans produced and used were the forerunners to modern scientific instrumentation, and their quantitative rules of thumb were forerunners to physical laws. What the superior artisans lacked, however, was a "methodological training of intellect," which up to now had been restricted to the scholars and humanists.

In short, Zilsel's thesis is that there was a social barrier between the analytical and the experimental components of the scientific method, and that "as long as this separation persisted, as long as scholars did not think of using the disdained methods of manual workers, science in the modern meaning of the word was impossible."[127] This situation started to change around 1600:

> the rise of the methods of the manual workers to the ranks of academically trained scholars at the end of the sixteenth century is the decisive event in the genesis of science. The upper stratum could contribute logical training, learning, and theoretical interest; the lower stratum added causal spirit, experimentation, measurement, quantitative rules of operation, disregard of school authority, and objective co-operation.[128]

Zilsel singles out Gilbert, Galileo and Bacon as pioneers who combined the methods of the manual workers and academics, a move that resulted in the "Scientific Revolution."

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[126] Zilsel (2000), p. 12.

[127] Zilsel (2000), pp. 14-15.

[128] Zilsel (2000), p. 17.

As might be expected, Zilsel's account of the genesis of modern science was controversial and continues to be so.[129] Schmitt, for example, takes issue with Zilsel's characterization of the scholastics and in particular accuses him of "caricaturing" Scholastics like Zabarella as an "ivory tower academic, innocent of experience and of knowledge of technology."[130] In a footnote, Schmitt casts doubt on the priority Zilsel accords manual workers in the development of an experimental science:

> Although a good deal has been written concerning the influence of technology and craftsmanship on the emergence of 'modern science' and 'scientific method' during the 16th and 17th centuries, little attempt has been made to relate this tradition to the continuing tradition of natural philosophy in the universities (e.g., see Zilsel, 'The Sociological Roots …', p. 550). The examples we have cited from Zabarella are by no means unique … Without a doubt Leonardo, William Gilbert, and Galileo were influenced by technology, but to a significantly greater degree than those stodgy conservatives who held university chairs?[131]

On Zilsel's behalf, I think it is still possible to answer "yes" to Schmitt's question, despite the allegedly contrary evidence Schmitt claims to have provided. For his criticism of Zilsel is based, I believe, on an oversimplification of the latter's argument. In the passage to which the footnote is attached, Schmitt writes that his analysis of Zabarella's practice of natural philosophy shows that

> Zabarella had at least some awareness of the practical techniques used by the artisans of his time, something which most interpreters of Renaissance technology and craftsmanship have denied to university professors who were supposedly trapped in an ivory tower where no demeaning, practical considerations were allowed to enter.[132]

As this and the passages quoted above make clear, Schmitt thinks that Zilsel's claim is that the university scholars were unaware, or had no knowledge of, technology and the crafts. It is certainly true that Zilsel caricatures the Scholastics to the extent that his claims about the pioneering role of the artisans imply that the Scholastics had no interest in empirical observation or causal thinking. Randall, for one, offers plenty of evidence that

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[129] Pastorino (2017), p. 768. In his (2017), Pastorino argues that Francis Bacon's philosophy of experimentation was influenced by Bacon's experience supervising the drafting of patents while he served as a solicitor and attorney general during the reign of James I.

[130] Schmitt (1969), p. 127.

[131] Schmitt (1969), pp. 94-95.

[132] Schmitt (1969), p. 94.

they were deeply interested in the causal analysis of experience. But Zilsel's essential point has nothing to do with what individual university professors happened to be aware of or have knowledge of (if by the latter is meant propositional knowledge), as Schmitt would have it. The point is that, for the most part, they did not themselves *employ* the techniques used by the artisans of their time; at most they observed the artisans using them, as Schmitt documents for Zabarella's case. The scientific method consists of (at least) two skill sets: training in systematic and logical inquiry on the one hand, skills for the manipulation, production and control of natural objects on the other. The latter skills were acquired, claims Zilsel, over generations by the development of technology and handicraft production. This development was a necessary but not sufficient condition for the emergence of modern science, for the latter also required more narrowly intellectual skills such as those developed by the Scholastics. Technology itself is mainly important in Zilsel's account because it represents the skills necessary for experimentation. Once these skill sets were combined by Galileo, Bacon and others, modern science could take off. In short, Zilsel's story is about the merging of manual and intellectual labor, forms of labor that tended to be separated by status and prejudice in pre-capitalist societies.

I should probably add that my reading of Zilsel departs somewhat from the letter of his account. In its strong form, the Zilsel thesis is that both the bulk of the empirical content of science, as well as the experimental methodology, resulting from the Scientific Revolution came from craftsmen. In the terminology introduced in chapter 2, they provided the EK and MK of early modern science. Zilsel attributes what some would consider the most important features of science—research into causes, laws of nature, emphasis on empirical observation, quantitative measurement, skepticism towards authority, transgenerational cooperation—to the influence of craftsmen, leaving to the scholars only logical training and a systematic approach to research. I suspect that such a strong version of the thesis is hard to defend. What I take from Zilsel, in any case, is that *know-how* in the form of manual skills and the abilities to produce and use instrumentation are essential to modern science, and that the weakening of social barriers was an important factor in making this form of knowledge important in science. My use of Zilsel is thus guided by the broad conception of knowledge discussed in the first chapter.

In this connection, I think a few comments on a possible objection to my reading of Randall may be in order here. The objection starts from the fact that Randall does not claim that the philosophers at Padua developed their method in a bubble, but rather that they developed it "in fruitful commerce with the physicians of its medical faculty" (178). He further mentions the contributions of several medical writers and physicians, such as the Pietro d'Abano, Jacopo da Forli, and Hugo of Siena mentioned above. Given the manifest influence of medicine on the development of the method, my imputation of the Internalist Thesis to Randall would appear to be a case of false attribution.

I have described the Internalist Thesis as the claim that the idea of an experimentally grounded science was developed by means of an internal critique of Aristotelian ideas on causal demonstration. On my reading of Randall, the physicians made the following contributions to this development. First, they pointed out the methodological utility of Aristotle's theory of proof, both with regard to teaching as well as to discovery (187-189). Second, they provided empirical material for the refinement of causal analysis, in the form of diseases or anatomical demonstrations.

The first contribution found new uses for Aristotle's theory of proof. But as far as one can tell from Randall's account, it was always presupposed that the conceptual and logical requirements of teaching and discovery were to be satisfied by drawing on, with perhaps some adjustments, the resources of Aristotle's system. For example, the terms *resolution* and *composition,* which Pietro d'Abano, following Galen, used to designate two different ways of teaching, were identified with demonstration from effects to causes and from causes to effects, respectively (187-188).

The second contribution provided a spur for the philosophical critique of Aristotle. Yet, in the form of *experience*, new empirical material had always played a significant role in the Aristotelian tradition, starting with Aristotle. But at no point does it seem to have occurred to the philosophers discussed by Randall that *experimental* interventions, like dissection, were something the natural philosopher should *do*. For this, they would have had to conceive of their own activity as productive, a conception that I argued above was discouraged by the division between theoretical and productive sciences and by the elite social prejudice against manual labor.[133] Therefore I do not think the influence of the medical discipline represents a threat to the internalism I have attributed to Randall.[134]

3.2.6 Ways forward?

3.2.6.1 Logic as instrument

I argued above that Aristotelianism contains an inherent bias against viewing productive labor as a properly scientific activity. By way of concluding this paper, I will suggest that the late Scholastics did not remain hermetically sealed against ways of thinking derived from production, but on the contrary were starting to reconceptualize science as a productive activity around the time their tradition went into decline.

My evidence that such a reconceptualization was afoot is the view of logic advanced by Zabarella and the Jesuit Paulus Vallius that logic is an instrument, not a science or an art. Zabarella argued for this view in his “De natura logicae” of the *Opera Logica,* published in 1578, Vallius in his *Logica*, published in 1622 (though probably written much earlier[135]). According to Antonino Poppi, the puzzle Zabarella tried to solve in his work on the nature of logic was twofold.[136] On the one hand, Aristotle did not have a term corresponding to “logic,” even though he pioneered its study. It was therefore unclear where the discipline was to be located within Aristotle’s tri-partite division of the sciences. Thomas Aquinas, Duns Scotus and others held that it should be considered a

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[133] In *Promethean Ambitions* (2004), William Newman argues that the notion of a ‘perfective art’ in Aristotle provided the starting-point for the development of an experimental Aristotelianism in the Middle Ages. I critique Newman’s view in the next section.

[134] To be fair, in his (1926) *The Making of the Modern Mind,* Randall does mention the influence of the crafts on scientific development (pp. 217-219). He credits them with sustaining the interest in mathematics during the Renaissance, and with accumulating “a body of experience and knowledge about nature quite independent of the traditional lore.” But they are absent in his discussions of scientific method (pp. 219-224, 262-266).

[135] According to Mancosu (1996), p. 19. Wallace (1992), p. 24 suggests that Vallius followed Zabarella chronologically in dealing with this question.

[136] Poppi (2004), pp. 42-44 and 55-57.

theoretical science, along with natural philosophy and mathematics.[137] On the other hand, it was also unclear how logic is related to the intellectual virtues Aristotle lists in the *Nichomachean Ethics*, where it is conspicuously absent despite its importance as an intellectual state of the soul. Zabarella solved both of these puzzles by arguing that logic is an instrumental habit of mind. It could not be a theoretical science, he argued, because these aim at knowledge for its own sake, which is knowledge of necessary and immutable truths. Logic, on the other hand, deals with the discernment of truth from falsity in human concepts of reflection, and therefore with a contingent object.[138] So while logic can serve the sciences to acquire knowledge, it cannot itself be a theoretical science. Nor can it be a practical or productive science, because it does not produce an external reality but instead operates exclusively within thought. In a similar vein, Zabarella argues that logic, in contrast to the intellectual virtues listed in the *Ethics*, is not a habit sought for its own sake but rather because it serves the acquisition and operation of the virtues, which are ends in themselves.

Vallius, for his part, agreed with Zabarella that logic was an instrumental habit, though differing with Zabarella on its object. For Vallius, logic directs the operations of the intellect by employing beings of reason as instruments of knowledge. He concluded that logic could not be a science because the latter has for object real beings and their causes, not mind-dependent *ens rationis*.[139]

The characterization of logic as an instrument appears to be an innovation of late Scholasticism, one not made by Aristotle himself or his medieval followers.[140] Galileo appears to adopt this understanding of logic in his logical methodology.[141] Though Randall mentions Zabarella's instrumental characterization of logic, he sees it as a step in the transformation of the syllogism from a structure of proof to a method of causal inference (197). I think it is interesting that the concept of the instrument, closely connected with

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[137] Wallace (1992), p. 24.

[138] Wallace (1992), pp. 58-60.

[139] Wallace (1992), p. 24.

[140] The title *Organon*, meaning "tool," was used by medieval scholars to refer to a body of Aristotle's texts that included his logical works, but was not used to refer to logic itself.

[141] Wallace (1992).

those of production and labor, should make its appearance within Scholasticism at the same time as the developments described by Zilsel. Wallace (1992) claims that the conception of logic as instrument was an element of continuity connecting Galileo with the Scholastic tradition.[142] On the other hand, Wallace views Galileo's emphasis on experimentation as a point of discontinuity between them. Understood as a form of productive activity, however, experimentation seems at least conceptually connected with the innovation introduced by Zabarella and Vallius. Perhaps further reflection on the nature of instruments would have encouraged the Scholastics to incorporate manual labor within their conception of natural philosophy, thereby creating the possibility of a truly experimental Scholastic science.

3.2.6.2 Newman on the "perfective arts"

In *Promethean Ambitions* (2004), William R. Newman appeals to the history of medieval and early modern alchemy to make a strong case against what he calls the "noninterventionist fallacy." This fallacy consists of

> the very widely held idea, common not only among historians of science but also among students of Aristotle, that the Stagirite and his followers were fundamentally nonexperimental or even actively opposed to experiment, because experimentation involved intervention in natural processes.[143]

In book 2, chapter 1 of the *Physics*, Aristotle states that "[o]f things that exist, some exist by nature, some from other causes," the other causes being human production of artefacts. The characteristic of natural things is that

> each of them has within itself a principle of motion and of stationariness (in respect of place, or of growth and decrease, or by way of alteration). On the other hand, a bed and a coat and anything else of that sort, *qua* receiving these designations—i.e., insofar as they are products of art—have no innate impulse to change.

Aristotle concludes that "nature is a principle or cause of being moved and of being at rest in that to which it belongs primarily, in virtue of itself and not accidentally" (192b9-23). Thus the feature of artifacts that sets them apart from things in nature is that "[n]one

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[142] Wallace (1992), p. 301.

[143] Newman (2004), p. 238.

of them has in itself the principle of its own production" (192b29-30). To the contrary, artifacts have their origin in in the maker:

> All art is concerned with coming into being, i.e., with contriving and considering how something may come into being which is capable of either being or not being, and whose origin is in the maker and not in the thing made; for art is concerned neither with things that are, or come into being, by necessity, nor with things that do so in accordance with nature (since these have their origin in themselves).[144]

On this view, art would appear to be largely irrelevant to an understanding of natural things.

According to Newman, such a watertight separation between the artificial and the natural is qualified later in the *Physics* when Aristotle introduces the concept of a "perfective art." At *Physics* II 8 199a15-17, Aristotle adds that art can function in two different ways: "generally art partly completes what nature cannot bring to a finish, and partly imitates her." For Newman, the distinction introduced here "allowed the possibility of having two distinct types of art, one that perfects natural processes and brings them to a state of completion not found in nature itself and another that merely imitates nature without fundamentally altering it."[145] A common example of a perfective art is medicine, because the aim of medicine is not to lead the human body to an unnatural state, but rather to bring it to its natural condition of health by eliminating impediments.

The objective of *Promethean Ambitions* is to argue that medieval and Renaissance alchemists, working within an Aristotelian conceptual framework, relied on this concept of a perfective art to defend their work from attacks by "orthodox" Aristotelians that hewed to a strict division between nature and art. For the latter, the apparent replication of natural phenomena in the laboratory was a superficial imitation of nature that did not provide true understanding of it. For the former, in contrast, such experimentation was not merely imitation but the setting up of conditions under which a natural process could complete itself. Doing so allowed the natural philosopher to understand how the process works. One of Newman's strongest examples is the work of Themo Judaei, a 14th-century alchemist who had studied under the great Parisian Scholastic Jean Buridan. In what Newman calls

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[144] *Nichomachean Ethics* 6.4, 1140b1-23.

[145] Newman (2004), p. 17.

"a fully generalized credo on the advantages of what can only be called maker's knowledge," Themo argues for the value of replicatory experimentation:

> Likewise it must be known that it is said in the title of the question: "just as the rainbow or halo etc.," because it is difficult to know well the composition or manner of composing metals, just as it is difficult to know the way of generating the rainbow. And unless we knew how to make or to see the rainbow and its color, as well as the halo, by means of art, we would hardly be led to an understanding of the rainbow or the halo—how they come to be thus. Similarly, we would hardly—or never—know the composition of gold or silver unless we knew it through art; indeed, through art we can more completely know the operation of nature. And for this reason the question was placed under the aforesaid form.[146]

Newman interprets this passage as indicating that Themo has assimilated the notion of a perfective art to the replication of a natural phenomenon, and cites in support of this interpretation an assertion of Themo's that the artificial rainbow is a case of art aiding nature (*artem invantem naturam*).[147] Thus, Aristotle's concept of an art that completes nature provided a conceptual bridge from the natural to the artifactual by means of which alchemists like Themo defended their work.

Such evidence undermines the non-interventionist thesis because it shows, not only that at least one strand of Aristotelianism championed experimentation, but also that in doing so it did not break with Aristotle's own thought. This evidence would appear to undermine the view that experimentation was excluded for Aristotelians because it interfered with natural processes. Newman quotes Peter Dear as a proponent of this view:

> The natural course of a process could [only] be subverted by man-made, artificial causes, because art replaced nature's purposes with human purposes. An aqueduct, for example, is not a natural watercourse; it reveals the intention of the human producer, which thwarts that of nature….

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[146] Themo, *Quaestiones*, question 27, 203r–203v: ¶"*Item sciendum quod dicitur in titulo questionis: sicut iris vel halo etc. quod difficile est cognoscere bene compositionem vel modum componendi metalla sicut et difficile est cognoscere modum generationis iridis: et nisi per artem sciremus facere vel videre iridem et colorem eius et halo: vix duceremur ad cognitionem iridis seu halo quomodo fierent sic: similiter quod vix vel nunquam sciremus compositionem auri vel argenti: nisi per artem sciamus: vel per artem possumus scire completius operationem nature. Et propter hoc mota fuit questio sub forma predicta*." Quoted in Newman (2004), p. 248.

[147] Newman (2004), p. 248.

> *The Aristotelian distinction between art and nature depended on seeing human purposes as separate from natural ones and hence irrelevant to the creation of a true natural philosophy.*[148]

Dear's view is undermined if one allows that man-made, artificial causes could have an application beyond realizing human purposes, namely, completing those of nature. Such, according to Newman, was exactly the use made of artificial causes by the medieval alchemists.

To return to the main theme of section 3.2.6, the concept of a perfective art suggests another way forward for the Scholastics. Randall's Scholastics were largely concentrated on Aristotle's methods of reasoning, as expounded in the *Organon*, especially the *Posterior Analytics.* The alchemists, on the other hand, focused on Aristotle's detailed studies of the natural world, such as the biological works, the *Meteorology* and the *Parva naturalia*.[149] This difference of focus suggests a revised Internalist Thesis, one suitably expanded beyond the theory of causal demonstration. Perhaps a conceptual route to the experimental method existed within Aristotelianism after all.

I think it is worth pointing out, however, that the passage of the *Physics* that Newman draws on for the concept of a perfective art is more supportive of Dear's position than Newman allows. Here is a more extensive quotation of the passage from which Newman only quotes that "generally art partly completes what nature cannot bring to a finish, and partly imitates her":

> Further, where a series has a completion, all the preceding steps are for the sake of that … Each step then in the series is for the sake of the next; and generally art partly completes what nature cannot bring to a finish, and partly imitates her. If, therefore, artificial products are for the sake of an end, so clearly also are natural products. The relation of the later to the earlier terms of the series is the same in both.
>
> … If then it is both by nature and for an end that the swallow makes its nest and the spider its web, and plants grow leaves for the sake of the fruit and send their roots down (not up) for the sake of nourishment, it is plain that this kind of cause is operative in things which come to be and are by nature. *And since "nature" means two things, the matter and the form, of which the latter is the end, and since all the rest is for the sake of the end, the form must be the cause in the sense of "that for the sake of which."*

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[148] Dear (1995), p. 155. Quoted in Newman (2004), p. 240 (Newman's emphasis).

[149] Newman (2004), p. 289.

> Now mistakes come to pass even in the operations of art: the grammarian makes a mistake in writing and the doctor pours out the wrong dose. *Hence clearly mistakes are possible in the operations of nature also*. If then in art there are cases in which what is rightly produced serves a purpose and if where mistakes occur there was a purpose in what was attempted, only it was not attained, *so must it be also in natural products, and monstrosities will be failures in the purposive effort*. Thus in the original combinations the "ox-progeny" *if they failed to reach a determinate end* must have arisen through the *corruption* of some principle corresponding to what is now the seed.
>
> …Moreover, among the seeds anything must have come to be at random. But the person who asserts this entirely does away with "nature" and what exists by "by nature." For those things are natural which, by a continuous movement originated from an internal principle, arrive at some completion; the same completion is not reached from every principle; nor any chance completion, but always the tendency in each is towards the same end, if there is no *impediment*.[150]

This passage has two features that support Dear's position. First and most importantly, it stresses the distinction between natural and human purposes. The distinction is here grounded in the notion of a formal cause. Newman tends to neglect the formal cause in favor of the existence, or lack thereof, of an internal principle of movement. But this passage makes clear that the internal principle is entirely subordinated to the purpose "for the sake of which" the movement happens. Second, though Newman focuses on the idea of art "completing" nature, the passage suggests that Aristotle has a fairly negative conception of such an art, as an artificial compensation for the "mistakes," "failures" "corruptions" and "impediments" that prevent the natural purpose from being realized. Granted, this negative conception does not exclude a more liberal understanding of "completion," but the spirit of the passage is more restrictive than permissive.

So while I agree with Newman that Dear no doubt exaggerates the restriction imposed by the distinction between human and natural purposes on the use of "artificial contrivances" in natural philosophy, nothing in Aristotle's reflection on perfective art suggests that it is wrong to think that that distinction dictates what counts as an acceptable *ontology* for Aristotelian natural philosophy. The acceptable ontology is the class of

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[150] *Physics* II.8.199a9-199b18. My emphasis. Charlton's (1970) translation does not differ in substance from the one by Hardie and Gaye used here and in Barnes' (1991) *The Complete Works of Aristotle*.

processes fulfilling a natural purpose. In slogan form, one might put the ontological point as *teleology dictates scientific ontology*.

In my view, all that the "perfective art" passage licenses is the completion of a natural purpose by artificial means. This concept therefore enlarges the scope of what counts as a natural process—to the artificial engineering of the conditions necessary to complete some natural purpose. But there are many processes studied by modern science that do not seem to be accomplishing any natural purpose. The development and study of processes for making things that do not exist in nature—synthetic compounds, new elements, instrument-specific phenomena like mass-spectral fragmentations or particles created through high-energy collisions—arise from human purposes, not natural ones. I conclude that the concept of a perfective art does not license a *fundamental* role for "maker's knowledge" in science, in which processes serving human purposes count as legitimate objects of scientific inquiry. In modern science, *teleology no longer dictates ontology*.[151] The concept of a perfective art therefore does not settle the question whether some break with Aristotelianism was necessary to achieve a full-blown experimentalism.

3.2.7 Conclusion

The moral of this section is that the incorporation of instrumentation and experimentation as fundamental components of the scientific method was not trivial. It was discouraged by dominant attitudes towards manual labor and the division of labor between theoretical and productive sciences, as well as by beliefs about the proper object of scientific inquiry. Even Newman, a champion of the idea that there was an Aristotelian tradition of experimental science, has to locate the latter in a minority current that was constantly having to defend its very *raison d'être* to a hostile mainstream.[152]

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[151] Klein (2008) and Des Chenes (2007) make similar points in response to Newman's critique of the non-interventionist fallacy.

[152] See, for example, Newman (2004), pp. 242-250, 250-256, 269, 281, 286, 289. Vickers (2008) argues that Newman's own evidence demonstrates a "massive consensus" among philosophers, theologians and artisans that alchemical transmutation was impossible.

What also emerged in the discussion of the Randall theses is that the role of manual labor in the story is not merely that of a social background condition, as for example, Hessen and Grossmann conceived the contribution of technology to the development of physical theory (see chapter 1). Nor does it serve as merely a social *explanans* of the *explananda* provided by intellectual history. As note above, Edgar Zilsel took this approach: while taking for granted the fruits of the Scientific Revolution as recounted in intellectual histories—research into causes, laws of nature, emphasis on empirical observation, quantitative measurement, skepticism towards authority, transgenerational cooperation—Zilsel gave these developments a sociological spin, by attributing the scientific method mostly to superior artisans and limiting the contributions of the scholars to logical training and a systematic approach to research. In contrast, what I emphasized above is the emergence of know-how as both a component of scientific method—as represented in the use of instrumentation and in experimentation—and a product of scientific research. Know-how is not merely a background condition to modern science, but rather a constitutive part of it, and to my knowledge, at least, it is not the usual focus of intellectual history. We thus see the value of the broad conception of progress discussed in the first chapter: it allows us to identify kinds of progress that may not be as easily discerned by more traditional types of analyses, be they intellectual or social.

3.3 From perception to measurement in Kepler's *Optics*

3.3.1 Introduction

In section 3.3 I will study the relationship between Kepler's efforts to improve astronomical observation, on the one hand, and the manner in which he theorizes about the functioning of the eye, on the other. Certain modern commentators have drawn negative epistemological consequences from his treatment of the eye. By treating the eye like an instrument no different in essence from an artificial one, it is alleged that Kepler introduced a fundamental worry about the certainty of our perceptions. My purpose in section 3.3 is not so much to refute the allegation as to accentuate a more positive epistemological moral. The "instrumentalization" of the eye by Kepler should be viewed as an example of a general strategy for dealing with systematic measurement error, one that shifts the focus of research onto sources of error and (hopefully) results in their correction. Thus if we focus on the certainty of *measurement*, as opposed to *perception*, Kepler's approach is empowering rather than undermining. The study will largely consist of an analysis of selected passages from the *Ad Vitellionem paralipomena* (Frankfurt, 1604), or what I will refer to henceforth as the *Optics*, following the English translation by William H. Donahue.

Why focus on Kepler? Certainly, Kepler's awareness of experimental error has been noted before (Hon 1987, 2004). Likewise, Kepler's treatment of the eye as an instrument has been discussed earlier (Gal & Chen-Morris 2010a and b, 2013; Straker 1976). These latter authors, however, have considered Kepler's "instrumentalization" of the eye (the term is from Gal & Chen-Morris 2013) significant mainly because they think it heralds a modern world-view in which humans are alienated from nature. This epistemologically and metaphysically pessimistic interpretation draws its force from the assumption that the only general epistemological problem Kepler's theory is relevant to is the same problem that animated his Perspectivist predecessors, namely how it is that humans can have perceptions of the world that correspond to the way it truly is, or in other words how veridical perception is possible. On this assumption, Kepler's solution raises epistemological worries that the Perspectivists' did not, and so the epistemological balance seems negative.

In contrast, I will argue that there is a different, more positive sense in which the instrumentalization of the eye breaks with the Perspectivists. Kepler's mode of theorizing is relevant to a different problem than that of veridical perception, though one that is also quite general: how to achieve accurate measurements by means of vision. The conceptualization of the eye as an instrument is simply an appropriate way of dealing with this problem. The appropriateness of this conceptualization derives from the fact that measurement is not essentially anthropocentric, in the sense that the human perceptual apparatus is not required to produce a measurement. Below, I characterize measurement as involving *inter alia* (i) a physical interaction, which is used to (ii) selectively represent the entity in terms of the value of some physical parameter that characterizes the entity (iii) in order to obtain information about the entity. But in general, there is no reason why the physical process of interaction with, and representation of, the entity must be carried out by a human; an instrument can usually do it as well or better. On this view, measurement is inherently “instrumental” or “non-anthropocentric” in that the physical interaction and production of the representation that are at its core need not be carried out by a human or involve human perceptual faculties. What matters is whether there are sources of error in the process that create uncertainty in the measured value. My interpretation of the *Optics* is that Kepler understands measurement along these lines, and that his treatment of the eye as an instrument simply follows from its use in measurement.

Though I wrote above that measurement is inherently non-anthropocentric in the sense specified, it does not follow that it is *radically* non-anthropocentric. On the contrary, the measurement process is always tethered to the agents’ purpose of obtaining information, and its aim is to produce a representation that is conceived by the agents and serves their purposes. The commentators alluded to above have tended to see in Kepler’s optics a radically de-subjectivized theory, an interpretation that supports the negative morals drawn from it. Thus, in this discussion I will be contrasting two interpretations of Kepler’s optics, what I will call a ‘Naturalist Interpretation’ on the one hand, and a ‘Metrological Interpretation’ on the other. The Naturalist Interpretation holds that, in contrast with the tradition that preceded him, Kepler was responsible for naturalizing vision, in the sense of purging the theory of vision of any essential role for human agency. According to the Metrological Interpretation, Kepler subordinates his theorizing about vision to the requirements of accurate measurement, which does retain a role for human agency in the manner mentioned above.

Though Kepler is directly concerned only with astronomical measurement, I show that the benefit of conceptualizing the eye as an instrument has nothing specifically to do with astronomy, but rather with the characteristics of measurement in general. For this reason, I argue, Kepler’s theory of vision suggests an egalitarian attitude towards the means of observation and helped to prepare the conceptual ground for the instrumentalization of scientific observation.

The section is organized as follows. In subsection 3.3.2, I review the claims of some of the commentators mentioned above, focusing especially on Ofer Gal’s and Raz Chen-

Morris's claim that Kepler's work represents the "disappearance of the observer" in modern science and philosophy. Subsection 3.3.3 compares Kepler's and the Perspectivists' theories of vision. I introduce the distinction between the problem of certainty in measurement, as contrasted with the problem of certainty in perception, in subsection 3.3.4, and argue for the non-equivalence of the two. In subsection 3.3.5, I consider the problem of certainty in measurement in more detail, drawing on Pierre Duhem's classic analysis of criteria for the assessment of observation reports in physics. I also provide evidence that both measurement and the assessment of observation reports are a central concern in the *Optics*, and that "observation," for Kepler, is essentially accurate measurement. Subsection 3.3.6 shows how Kepler's treatment of the eye is geared towards supporting the certainty of measurement. I offer concluding remarks in subsection 3.3.7.

3.3.2 Gal and Chen-Morris on the "disappearing observer"

In *Baroque Science* (2013), Ofer Gal and Raz Chen-Morris make the provocative claim that "in the seventeenth century the human observer gradually disappears from optical treatises." They attribute this disappearance to the evolving understanding of the eye in optics:

> the observer disappears from optics *because of* the evolving understanding of the eye as a natural, material optical instrument. It is the naturalization of the eye that begets the estrangement of the human observer from nature. The naturalized eye no longer furnishes the observer with genuine re-presentations of visible objects. It is merely a screen, on which rests a blurry array of light stains, the effect of a purely causal process, devoid of any epistemological signification.[153]

According to Gal and Chen-Morris, then, it is the assimilation of the eye to the category of optical instruments, or what they call "radical instrumentalism,"[154] that "estranges" the observer from nature. In their view, the scientists most responsible for "disappearing" the observer are Kepler, Galileo and Descartes. The movement begins with

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[153] Gal & Chen-Morris (2013), pp. 15-16 (emphasis in original). See also Gal & Chen-Morris (2010a) and (2010b) for similar statements of their view.

[154] Gal & Chen-Morris (2010b).

Kepler: "The human observer starts slipping out of optics when Kepler turns his optical opus magnum, the *Ad Vitellionem paralipomena* [1604] to artificial observations."[155] The alleged disappearance of the observer in Kepler's optics was no minor event, they argue, for it eliminated "the import of optics as the epistemological anchor for all other sciences" by turning vision "into a mystery."[156] A few years later, Galileo, in his exchange with the Jesuit Horatio Grassi on the observation of comets, seems to deny any epistemic privilege of naked eye observation over instrument-mediated observation. Descartes brings this movement to maturity with his radical doubt (influenced by his own theory of vision).[157]

Gal and Chen-Morris (GCM henceforth) are not alone in viewing Kepler's optical work as introducing a source of epistemological anxiety. A. Mark Smith describes the anxiety in terms of a "problem of correspondence" between the physical causes of sight and their perceptual effects:

> By disjoining the physics of light from the psychology of sight, both domains subject to wholly different laws, Kepler brought the problem of correspondence into stark relief. *How can we be sure that our internal impressions of external objects match them in a meaningful way? Kepler's model of retinal imaging destroys all hope of establishing such certainty* because physical cause and perceptual effect are nothing like each other.[158]

According to Smith, there was no such problem for Kepler's perspectivist precursors, because "the perspectivist account was designed to ensure that the objective cause of vision, in the form of luminous color, corresponds to its subjective effects according to a rigidly interconnected succession of intentional species."[159] Thus, rather than focus on how the eye's relation to instrument-aided observation was conceived, as GCM do, Smith attributes the epistemological anxiety ushered in by Kepler to a

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[155] Gal & Chen-Morris (2013), p. 16.

[156] Gal & Chen-Morris (2013), pp. 24-26.

[157] Gal & Chen-Morris (2013), chs 1 and 3. They also cite Hooke as a later representative of radical instrumentalism.

[158] Smith (2015), p. 370. My emphasis.

[159] Smith (2015), p. 369.

discontinuity between the laws governing the differing domains of optical physics and visual psychology.[160]

Straker (1976) connects the discontinuity between the domains to a general anti-humanist tendency of the 17th century:

> Kepler has essentially replied [to the question how the retinal picture is cognitively processed and interpreted], 'That's not my department;' the philosophers will have to worry about that. We should recognize, however, that such disdain is one mark of the New Science of the 17th Century; one might as well ask Galilei, or, indeed, Newton, for the cause of gravity as to try to insist that Kepler *tell us the nature of visual perception from a human perspective*. …. *Both [Kepler and the artists he emulated] are estranged from the actual having of a visual experience*, and as a result of that estrangement, we find encouraged a visualisation of seeing in which, paradoxically, the seer is seen as a passive, optical receptacle. The soul, which in an earlier view actively participated in the seeing of things that are seen, *is now pushed back behind the eye where it resides in severe danger of absolute eviction*.[161]

The historian Vasco Ronchi doubted that such an apparently de-subjectivized theory could still be called 'optics:'

> It is impossible to go on studying a science which completely abstracts from human eyes, and still call it optics: this would be optics also valid for blind men. What happens *in front of the retina* is not optics, but just physics.[162]

Ultimately, such anxiety may be traceable back to E. A. Burtt's thesis that the primary/secondary quality distinction of the seventeenth century scientific revolutionaries introduced a new set of worries about humanity's metaphysical and epistemological situation. For example, writing of Galileo's version of the distinction, which he views as a radicalization and making explicit of Kepler's, Burtt claims that

> *It is a fundamental step toward that banishing of man from the great world of nature* and his treatment as an effect of what happens in the latter, which has been a pretty constant feature of the philosophy of modern science, *but bringing in its train the big metaphysical and especially epistemological problems of modern philosophy.* Till the time of Galileo it had always been taken

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[160] For a dissenting view on whether Kepler himself thought there was such a discontinuity between the two domains, see Baker (2016).

[161] Straker (1976), p. 21. My emphasis.

[162] Ronchi (1963), p. 622.

> for granted that man and nature were both integral parts of a larger whole, in which man's place was the more fundamental.[163]

The elimination of any priority of man in nature, and the treatment of man as a natural effect, is echoed in GCM's claims about the disappearance of the observer accompanying Kepler's alleged naturalization of the eye.

3.3.3 Kepler and the perspectivists

Since in this paper we are concerned with the nature of scientific observation as Kepler understands it, I will focus here on GCM's assessment of the epistemological consequences of Kepler's optical work. What exactly is the "mystery" into which vision has been turned at Kepler's hands? In the *Optics,* Kepler holds that images of visible objects are generated when light rays originating from the objects strike an opaque screen:

> let us embrace the true opinion described in this chapter, and established by irrefutable examples [*experimenta*]: that from the sun, and from the colors illuminated by the sun, there flow out forms [*species*] similar to each other; and that in this flow itself they are diluted, until they strike upon a medium that is in some proportion opaque, and there they represent their source; and that vision occurs … when the opaque wall of the eye is colored in this manner, the vision being confused when the images of different colors are intermingled, and distinct when they are not intermingled.[164]

Thus, on Kepler's theory the perceiver's access to the world is mediated by the image, which he calls a "painting" (*pictura*) created on the retina (the "opaque wall of the eye") by the illumination of light rays.

This is in contrast to the perspectivist view. According to this view, light (called *lux*) and color are formal characteristics of the surfaces of bodies. Vision occurs because these characteristics are propagated as physical species from the surfaces of visible objects.

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[163] Burtt (1932), p. 78. See Daston (1991) for a retrospective overview of Burtt's legacy.

[164] Kepler (2000), p. 48. "*Amplectamur ergo veram sententiam hoc capite descriptam, et irrefutabilibus experimentis stabilitam, à Sole scilicet, et à coloribus Sole illustratis, defluere species consimiles, ipsoque fluxu attenuari, donec in medium quacunque ratione opacum incidant, ibique suum fontem depingant : fierique visionem ... cum opacus oculi paries hoc modo pingitur, confusam, cum confunduntur ibi picturae variorum colorum, distinctam, cum non confunduntur.*" Kepler (1939), pp. 41-42, Kepler (1604), p. 33 (italics in original).

They are received in the lens of the eye and replicated there as a punctiform visible species. The replication is ensured by visual spirits produced by the brain and infusing the lens, which spirits sense the species emanating from the object in a pointwise fashion. This visible species is then transmitted to the optical nerve and thence to the common sense and imagination, where it is replicated more abstractly as a sensible species. The reasoning faculty then replicates the latter yet more abstractly as an intelligible species, according to the conceptual core of the object.[165] The overall progression is from a brute sensory representation in the eye to an increasingly abstract and conceptual representation in the mind. This process was very important from the point of view of Aristotelian epistemology, since all our concepts and the first principles of science were supposed to be acquired through the abstraction of concepts and principles from experience, following Aristotle's account in the *Posterior Analytics*. This was how the major Perspectivist authors, Roger Bacon (ca. 1214-1292), John Pecham (ca. 1230-1292) and Witelo (ca. 1230-after 1280) conceived the process. All three followed closely the account of Arab scholar Ibn Al-Haytham (965?-1040/41), or "Alhacen" in Latin. For example, A. Mark Smith gives the following account of Bacon's explanation of visual sensation:

> … by adopting the Alhacenian model of punctiform radiation, Bacon can explain the act of visual sensation in essentially the same way as Alhacen. Each point of *lux* [a formal characteristic of objects that makes them luminous] and illuminated color on a visible object's surface multiplies its species radially to every exposed point on the surface of a facing eye, and thence to every exposed point on the lens's anterior surface. Likewise all points of *lux* and color on the visible object's surface radiate their species to each point on the eye's surface and thence to a single exposed point on the lens's anterior surface.
>
> The result is utter confusion on the lens's surface, but because of its sensitivity, the lens makes coherent sense of this confusion by selecting only the species that reach it orthogonally, since they make the strongest impression. The rest, having reached it along weaker, oblique paths, are ignored. All the individual impressions selected in this way comprise a pointillist representation of the object's surface that constitutes what we (not Bacon) might call the visible species. Each impressed species at a given point within this composite visible species multiplies straight through the glacial humor toward the center of sight within a cone of radiation based on the object surface. Refracted at the interface between glacial and vitreous humors, the visible species multiplies in proper upright and left-to-right order into the hollow optic nerve and thence to the optic chiasma, or

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[165] Smith (2015), p. 278.

> "common nerve" (*nervus communis*), where it is fused with its counterpart from the other eye. It is there, as well, that the final sensor (*ultimum sentiens*) apprehends the species to complete the sensitive phase of vision.[166]

This process is illustrated in Figure 3.1a:

(a) (b)

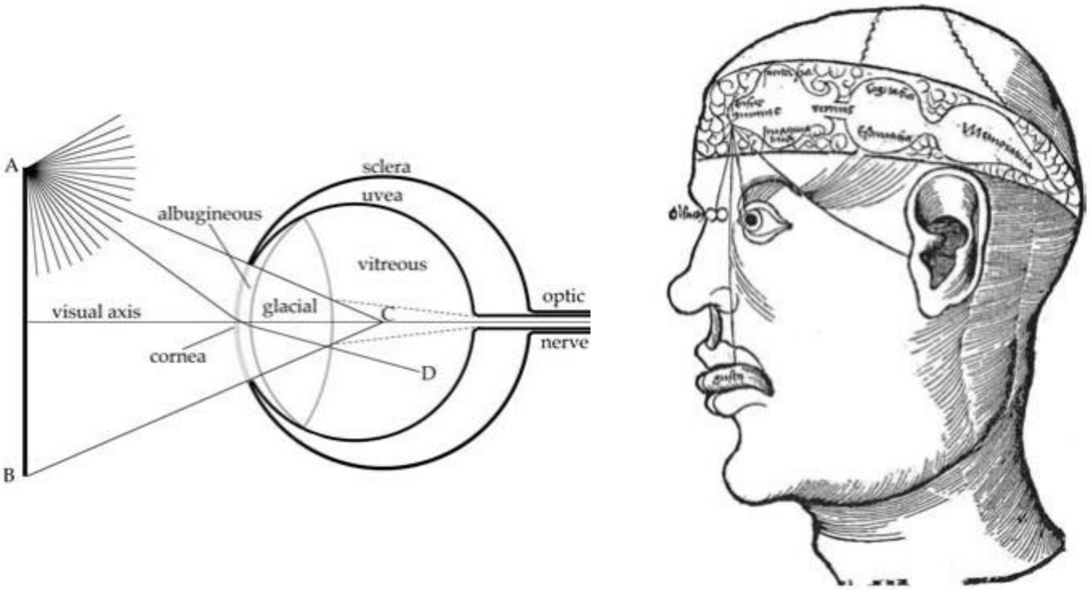

Figure 3.1 (a) A. Mark Smith's reconstruction of the process of visual sensation, according to the Perspectivists. Smith (2015), p. 186. (b) A woodcut of 1503 from Gregor Reisch's account of psychology and cognition in the Margarita Philosophica ("Philosophical Pearl"). The woodcut illustrates how the process of visual sensation was, for the Perspectivists and their followers, connected with the process of cognition.

Since my focus is on what happens in the eye, the "replication" carried out by the lens deserves some comment. The interaction between the lens and the species of *lux* and color was conceived by analogy with physical contact, in that the lens would "feel" the impingement of the species striking it the way one would feel a physical blow. Stronger impressions would last longer as afterimages, and could even be painful, as for example looking at sunlight creates a long-lasting afterimage and causes pain.[167] Witelo is clear on this point:

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[166] Smith (2015), p. 264.

[167] Smith (2015), p. 188.

> Vision does not take place without pain (*dolore*) and suffering (*passione*) endured by the substance of the eye. From which it is clear that the eye ought to be of an adequate disposition in [its] health in order to prosecute completely the [process of] vision.
>
> Indeed since the glacial receives the form of light and color, and light and color toil in the glacial, that work will necessarily be not without pain, however often that pain may not be felt, simply because it is not strong enough. In truth strong lights narrow the [pupil of the] eye and hurt (*ledunt*) the same manifestly, as is clear in the sun's light, or in light reflected by polished bodies to the eye.[168]

Perpendicular impressions would be stronger than oblique ones, allowing the visual spirits to distinguish between them and select only the former, thus ensuring the coherence of the representation entering the eye. The lens also maintained the relative spatial positions of the entering rays. As far as I can tell, the selection results in the *passage* of the selected species into the eye. For example, Ibn al-Haytham claimed that "the sentient member [the lens] receives and feels these forms; and they pass through it according to both its transparency and its sensitive virtue."[169] The physical species are therefore "replicated" in the sense that a certain subset of them are allowed to pass through the eye such that their relative positions, and thus the coherence of the image, are maintained.

Besides assuring coherence, the lens had another function, this one associated with the visual cone model that originated with extramissionist accounts of vision. As implied in the preceding discussion, the "classical" Perspectivists were intromissionists, i.e., on their account of vision the latter occurs by means of rays entering the eye rather than emitted by it. Euclid and Ptolemy, on the other hand, had theorized vision in terms of a visual cone of emitted rays, with its vertex in the eye and its base defining the field of view. The Perspectivists followed Ibn al-Haytham, who flipped the visual cone from

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[168] Unguru (1991), p. 122.

"*Visio non fit sine dolore et passione a substantia oculi abiciente. Ex quo patet visum oportere convenientis dispositionis in sanitate esse ad hoc ut complete exerceat visionem.*

Quoniam enim glacialis recipit formam lucis et coloris, et lux et color operantur in glacialem, erit necessario illa operatio non sine dolore, quamvis quandoque non sentiatur ille dolor, ut cum non est valde fortis. Luces vero fortes angustiant visum et ledunt ipsum manifeste, ut patet in luce solis, vel in luce reflexa a corporibus tersis ad visum" [Unguru (1991), p. 310 (*Perspectiva* III.16)]. According to Smith (2015), p. 273, Witelo's *Perspectiva* was completed ca. 1275 CE.

[169] *De Aspectibus* II 1., Prop. 4, p. 26. Quoted in Smith (1981), p. 582.

extramission to intromission. According to his theory, the intromitted rays do not actually meet at the vertex, because they are refracted at the interface of the lens and vitreous humor so as to be funneled into the optic nerve, which was thought to be hollow. Yet even though the rays do not meet, they are felt by the lens, which posits them meeting virtually in the middle of the eye. This virtual "center of sight" (*centrum visis*) was "the viewpoint from which everything encompassed by the cone of radiation is perceptually judged according to its spatial characteristics and location."[170] By attributing this further role to the visual spirits, the Perspectivists were able to secure a good deal of the explanatory framework of the visual cone model.

It is worth pointing out that this theory puts a heavy burden on the visual faculty to interpret the impressions correctly. Each point of the eye is being simultaneously struck by oblique rays from every point in the field of vision, including peripheral objects outside the cone of vision. The lens was thought to have an active power of selection, and authors like Bacon talked about the visual spirits making active judgments: "the eye [itself] necessarily makes judgments and has the power of sight, though incompletely."[171] Witelo distinguishes between two kinds of vision, one of which appears to involve active judgment by the eye:

> First we call *simple sight* that act by means of which the form of the visible object is received for the first time directly in the surface of the eye, while we call *intuition* that act by means of which the eye inquires diligently and thoroughly after the comprehension of the form of the object, not [being] content with the mere reception but [striving for] a profound examination.[172]

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[170] Smith (2015), p. 188.

[171] Lindberg (1996), p. 67. "*[O]culus necessario habet iudicium et virtutem videndi, licet incompletum*." (Bacon, *Perspectiva* I.5.3). This passage will be discussed more in section 3.3.6.

[172] "*Aspectum primum simplicem dicimus illum actum quo primo simpliciter recipitur in oculi superficie forma rei vise, intuitionem vero dicimus illum actum quo visus veram comprehensionem forme rei diligenter perspiciendo perquirit, non contentus simplici receptione sed profunda indagine*" [Unguru (1991), p. 351; *Perspectiva* III. 51]. I wrote that this quotation *appears* to attribute the power of judgment to the eye because a few paragraphs later Witelo invokes the "observer" (*videns*), and not just the eye, as the subject of intuition: "And so if the observer will have wanted to obtain a certification concerning the form of the whole visible object, he will have moved both eyes until their centres may be opposite to any parts or points of the surface of the

Importantly, a dead eye—one separated from its owner or belonging to a deceased individual—would not produce a representation. Veridical perception was guaranteed by the properties of the visual spirits. These properties were themselves inferred from the needs of human cognition, according to Aristotelian epistemology. So this theory of vision was essentially teleological in conception.

Both GCM and Smith insist that the visual process, according to the perspectivists, is supposed to yield a veridical representation of external objects provided that they are viewed under appropriate normative conditions (adequate lighting, appropriate distance from the object, the eye is healthy, etc.)[173] and that the reasoning faculty can rectify visual deceptions arising from reflection and refraction. This veridicality derives from the fact that there is supposed to be a point-to-point replication of the object's surface at each stage of the process.[174] There was, as GCM put it, "a particularly privileged relation between between source and image" in perspectivist optics. Hence it is possible to find strong perspectivist assertions like the following one from John Pecham that GCM are fond of quoting: "vision takes place by the arrangement of the species on [the surface of] the glacial humour [i.e., the surface of the lens] *exactly* as [the parts] of the object [are arranged] outside … *unless* this were so, the eye would not see the object distinctly."[175] Though this

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visible thing opposed to it, and then, since both radial axes will be incident to any of the points, by the 32nd [prop.] of this [book], the complete intuition of the entire form will be achieved in this manner." It is therefore ambiguous whether the eye or the entire cognitive apparatus is intuiting the form. The original reads: "*Si itaque videns voluerit certificari de forma totali rei vise, movebit ambos visus donec medium eius opponatur cuilibet partium vel punctorum superficiei rei vise sibi opposite et tunc, quia ambos axes radiales, per 32 huius, incident unicuique punctorum, fiet hoc modo intuitio completa totius forme*."

[173] According to Smith (2015), pp. 192-193, Alhacen adumbrated eight conditions for normative viewing: that there be adequate distance between eye and object, that the object face the eye directly enough to be properly seen, that there be adequate illumination, that the object be of an adequate size, that it be adequately opaque, that the intervening medium be adequately transparent, that there be adequate time for proper perception, and that the eye be adequately healthy.

[174] Smith (2015), pp. 369-370; Gal & Chen-Morris (2013), pp. 20-24.

[175] Quoted in Gal & Chen-Morris (2013), p. 22, (2010a), p. 198 and (2010b), pp. 137-138. Italics added by GCM. It is possible that the translation, by David Lindberg, that GCM are using

translation may be misleading as to the degree of correspondence asserted by Pecham (see footnote 187), it supports GCM's case by getting across both (i) the correspondence of the visible species to the visible object and (ii) a teleological element in the perspectivist explanation of vision (in Pecham's case, that the correspondence is explained by the requirement of distinct perception) that GCM emphasize.[176]

Kepler's theory of retinal image formation is illustrated in Figure 3.2a. The globe represents the lens, and the semi-oval represents the retina. The diagram is slightly distorted in that the real lens is not completely spherical but is hyperboloidal in the back, and the retina is more or less spherical in shape. These distortions, however, don't affect the basic mechanism of vision. I use the word "mechanism" knowingly, for there is no role for visual spirits in image formation here. The rays from points in the visual field enter through the pupil and then are refracted twice by the lens in such a way as to focus on the retina. The focusing of the rays at points on the retina creates an inverted picture of the visual field. The following points are worth noting. First, the production of the picture is "mechanical," in that it depends entirely on the organization of the material parts. There is no active power of selection or judgment operating here. Second, and famously, Kepler has no account of how the picture thus formed is then perceived and judged by the cognitive faculties. Third, a dead eye is just as effective as a living eye in producing the retinal image. This follows from its mechanical operation.

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exaggerates the degree of correspondence Pecham is asserting for the visible species. The original reads: "*Visionem fieri per hoc, quod in glaciali est ordinatio speciei sicut exterius rei ... quod nisi fieret oculus rem distincte non videret*" (*Perspectiva communis* I.37^{a}{40^{a}}). I am told that the Latin word *sicut* corresponding to 'exactly' in the translation is best translated 'just as' or 'like' or even perhaps 'in some way' (personal communication from Dr Tawrin Baker, Department of History and Philosophy of Science, University of Pittsburgh). See Pecham (1970), pp. 120-1.

[176] E.g., Gal & Chen-Morris (2013), p. 21; (2010a), pp. 193, 196-7, 199, 204, 206.

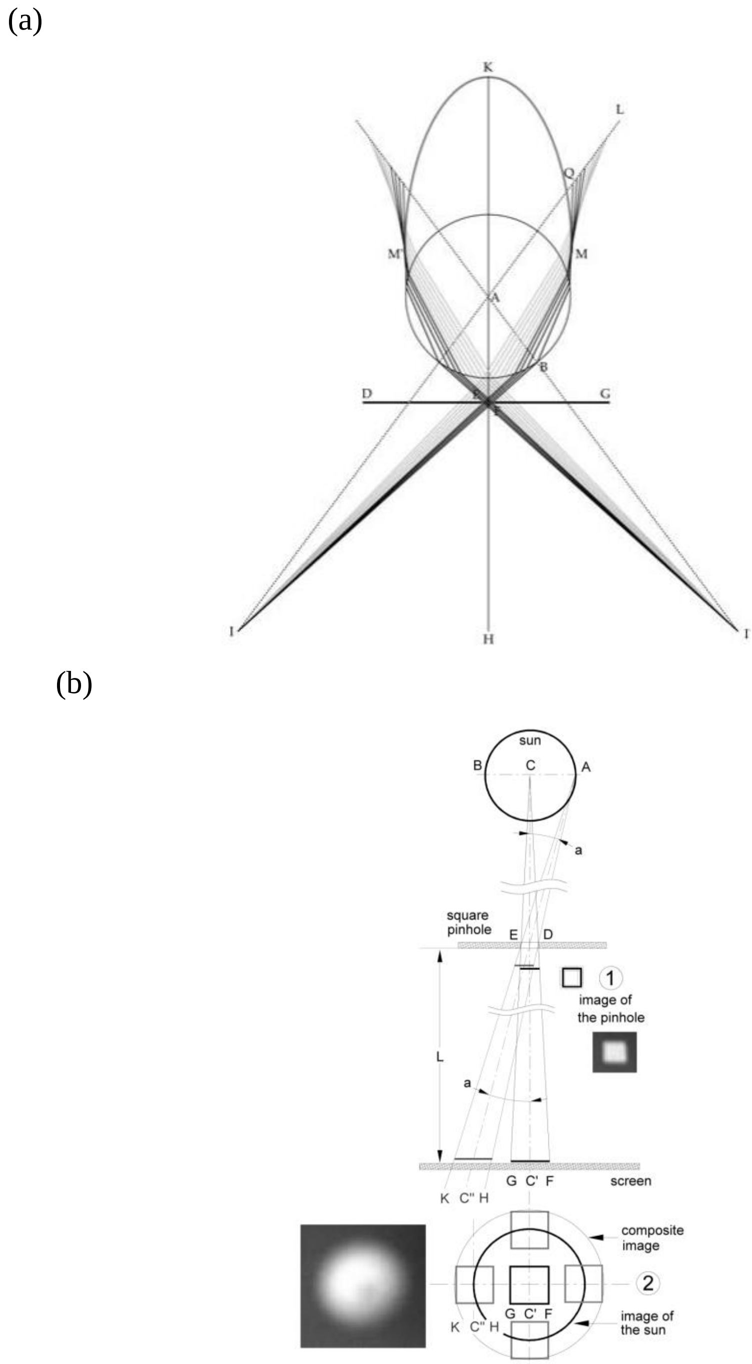

Figure 3.2 (a) A. Mark Smith's reconstruction of the process of visual sensation, according to Kepler. Source: Smith (2015), p. 362. (b) An analysis of image formation in a pinhole camera by Giora Hon and Yaakov Zik. Note how the image of the sun (bottom right) results from a combination of images of the pinhole. This process leaves a penumbra of width KC" around the true image of the sun. Source: Giora Hon, Yaakov Zik, 'Kepler's Optical Part of Astronomy (1604): Introducing the Ecliptic Instrument', Perspectives on Science, 17:3 (Fall, 2009), pp. 307-345. © 2009 by the Massachusetts Institute of Technology.

In contrast, the correspondence of Kepler's retinal picture to the object it represents is subject to two sources of doubt. First, the picture is not the result of the transmission of intentional species from the object but rather the result of light rays bouncing off the object and landing on a screen. So whether or not the resulting picture corresponds to the object depends entirely on the paths followed by the rays and on the mechanism by which they enter the eye and are projected onto the retina. There is no guarantee of resemblance to the object; for example, in the *camera obscura*, to which Kepler compares the eye,[177] "the projected image consists of an infinite number of overlapping images of the aperture that combine to take the shape of the luminous object"[178] (Figure 3.2b). Whether or not the images are combined in such a way as to take the shape of the object depends on structural features of the eye or instrument. Defects in these features may cause errors of vision, such as image distortions or multiplications. For the perspectivists, on the other hand, the resemblance of the visible species to the object was due entirely to the sensitivity of the visual spirits, not to the structure of the eye. So long as there was an adequate flow of visual spirits from the brain to the eye, and so long as the lens was undamaged, this sensitivity guaranteed that the physical species impinging on the lens passed through as a coherent, point-to-point representation of the object. Second, once the picture is produced on the retina, Kepler has no account of how the intellect manages to judge the image and thereby perceive the object of which it is the image. The incompleteness of the theory is made more bothersome by the fact that the picture on the retina turns out to be an inversion of the real object.

That said, it is possible that Smith and GCM exaggerate the difference in degree of certainty between the perspectivist and Keplerian accounts. As noted above, the interaction between the lens and the species radiated by the object of vision yields a punctiform, two-dimensional representation of the object. Arguably, the perspectivists would have had to include an inference from this representation produced in the lens to the object itself, and therefore would involve an "interpretative leap" analogous to that required by Kepler's

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[177] E.g., Kepler (2000), p. 184.

[178] Smith (2015), p. 353 and Kepler (2000), chapter 2.

account.[179] Is the argument of Smith or GCM that the mechanism of species transmission simply allowed for fewer ways in which the inference could go wrong? Or is it that the species accounting for each point bore a likeness to the point of origin in the object? One might contrast the latter case with the image in the *camera obscura*. There, though the resulting image resembles the projecting object, each individual patch of light resembles the aperture. There is thus no necessary likeness between the elements of the image and the elements on the surface of the object. It is simply a matter of chance, resulting from the nature of light together with the conditions of the chamber, that the light rays happen to combine in such a way as to constitute an image resembling the object. Perhaps this is the specific source of uncertainty affecting Kepler's theory.

If despite the reservation broached at the beginning of the last paragraph we grant the Smith/GCM view that perception was more subject to doubt on the Keplerian account of the eye than the perspectivist, it is well to recall here that Kepler sets out to investigate optics in order to enable astronomers to make "technically-sound" observations (*artificiosa observationes*) of celestial objects. Given these sources of doubt, however, how can we trust images of these objects produced by light? According to GCM,

> we can trust images, whether on the pavement or on the retina, whether far away or nearby, because they are outcomes of a purely natural, causal process that we can investigate through experimenting and theorizing. This means that we can trust observations of stars as much as those of books, and we can trust instrumental, artificial observations as much as we trust our eyes.[180]

It is unclear what GCM take to be the contrasting case here, but presumably the species account of vision, with its role for judgment and its teleological element, is not a "purely natural, causal process." In any case, after Kepler it seems that the trustworthiness of vision or of instrumental observation now depends on the state of science itself, on our "experimenting and theorizing." Some would see this development as grounds for optimism, since observation itself is now amenable to empirical inquiry and presumably improvement resulting therefrom. But GCM emphasize that the Kepler's account of vision comes at "a steep epistemological price:" "if the instrument is not prone to error more than

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[179] Smith, (2015), p. 369.

[180] Gal & Chen-Morris (2013, p. 24.

the eye, it is immediately implied that the eye is as vulnerable to error as the instrument."[181] This vulnerability affects the very structure of vision, on which they quote Kepler: "for it has been demonstrated most clearly, *from the very structure* [conformatione] *of vision,* that it frequently happens, that an error befalls the sense of vision."[182]

Surely, Kepler's predecessors were aware of the existence of visual errors? GCM admit that "[v]isual errors are of course nothing new" but maintain that "[i]n the Aristotelian paradigm, errors are created by the intervention of the human imagination; the visual data are indubitable."[183] This indubitability of the image is supposed to contrast with Kepler's theory, where the perceived images themselves are subject to the two doubts mentioned above. Since the process of image formation no longer guarantees the reliability of the resulting image, the optical scientist now needs to provide a *demonstration* that the pattern on the screen (of the retinal wall, the camera obscura, etc.) does indeed correspond to the projected object. She cannot hope to escape doubt by means of this stratagem, however, for "[t]he real doubt does not arise from the possibility of error but from the need for such demonstration, and is all the more devastating for that."[184]

How did Kepler get us into this epistemological bind? He is certainly no skeptic; on the contrary, "Kepler's optics is as much epistemologically oriented as traditional optics." Indeed, this situation results from Kepler's effort to put astronomical observation on a more secure footing:

> instead of guaranteeing the veridicality of our *visual knowledge in general*, it aims at supporting the empirical underpinning of his new astronomy, and of long-distance *instrumental observation in particular*. More crucially, it fulfills this *immediate task* at the expense of the *general epistemological assuredness* provided by traditional optics.[185]

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[181] Gal & Chen-Morris (2013), p. 25.

[182] Gal & Chen-Morris (2013), p. 25, italics added by the authors; Kepler (2000), p. 236. "*Demonstratum enim est euidentissimè, ex ipsa visus conformatione, fieri crebrò, ut visui error accidat, dum lucida nimis magna existimat*." Kepler (1939), p. 197; (1604), p. 221.

[183] I will say more about the causes of error, according to the perspectivists, in section 3.3.6, with reference to Roger Bacon's treatment in the *Perspectiva.*

[184] Gal & Chen-Morris (2013), p. 25.

[185] Gal & Chen-Morris (2013), p. 24. My emphasis.

In other words, Kepler has traded general epistemological “assuredness” for a more specific kind of epistemological assuredness. In trying to solve the problem of how to observe distant objects, Kepler has opened the door to skeptical worries about observation in general. Kepler has won a narrow scientific victory at the cost of a general epistemic defeat.

This defeat arises from Kepler’s egalitarian treatment of eye and instrument as species of “natural, causal processes.” In the following sections, I will argue that GCM unfairly minimize the epistemological pay-off of Kepler’s instrumentalization of the eye because they underappreciate the scope of the scientific problem he is dealing with. In the course of the discussion, I will show that the problems Kepler is concerned with would affect the perspectivists also, and indeed would be problems on *any* theory of vision. So his solutions to them are “general” too, in a sense.

3.3.4 Error, perception and measurement

For the moment, I note that the notion of “error” is underspecified in their analysis. There are different kinds of error that can affect observation, depending on the context. Ordinary observation, for example, can be misled (in extreme cases) by hallucination, a fact which gives rise to traditional skeptical worries about the possibility of our perceptions being manipulated by evil demons and the like. It can also be misled by non-normative viewing conditions (adequate lighting, appropriate distance from the object, ill-health of the eye, etc.). This is not the kind of error, however, that is of concern to scientists. ‘Error’ has a different meaning for scientists, a meaning that is quite general and that cannot be reduced to the ‘error’ of ordinary observation. When scientists speak of error, they usually have in mind the kinds of error that affect measurement. There are three basic kinds. First, there is non-systematic, random measurement error associated with vagaries of observation and vagaries of the environment, beyond those we can control for, that yield a residue of imprecision and uncertainty in our measurements. Then, there is systematic measurement error associated with inadequate schematic models of our instruments and observation procedures, which yield biased error in interpreted experimental results. Finally, there is "real" error—true physical deviations from the predictions of our theories. Thus the distinction between ordinary observation and *scientific* observation (at least, scientific observation of the kind relevant to measurement-based sciences like astronomy) reveals *two* general epistemological problems concerning observation:

1. how to achieve certainty in our perceptions; and
2. how to achieve certainty in our measurements.

The two problems are not equivalent. From an empirical perspective, there is much measurement in science that does not depend on perception. For an extreme example, sophisticated experiments in contemporary physics (e.g., the Large Hadron Collider, LIGO, unmanned space probes to other planets, etc.) rely on "observations" carried out by instrumentation that, besides being highly automated, interacts with the measured entities in ways that are impossible for human perceivers. The only role for human perception here is reading the processed results off computer screens and print-outs. From a philosophical perspective, I do not think there is a good *a priori* reason to think that in general, the solutions to (2) will be equivalent to, or even harmonize with, the solutions to (1).

How is this lack of dependence possible? As suggested in the previous paragraph, I do not think "instrumental observation" is necessarily a kind of visual or perceptual observation. Vision need not enter into the acquisition of instrument-mediated knowledge at all, except in the mundane sense of reading processed results. That is to say that vision need not enter in a way that would make the 'error' to which ordinary observation is prone relevant to the quality of the instrument-mediated knowledge. Instrument-mediated knowledge of the kind of interest to Kepler is acquired through *measurement*, and as such is acquired by different methods, and is subject to different criteria of certainty and error, than the methods used to acquire ordinary visual knowledge. In short, these seem to be different kinds of activity altogether. GCM may be committing a category mistake by assuming that instrumental observation is a species of visual observation.

More precisely, the mistake would consist in thinking that measurement is related to vision as a particular kind to a more general category. On the contrary, I claim that they belong to different categories of activity:

a. Vision belongs to the category of ordinary sensory observation, which has its own criteria of certainty and error.

b. Measurement, on the other hand, is *a process of material representation*, which has different criteria of certainty and error.

I will elaborate on (b) in the next paragraph, but for the moment I will point out that I am not claiming that ordinary observation is irrelevant to measurement. I grant that ordinary observation is indispensable to our *use* of the results of measurement. But I do not grant that it is essential to the *production* of the results themselves.

In order to make sense of the idea that measurement is a process of material representation, I will provide the following approximate characterization of measurement.[186] A measurement is:

i. A physical interaction with an entity, which is used to
ii. selectively represent the entity in terms of the value of some physical parameter that characterizes the entity
iii. in order to obtain information about the entity.

This representation is itself material, generally taking the form of locating the entity on a scale. With this clarification of the nature of measurement, we can now see why it is that much measurement in science does not depend on human perceptual faculties: in general, the physical process of interaction with, and representation of, the entity can be carried out by an instrument as well as a human. On this view, measurement is inherently "instrumental" or "non-anthropocentric" in that the physical interaction and production of the representation that are at its core need not be carried out by a human or involve human perceptual faculties.

Accurate measurement is therefore facilitated (at least) by knowledge of how the process works and of what features of the process may introduce errors in the representation. This knowledge must be acquired by inquiry into the nature of the process. Measurement has a modal aspect, because it presupposes a representation of what could happen—that the physical parameter could take on any of the values represented on the scale. This modal aspect distinguishes it from ordinary observation: In ordinary observation, we do not need to locate the object on a previously recognized space of possible values, because under normal conditions, we directly observe the actual state of

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[186] The characterization is loosely based on van Fraassen (2008), pp. 179-180.

the object. The main question is whether those conditions obtain. Moreover, we observe its actual state in relation to ourselves. This is what Currie (1995) calls the perspectival aspect of seeing: "from what perspective I see things depends on the location of my body—or at least of my eyes—relative to the things I see."[187] Whereas measurement provides information relative to the space of possible outcomes built into the instrument, seeing provides what Currie calls "egocentric" information, information about an actual state of affairs relative to the perceiver's state "here and now." For example, it is easy for a viewer to make small mistakes about the distances and directions of objects relative to him or her. It is even possible to makes mistakes concerning temporal egocentric information; for example, for astronomical distances we are prone to think that what we see tells us how the seen object is now, because we do not account for the time required for light to travel from the object to us.

Currie contrasts egocentric information with the information provided by photographs. These do not convey egocentric information, because seeing a photograph does not provide the viewer with information about where the object photographed is in relation to the viewer.[188] Since photographic information is not egocentric, the types of discriminatory errors characteristic of vision do not apply to the seeing of photographs. I submit that *a fortiori*, there is even less overlap between vision and measurement.

It follows that the considerations that come into play in evaluating the certainty of measurements are not the same as those that are relevant in evaluating the certainty of our perceptions. Because measurement is a process of material representation, it suffers from

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[187] Currie (1995), p. 66.

[188] Of course, when combined with information from other sources, photographs can provide information for an *inference* to egocentric information. If the viewer knows where and when the photograph was taken, where he or she is now, and what time it is now, the viewer may infer that the photographed scene stands in a certain spatiotemporal relation to his or her time-slice. For example, if I see a photograph of a May 1st parade in Paris taken in 2017, when I am in Pittsburgh on May 1st, 2018, and was there also on May 1st, 2017, I can infer that the event depicted took place a year ago and several thousand kilometers away from where I am now and where I was then. But the egocentric information available in ordinary seeing is not obtained by inference from the visual experience together with other information. Egocentric information can be obtained by non-perceptual paths.

errors that arise from the contributions of the process to the result, which may cause the latter to deviate from the true value. Certainty can be approached, but only *a posteriori*, by identifying those contributions and accounting for the difference they make to the result.

My claim is that this understanding of error and certainty guide Kepler's theorizing in the *Optics*. At this point, the difference between the perspectivists and Kepler concerning vision may be put thus. For the perspectivists, the key epistemological problem was: how is it that humans can have perceptions of the world that correspond to the way it truly is, or in other words how is veridical perception possible? Their theory was designed to explain this correspondence. Kepler's conceptualization of the eye as an instrument is aimed at a different problem than veridical perception: how to achieve accurate measurement by means of vision. If this interpretation of Kepler is correct, then one would expect Kepler's theorizing to have the following characteristics:

1. That the errors Kepler is interested in are errors specific to measurement.
2. That at least some of the specific sources of error dealt with in the *Optics* would be neutral with respect to differences between theories of vision. This neutrality arises from the fact that sources of error will inevitably be located in sites external to the human perceptual apparatus, in the experimental set-up, for instance, or in relational features of the experiment.
3. That certainty is achieved not by an *a priori* guarantee of correspondence between the world and our perceptions of it, but *a posteriori*, by identifying and correcting for sources of error. Perspectivist theory provided such an *a priori* guarantee because according to it, the visual spirits ensured a faithful representation of the visual field, and the mind had direct access to this representation.

In the next two subsections, I will argue that these three "predictions" are borne out by the *Optics*. Kepler displays a keen interest in systematic error; he deals with problems that are neutral between theories of vision; and he identifies and corrects for sources of error by focusing research on causes of error built into the measurement process.

3.3.5 The problem of the certainty of measurement

I will begin with a brief philosophical excursus. I am not the first to note the difference between the certainty of ordinary observation and that of scientific observation. For example, Pierre Duhem, writing at the beginning of the 20th century, observed that

> [a]n experiment in physics being quite another matter than the mere observation of a fact, there is no difficulty in conceiving the certainty of an experimental result to be of quite another order than that of a fact merely observed by the senses. It is similarly understandable that these certainties of such different sorts should become known by entirely distinct methods.[189]

The difference between an experiment in physics and "the mere observation of a fact," according to Duhem, is that observation (in the sense of the sensory observation of the experimental apparatus) in physics is always accompanied by an interpretation of what has been observed. So in analyzing the methods by which the two certainties are known, Duhem focuses on testimony, the linguistic expression of that interpretation. Duhem goes on to describe differences in the criteria by which testimony of ordinary observations on the one hand, and scientific observation reports based on performed experiments on the other, should be evaluated. In order to evaluate the former, it suffices to know whether the observer is sincere, "sound enough in mind not to confuse the play of his imagination with perceptions," and sufficiently knowledgeable of the language she uses to express her thought clearly. If the three criteria are satisfied, then when the observer "says he has observed a fact, the fact is certain." In Duhem's example, if the observer reports having seen a white horse in a certain street at a certain date and time, then dishonesty, hallucination, memory etc. might leave room for doubt. But if he satisfies the criteria, then there is little room for serious questioning.

In contrast, experimental scientists are rarely questioned on grounds of honesty or veridical perception. The relevant criticism of scientific observational reports involves

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[189] Duhem (1982 [1906]), p. 158. A few decades later, Gaston Bachelard famously maintained that there was a break between the methods of *la connaissance commune* and those of modern science (e.g., Bachelard (1949), ch. VI). Though the reference to Duhem might seem dated to some readers, it has the virtue of providing a clear exposition of differences between how certainty is achieved in ordinary observation and how it is achieved in measurement.

criticisms of interpretation. Duhem identifies four steps in the critical analysis of scientific observation reports. First, identify the theories employed by the scientist to interpret the experiment. Second, assess whether the theories are employed properly in the interpretation. Third, assess the relationship between the idealized experimental set-up, as reported, say, in a schema of the experiment, and the actual experimental set-up, including the instruments employed. Fourth, assess the degree of approximation involved in the interpretation of the result.[190]

The third step is the most important for understanding Kepler's approach to observation in the *Optics*, though the first and second are also relevant. One of the main points of assessing the relationship between the idealized experiment and the actual experiment is to determine whether all the important causes of systematic measurement error have been eliminated and all the desirable corrections made. Throughout the *Optics*, Kepler displays a deep concern with identifying sources of systematic error in visual observation and correcting for them.[191] Indeed, there is reason to think that Kepler conceives of observation itself as being distinguished from mere perception in virtue of involving safeguards against error, as the following quotation from the Conclusion suggests: "*up to this point we have dealt with the deceptions of vision, and, treating the subject through the procedures and precautions of observing, we have brought it down pretty much within the limits of books 4, 5, and 6 of Ptolemy's Almagest.*"[192] Here observation is distinguished from vision in virtue of involving "procedures and precautions."

On this score, I note in passing that there is some ambiguity in the meaning of Kepler's use of the term *artificiosa observatione* in the subtitle of the *Optics*. The part of the subtitle in which the term appears is "*DE ARTIFICIOSA OBSERVATIONE ET ÆSTIMATIONE DIAMETRORUM deliquiorumq́; Solis & Lunæ*" which Donahue translates as "on the Technically Sound Observation and Evaluation of the Diameters and

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[190] Duhem (1982), pp. 158-163.

[191] Hon (1987) discusses Kepler's awareness of experimental error in the *Paralipomena.*

[192] Kepler (2000), p. 432. "*Hactenus itaque de visus deceptionibus egimus; remque per obseruandi modos et cautiones traductam, penè intra limites libri* 4. 5. *et* 6. *in Opere Magno PTOLEMAEI deuoluimus.*" Kepler (1939), p. 378; (1604), p. 449.

Eclipses of the Sun and Moon." GCM, on the other hand, consistently translate the term as "artificial observation" and even treat "artificial" as equivalent to "instrumental," as in the passage, quoted above, where they gloss Kepler as suggesting that "we can trust instrumental, artificial observations as much as we trust our eyes." According to several Latin dictionaries that I consulted, however, the primary meaning of *artificiosus* was "skillful" (hence Donahue's translation as "technically sound").[193] Though *artificiosus* could be used in the sense of "artificial" as opposed to "natural," this was apparently a secondary meaning of the term. If the Donahue translation is the correct one (though I'm not in a position to ascertain this), then this rendering of the subtitle indicates that what is important for Kepler is not so much that observation be done via instruments, but rather that it be done with *skill*, i.e., with "procedures and precautions."

This reading of *artificiosa observatione* may seem to be in tension with my general thesis that Kepler paved the way for the instrumentalization of scientific observation. On the other hand, it supports my claim that Kepler conceives of scientific observation as a special kind of activity, the reliability of which cannot be ascertained by the criteria of ordinary observation. In my view, Kepler paved the way for the instrumentalization of scientific observation not simply because he used instruments to observe, but because he treated the activity he was concerned with—measurement—as a process that is not essentially anthropocentric. So the reading creates tension with my general thesis only on the assumption that "instrumentalization" simply refers to the use of instruments. On the other hand, if it refers to the non-anthropocentricity of the process *for which* the instruments are used, there is no tension.

Before continuing, two more points should be made here. First, recall that I characterized systematic measurement error as a type of error associated with inadequate schematic models of our instruments and observation procedures. To my knowledge Kepler never uses the terms 'systematic' or 'experimental error,' but just 'error.' Moreover, he does not use the vocabulary of 'models' to describe observational errors. Nevertheless, as I hope will become clear from the evidence I have extracted from the *Optics,* Kepler was

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[193] For example, *Cassell's New Latin Dictionary* translates *artificiosus* as: "(1) *skillful, accomplished* … (2) *skilfully made* … hence *artificial* (opp. to natural)."

keenly aware of what we would now call biased error arising from sources within the instrument, as well from astronomers' inadequate understanding of the workings of the measurement processes they employ. For example, at the beginning of the chapter "On the Means of Vision," Kepler writes that he intends to "explain the deceptions of vision arising from the construction of the instrument."[194] So I do not think I commit a malignant anachronism by using the term 'systematic' or 'biased error' to describe the errors he is dealing with.

Second, Kepler's theorizing about how the eye and other instruments work is motivated in part by a re-examination of past claims.[195] So the deep concern I attributed to him above is displayed not just in his positive contributions but also in the way he reacts to past work. For example, in chapter 2, "On the Shaping of Light," Kepler begins by discussing previous measurements of the apparent diameters of the sun and moon. These measurements had all exploited what Kepler calls a "theorem" introduced by Aristotle and John Pecham (mentioned above, whom Kepler calls "Pisanus"), namely that "the ray of the eclipsed sun [is] similarly eclipsed when it is taken through a small hole."[196] This "theorem" was the principle underlying measurements of the lunar and solar diameters during eclipses:

> … they [Aristotle and Pecham] afforded Reinhold, Gemma, and my teacher Maestlin, the opportunity to apply the theorem to a use that is no less noble [than explaining the shape of light rays cast through small holes]. For these authors I have named had taught astronomers how to use a compass to measure (*dimetiri*) the magnitudes of solar eclipses, the ratios of the diameters of the sun and moon, and the inclinations to the vertical of the circle drawn through the centers of the luminaries, avoiding the inadequacy of the eyes, and avoiding the error which generally occurs in a bare estimation. And so, from that time, however many solar eclipses were documented by eminent mathematicians, it is likely that they were observed (*obseruatas*) in the way just now described …
>
> It is indeed well worth while here to see how much detriment would result from the ignorance of the proof of this theorem. For since it escaped a number of authors, the result was that in believing in the theorem without restrictions they fell into a large error. For however many

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[194] Kepler (2000), p. 171. "Denique deceptiones visus, ab instrumenti conditione ortas, explicabo, et ad vsum astronomicum accommodabo." Kepler (1939), p. 144; (1604), p. 158.

[195] As noted by Hon & Zik (2009), pp. 309, 332-333, 340.

[196] Kepler (2000), p. 56.

eclipses were observed in this way, they all had come out much greater in the sky than it appeared in the ray: all showed a much greater lunar diameter in the sky than in the ray. Hence it is that that Phoenix of astronomers, Tycho Brahe, in his wonder, was driven to such straits as to pronounce that the lunar diameter is always a fifth part smaller in conjunctions than it appears to be in oppositions, even though it is the same distance from us in both instances …

It is my hope in these pages to remove these considerable difficulties, which wall off our entry to the prediction of eclipses and to an exact reconstruction of the moon's motion, by a straightforward demonstration of the theorem, and by laying bare the sources of the errors which displayed themselves for me to examine through a careful consideration of the solar eclipse that occurred in 1600.[197]

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[197] Kepler (2000), pp. 56-7. "*Caeterum et ARISTOTELES, et is quem dixi PISANUS, ad enodationem argumenti pulcherrimum experimentum afferens, de Solis deficientis radio similiter deficiente, cum is per angustum foramen recipitur: occasionem REINHOLDO, GEMMAE, et MAESTLINO Praeceptori meo*
subministrauit, accommodandi theorema ad vsum non minus nobilem. Nam hi à me nominati authores docuerunt Astronomos Eclipsium Solarium quantitates, diametrorum Solis et Lunae proportiones, et circuli
per luminum centra traiecti ad verticalem inclinationes, citra oculorum damnum, citraque errorem, qui solet nuda aestimatione committi, circino dimetiri. Ab eo igitur tempore, quotquot Solares Eclipses à praestantibus Mathematicis annotatae sunt, modo iam dicto obseruatas esse verisimile est: cum praeter hanc nulla alia certa rei, quae in codo fit, metiendae ratio possit institui.

Verùm hîc operae pretium est, videre, quantum incommodum habeat ignorata theorematis demonstratio. Haec enim cum authores aliquos fugerit, factum est, vt theoremati sine limitatione credentes in magnum errorem inciderint. Etenim quotquot hoc modo obseruatae sunt Eclipses, omnes illae multò maiores in codo euenerunt, quàm apparuit in radio, omnes diametrum Lunae in codo multò maiorem exhibuerunt, quàm in radio. Hinc est, quod Phoenix ille Astronomorum, TYCHO BRAHE, mirabundus in has coactus fuit angustias, vt diametrum Lunae quintâ semper parte minorem esse pronuntiauerit in coniunctionibus, quàm apparet in oppositionibus, quamuis vtrinque aequè à nobis absit. Non tamen negarim, alias etiam subesse causas, cur reuerâ nonnihil minor appareat Lunae diameter in coniunctionibus, de quibus infra.

Tantas difficultates, quae aditum nobis ad Eclipsium praescientiam, et ad exactam motus Lunae restitutionem obuallant, spero me his pagellis tollere, demonstratione theorematis expeditâ, et apertis errorum fontibus, qui mihi ex accurata consideratione deliquii Solaris, quod anno 1600. contigit, sese conspiciendos exhibuerunt." Kepler (1939), p. 40; (1604), pp. 39-40.

Kepler goes on to show that a theoretical derivation of the process by which the image of the eclipse is formed in the *camera obscura* reveals that the aperture adds a penumbra to the image. Since the previous eminent mathematicians and "phoenixes" of astronomy had been unaware of this source of error, they had failed to correct for it when measuring the lunar and solar diameters by this method, thus systematically overestimating the solar and underestimating the lunar diameter.

I have quoted this passage at some length, in part because it is typical of Kepler's approach in this work. For example, in the first paragraph it is clear that "observe" (*obseruare*) means "measure" (*dimetior*) in this context. Moreover, the passage shows the relevance of Duhem's analysis of scientific observation reports. Despite the fact that it may be anachronistic to characterize what Kepler is doing as dealing with "idealized models," the passage shows Kepler (i) identifying the method and assumptions underlying the astronomers' observation reports; (ii) criticizing an improper application of the key principle underlying the measurement—believing in the theorem without restriction or proof; and (iii) revealing how the improper application causes the astronomers to ignore an important respect in which the principle idealizes the process of image formation by leaving out the contribution of the instrument to the image. We see here that Kepler does not question his predecessors on the grounds of honesty, veridical perception and other criteria for assessing ordinary observation, but whether they have correctly interpreted the measurement process.

In the Preface to the *Optics*, Kepler writes that the optical part of astronomy arises as a response to the "mediatedness" of the human observer's relationship to astronomical objects:

> But because all celestial observation takes place through the mediation of light or shadow, and because the media between the stars and the eye have a variety of modifications, and because those things that we observe in the heavens are either motions (whose kinds include retrogradation, station, and so on), or arcs (that is, angles at the observer), or luminous bodies; and because all these are considered in optical science hence arises the third, *optical*, part of astronomy.[198]

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[198] Kepler (2000), p. 13. "*Quia vero omnis obseruatio coelestis fit mediante luce vel umbra, mediaque stellas inter et oculum distinctas habent affections, et quae in coelo obseruamus, vel motus sunt, quorum species retrogradatio, statio etc. vel arcus, hoc est, anguli ad visum; vel corpora*

In Chapter 10 on the "Optical foundations of motions of heavenly bodies" it will turn out that the observation of motions in the heavens is complicated, both by the fact that judgements of relative motion can be confounded by the observer's own physical relationship to the observed bodies, as well as by the fact that the motions have to be inferred from positional data. In the Preface, Kepler stresses the importance of measurement in astronomy:

> And thus the quantity of the image which the moon or the sun, whether whole or eclipsed, shows us, and of the shadow which the earth stretches out to the moon, must be carefully investigated by the astronomer. The diameter of the other stars are sought out to the extent that, if neglected, they will render the observations uncertain … [199]

In this passage we also see the concern with identifying and eliminating the causes of uncertainty in astronomical measurements.

It is important to recognize that what I have called the "mediatedness" of the observer's relationship to astronomical objects affects both Kepler and the perspectivists. For example, the problems of the apparent retrograde or stationary motion of the planets, or the interference of the media between the stars and the eyes, are problems that affect astronomical observation on any theory of vision. Indeed, many, if not most of the observational problems Kepler is concerned with in the *Optics* are neutral with respect to an intentional species account versus Kepler's light-based account, as well as with respect to the "epistemic gap" allegedly created by the latter. I give two examples here. In chapter 10, Kepler considers the problem of determining the speed of a planet.[200] The question is whether the planet's motion is eccentric with respect to vision. This question is important because if the motion is eccentric, some parts of its orbit will be farther away from the observer than others. Consequently, the planet will appear to pass through unequal arcs in

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lucida; omniaque ista in Optica Scientia considerantur; hin oritur tertia pars Astronomiae Optica …" Kepler (1939), p. 14; (1604), pp. 1-2.

[199] Kepler (2000), p. 16. "*Quantitas itaque specie, quam Luna Solue seu integer seu deficiens nobis ostendit, vmbraeque, quam Tellus ad Lunam extendit, Astronomo diligenter est inuestiganda. Stellarum caeterarum diametri eatenus quaeruntur, quatenus ignoratae obseruationes infidas redditurae sunt, et quatenus eadem circa illa, quae circa Solis et Lunae corporum moles scire satagimus*." Kepler (1939), p. 16 ; (1604), p. 4.

[200] Kepler (2000), p. 339; (1939), p. 282; (1604), pp. 328-9.

equal periods of time, and will therefore appear to slow down where the arcs appear small and speed up where the arcs appear large. That is, the observer's physical relationship to the planet may confound the determination of the speed. Whether his access to the planet is perceptually direct, by the conversion of the intentional species sent out by the planet into intelligible species in the mind, or indirect, by the interpretation of a picture on the retinal wall, is irrelevant to answering the question of eccentricity.

Another example involves the use of a type of quadrant to measure the angle between a pair of stars from the point at which the observer is located.[201] More precisely, an accurate measurement requires that the point at which the lines connecting each star to the observer meet be correctly located (Figure 3.3c). If one were to locate it at the point of contact between the eye and the instrument—that is, at the surface of the eye—one would introduce a systematic error into the measurement and obtain too large an angle. The reason is that the lines drawn from the stars through the upper sights of the instrument do not converge at the surface of the eye.

According to Kepler, the correct point is the center of the eye. I am unclear as to whether Kepler intends the center of the eyeball or the center of the lens. Based on his diagram of light being refracted through a globe on p. 213 of the *Optics*, and Smith's reconstruction of it, with retina added, in Smith (2015), p. 362 (see Figure 3.3a and b), the point of intersection would appear to be in the lens, and thus significantly off-center with respect to the eyeball. If Kepler intends the center of the eyeball, this might be a case of Kepler "baking in" certain aspects of the visual cone of the Perspectivists discussed above.[202]

In any case, the reason he gives for using the center of the eye is that the picture is arranged on the retina such that straight lines projected from individual points of the retina through the center will fall on the corresponding points of the visible object. The straight lines from each star will therefore intersect at the center, thereby allowing a measurement of the angle of vision. Presumably, a perspectivist would require that the center of sight be used, which Alhacen located in the center of the eyeball, in the vitreous humor between

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[201] Kepler (2000), p. 227; (1939), p. 190; (1604), p. 212.

[202] I owe this observation to Dr Tawrin Baker.

the lens and the optical nerve.[203] Though again I am uncertain as to whether Kepler's center and Alhacen's center are identical, regardless of how the center is determined, Kepler's main point is that using it avoids a systematic error. One would have to deal with this source of error regardless of whether one held that the images of the stars formed in the eye can be assumed to correspond to their objects by virtue of a transmitted similitude, as the perspectivists did, or whether one thought that the correspondence must be inferred based on traces left by the causal process of image formation (Keplerian patches of light on the retinal wall). Moreover, if Kepler is indeed smuggling in the visual cone model, then he is using the perspectivists' own explanatory success, rather than something new in his account of vision, to avoid the error.

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[203] Smith (2015), p. 186.

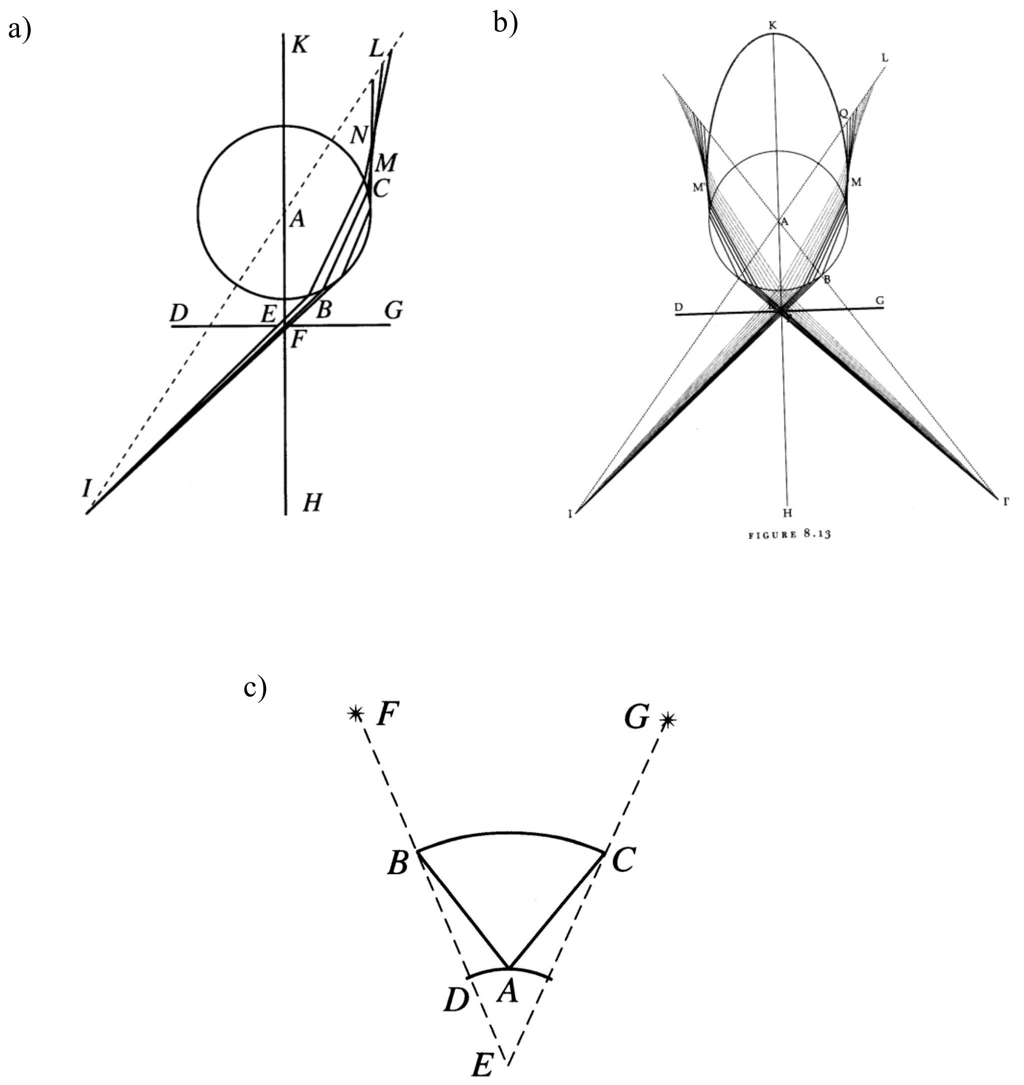

Figure 3.3 (a) Kepler's diagram of light refracting through a globe. (b) Smith's reconstruction. (c) Kepler's diagram of an instrument for measuring the angle of vision between stars.

As Duhem noted, the correction of systematic measurement error requires the use of theories, especially of theories concerning the phenomena implicated in the measuring instruments.[204] Though I have not found instances of the use of the term 'theory' in the *Optics*, in Chapter 10 of the *Optics* he argues that *reasoning* necessarily intervenes in our observation of celestial motions. He starts with a question about the nature of our perceptions of these motions:

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[204] Duhem (1982), pp. 156-158.

> Since in astronomy, our aim is the contemplation of the motions of the heavens, while everything that we know previously came into our sense, therefore, it is worth our while to consider whether the motions of the heavens come immediately into the perception of the eyes, and what kinds of optical illusions occur in the celestial motions.[205]

He then observes that "there exists no way to grasp motion visually except by comparison to some things at rest." He argues that we can be deceived about what is at rest, and hence vision may mislead us as to what is moving. He concludes with the surprising assertion that the problems of astronomical observation would not be resolved merely by bringing us closer to celestial bodies:

> From these things [various reasons why we can be deceived about what is at rest] it follows that even if someone were to carry us across to the moon or to another of the wandering stars, and the moon's motion is most highly perceptible because of its swiftness … nonetheless the moon is going to appear to be at rest along with us, while the sun and whatever heavenly bodies are at the right distance are all going to be thought to be moved with those motions which were proper to the moon itself alone, in addition to their own motions.[206]

I note again that the problem highlighted here would be important on any account of vision. Furthermore, this passage contradicts GCM's repeated assertion that the main problem dealt with in the *Optics* is one of bridging the distance between humans and astronomical objects.[207] Kepler is explicit here that closing this distance would not change the fact that visual judgments of motion are always comparative and hence involve fallible assumptions about what is at rest and what is not.

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[205] Kepler (2000), p. 335. "*Cum sint nobis in Astronomia propositi ad contemplandum coelorum motus, omnia verò, quae discimus, prius veniant in sensus, operae pretium est perpendere, an coelorum motus immediate incurrant sensum oculorum, et quaenam contingent deceptiones visus in motibus coelestibus*." Kepler (1939), p. 279; (1604), p. 324.

[206] Kepler (2000), p. 336. "*Ex his sequitur, etsi nos quis in Lunam aut aliud errantium astrorum trnasferat, motusque Lunae sit maximè sensibilis, causâ celeritatis, de quo postea, nihilominus visum iri Lunam nobiscum quiescere : Solem verò et quaecunque sidera in iusta fuerint propinquitate, omnia, praeter suos motus, iis etiam motibus putari moueri, qui fuerint ipsius solius Lunae proprii*." Kepler (1939), p. 280; (1604), p. 327.

[207] Gal & Chen-Morris (2013), pp. 19, 20, 24.

Kepler continues by arguing that the speed of the stars cannot be detected by sense perception. The motions of the stars must be inferred from their positions as a function of time.[208] But this inference is to be accomplished by geometrical demonstration:

> the foremost thing we seek in these bodies are their motions, so much to be wondered at. But in order that an astronomer be able to *fish these out with geometrical demonstrations (geometricis demonstrationibus expiscari*), one first needs to measure their position with instruments.[209]

He concludes that "whatever is in our senses concerning the motion of the heavens, we have absorbed thanks to the intervention of reasoning."[210]

In addition to these arguments, we should recall Kepler's derivation of the *camera obscura*'s penumbra in chapter 2. Kepler there refers to what he is doing, and what his predecessors should have done, as a "demonstration" (*demonstratio*) of how the image is formed inside the *camera*. A "demonstration" is here equivalent to a proof[211] in the geometrical sense of inferring a proposition from definitions and postulates. It thus seems safe to conclude that Kepler is *theorizing*, in the sense of using "a systematic inference procedure," as a means for correcting biased error in observation.

Whether he is using *theory* in a more modern sense is a further question. Jardine (1984) has attributed to Kepler's earlier (1600) *A Defence of Tycho against Ursus* the first appearance of a concept of scientific theory as "a systematic body of hypotheses that is related to a systematic practice of prediction, observation and instrumentation in some domain of inquiry."[212] This is basically the sense in which Duhem understood 'theory.' Certainly, Kepler's study of image formation in the *camera obscura* and the eye, together

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[208] Kepler (2000), p. 319; (1939), p. 264; (1604), p. 307.

[209] Kepler (2000), p. 319; (1604), p. 165. "*Caeterùm quod praecipuum in his corporibus quaerimus, sunt eorum motus tam admirabiles. Vt verò hos geomtricis demonstrationibus expiscari possit Astronomus; situm eorum prius instrumentis dimetiatur necesse est*." Kepler (1939), p. 265; (1604), p. 307.

[210] Kepler (2000), p. 337. "*Itaque quicquid de coelorum motibus est in nostris sensibus, beneficio ratiocinationis interuenientis hausimus.*" Kepler (1939), p. 281; (1604), p. 327.

[211] Donohue translates *demonstration* as either 'demonstration' or 'proof.' See Kepler (2000), pp. 55-57 and Kepler (1604), pp. 46-48.

[212] Jardine (1984), p. 289.

with his use of the results to correct observational practices employing these instruments, seems like a reasonably good fit for such a concept.[213] The various propositions he derives are related to each other systematically, in that they all follow from the same definitions and postulates as well as from each other. Moreover, he uses them to correct instrumentation and observational practices, as I will now show for the eye.

3.3.6 The eye and the problem of systematic measurement error

Kepler makes an understanding of the eye central to the task of identifying sources of observational error: "the occasion of looking into error in vision must be sought in the formation and functions of the eye itself."[214] Kepler follows the description of the eye given by Felix Platter (1536-1614), Professor of Medicine at Basel from 1560 to his death.[215] Kepler treats the organ as an instrument analogous to the *camera obscura*, something Platter did not do: "For the pupil takes the place of the window, the crystalline takes the place of the panel opposite."[216] In the perspectivist account, the crystalline lens was both optical and sensitive, optical in that it received visual rays, sensitive in that the function of the visual spirits was to "feel" the intentional species and replicate the image of the object as a visible species. In contrast, Kepler's lens is a purely optical device, in the sense of a

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[213] Straker (1981) seems to have arrived at the same conclusion.

[214] Kepler (2000), p. 171. "*Erroris itaque in visu, occasio quaerenda est in ipsius oculi conformatione et functionibus*."

[215] Kepler corroborates the description with that of his friend Johannes Jessenius, Professor of Medicine at Prague, where Kepler came to know him, and who in turn followed that of the pioneering Paduan anatomist Hieronymus Fabricius ab Aquapendente (1537-1619). See Kepler (2000), pp. 171-2. Smith (2015), p. 352 notes that Platter's description to some extent anticipates Kepler's "deadening" of the eye, for Platter's model locates visual sensitivity entirely in the retina. On the other hand, Platter did not explain how the lens projected an image onto the retina.

[216] Kepler (2000), p. 184. "[*E]t fit penè idem, quod supra cap. 2. demonstrauimus in clausa camera fieri. Nam pupilla est fenestrae loco, crystallinus loco oppositae tabulae*." Kepler (1939), p. 155; (1604), p. 173.

device that manipulates light[217] and that has the effect of refracting the entering light rays onto the retina. The upshot for image formation is that whereas for the perspectivists the image was formed on the lens, for Kepler it is formed on the retina by projection from the lens.

In general, there are two ways of responding to systematic measurement error: once the sources of biased error have been identified, either they are removed or their effect is reduced by adjusting the data to compensate for them. Corrections can be built into instruments, as for example when thermometers are constructed with a non-linearized scale of temperature markings to compensate for non-uniformities in the thermal expansion of mercury. Or they can be implemented in the theoretical interpretation of the experimental results, for example by applying formulae that take causes of error into account.

Kepler employs both kinds of response. With the eye, modification of the instrument is difficult, especially in his time. Replacing the eye with artificial instruments, however, permits both responses.[218] In his account of the *camera obscura* in Chapter 2 of

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[217] As opposed to a device that enables seeing. O*psis* is the Greek word for sight, which it still meant in Kepler's day (Dr Tawrin Baker, personal communication).

[218] Hamou (1999), pp. 236-237 argues that at the time of writing the *Optics*, Kepler could not conceive of substituting (*substituer*) the *camera obscura* for the eye, because the idea of adding a lens to the instrument could not have occurred to him before Galileo's disclosure of the telescope's powers. I do not think my assertion in this sentence contradicts Hamou, however, because for Hamou *substituer* instruments for eyes means *making the instruments more like the eyes*, e.g., by adding a lens to the *camera obscura*. Substitution in this sense is different from my sense of "replacing "the eyes with an instrument, which enables one to measure the same quantity by a different process altogether. This difference between Hamou and I reflects a difference in how the telescope and the *camera obscura* relate to vision. Whereas the telescope lends itself to an empiricist interpretation of its epistemic import, that it extends the visual sense, the *camera* breaks with vision rather than extending it. This in two ways. First, it was used by Kepler and other astronomers for measuring parameters of celestial objects, not for seeing distant objects. One could already see the sun and the moon; the question was how best to measure their diameters and so on. Second, it provided an alternative process for measuring a given quantity, rather than extending the reach of an existing, ocular process. The difference is rather like the one between the scientific balance, which accentuates the precision of ordinary balances, and the mass spectrometer, which measures mass by an entirely different process (Bachelard 1949, p. 103). Paradoxically, given the much

the *Optics*, for example, Kepler explains how the quality of the picture can be altered by manipulating the experimental set-up. The size of the aperture, the distance of the latter from the wall on which the picture is projected, the distance of the outside object from the aperture, the brightness of the ambient light around the object, and even the color of the air between the object and the aperture (which Kepler thought was blue) can all affect the quality of the picture.[219]

More importantly, from a historical perspective, Kepler also shows how his theory of the instrument can be used to make corrections to the data on solar and lunar eclipses. As noted above, Kepler motivates his account with the observation that previous astronomers, including Tycho Brahe, had been unable to calculate the correct apparent lunar diameter using *camera obscura*-observations of eclipses because they did not have a "proof" or "demonstration" of why the ray of the eclipsed sun is also eclipsed in the picture.[220] Straker has shown that though Brahe had an empirical method for correcting apparent solar diameters, this method was unsuccessful when extended to apparent lunar diameters during solar eclipses.[221] In his account of the *camera obscura*, Kepler derives theoretically that the image of the eclipsed sun in the picture will be augmented by a penumbra because the shape of the aperture will be mixed in with the shape of the sun. Without correction for the size of the aperture, then, the calculation of the apparent diameters of the sun and moon will systematically overestimate the diameter of the sun and underestimate the diameter of the moon (relative to the apparent diameters in the sky).[222] Using Kepler's computations, Straker has estimated the error introduced without the correction at about 12%.[223]

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greater prestige of the telescope, Kepler's treatment of the eye as a kind of *camera* seems like the more radical "instrumentalization" from this perspective.

[219] Kepler (2000), pp. 68-69; (1939), p. 59; (1604), p. 53.

[220] Kepler (2000), pp. 57-58; (1939), p. 48; (1604), pp. 39-40.

[221] Straker (1981).

[222] Kepler (2000), pp. 70-71; (1939), pp. 60-61; (1604), pp. 54-55. Hon (1987) claims that Kepler's use of theory-mediated corrections of data was quite innovative for the time.

[223] Straker (1981), p. 271.

Despite the advantages of man-made instruments, Kepler demonstrates that knowing how the eye works is also useful for correcting observations. Eyeglasses are an interesting example. According to Smith (2015), explaining how eyeglasses correct weak sight was problematic for the perspectivists, because the key mechanism for sight was the passage of the visual spirit from the brain to the lens.[224] For the perspectivists, there could be no grounds for *correcting* vision because the visual spirits ensured a faithful replication of the visual field. Unless the eye was physically damaged (e.g., by corneal lesions), the perspectivists apparently had few explanatory resources for accounting for weak vision other than to invoke some problem with the flow of visual spirits to the lens, for example a constriction of the optic nerves. This limitation led them to explain the functioning of eyeglasses as a form of deception rather than correction: convex lenses were thought to compensate for myopia by making objects appear larger and closer than they really are. This explanation encouraged physicians to favor treatments designed to improve the supply of visual spirits over the use of glasses.

The recognition by the mid-fifteenth century that concave lenses correct myopia represented an anomaly for perspectivism, because these lenses shrink rather than magnify the object of vision. Myopia should therefore be worsened rather than improved. The perspectivist Francesco Maurolico (1494-1575) was able to deal with the anomaly, but only by proposing that myopia is due to improper curvature of the lens's anterior surface. [225] The improper curvature has for consequence that the visible species enter the optic nerve in too compressed a form, because it alters the path along which the visible species are refracted from the lens to the nerve. This explanation violated the perspectivist rule[226] that the visual spirits in the lens are sensitive to perpendicular rays only. This sensitivity was an essential component of the perspectivists' explanation of why clear vision is possible—if both perpendicular and oblique rays could be sensed, then the impression at any given spot on the lens would be confused.[227] Maurolico needed to break this rule, however,

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[224] Smith (2015), p. 338.

[225] Smith (2015), p. 338-339.

[226] Among other rules. See Smith (2015), p. 340-341 for a discussion.

[227] On the other hand, the perspectivists all gave a secondary description of vision that did take into account all of the rays entering the eye (not just the perpendicular ones). This accounted for

because his explanation of myopia involves a double refraction of the visual rays—the first at the anterior surface of the lens, the second at the posterior surface. In orthodox perspectivism, because only rays perpendicular to the anterior surface are sensed, there is only a single refraction, at the posterior surface. Thus, though Maurolico's explanation points towards Kepler's later treatment of the eye as a purely optical device, the function of the visual spirits in perspectivist theory was not really compatible with such a treatment. This incompatibility is reinforced by the fact that, according to the perspectivists, the single refraction at the posterior surface was at odds with how rays would refract in the absence of the visual faculty: the refraction of the rays occurs differently in a dead eye (e.g., the eye of a dead person) compared to how it occurs in a living one.

In Kepler's model of the eye, on the other hand, clarity of vision is the result of the way the incoming light rays are refracted by the lens onto points on the retina. The sensitivity function is dropped, and the eye is treated as a purely optical mechanism. Therefore loss of visual acuity must be explained by a deformation of the organization of its material parts. Presbyopia and myopia, for example, can be explained as caused by late (presbyopia) or premature (myopia) focusing of light vis-àvis the retina due to structural deformations. Therefore the effects of eyeglasses can be explained as corrections to vision brought about refractively by the lenses. That is, eyeglasses *correct* rather than deceive. Convex lenses correct presbyopia, for example, by bringing the focus forward to the lens, whereas concave lenses correct myopia by bringing the focus back to the retina.[228] The moral of this non-scientific example is that on a Keplerian approach, any systematic error in observation, like presbyopia, has to be explainable in terms of some deformation or dysfunction in the mechanism of image formation. The approach thus demands inquiry into built-in sources of error. On the perspectivist approach, on the other hand, because so much work is done by the sense faculty, the explanation of such errors that produces the least

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peripheral vision, and so within this tradition itself there was a tension between the two accounts of vision. I thank Dr Tawrin Baker for pointing this out to me. Smith (2015), p. 217 thinks the second description of peripheral vision renders Ibn al-Haytham's visual cone model "incoherent."

[228] Kepler (2000), pp. 216-218 ; (1939), pp. 181-183; (1604), pp. 200-203.

strain with theory locates the source of the error in the production of the faculty rather than in the eye. Rather than a demand, there is a positive disincentive to focus on built-in causes.

This latter point deserves some elaboration. To view the eye as a "purely optical mechanism" is, roughly speaking, to reduce its contribution to sight to the organization of its material parts. Thus any defect of vision resulting from the eyes has to arise from some defect in that organization or the parts. The power of the visual spirits, on the other hand, is not due to the material organization of the eye, since they are produced by the brain, which produces them by giving form to highly rarefied animal spirits. Indeed, Bacon held that the visual power of the eye resided only in the anterior glacial humor, what we would call the crystalline lens:

> … only in it does the visual power reside, according to Alhacen and others. For all other humours anterior to it are its instruments and exist for its sake. For if the anterior glacial humour should be injured, while the others are preserved, sight is destroyed; and if the anterior glacial humour is preserved, while injury befalls the others (provided their transparency remains), sight is not destroyed but still functions.[229]

Similarly, Witelo claimed that "only the glacial [humor] is the proper organ of sight, and not the surface of the eye that is part of the sphere of the cornea."[230]

According to Bacon, this visual power flowed to the eye from the common nerve at the front of the brain, where the species passed on from the lens of each eye were combined into a single species:

> It is thus evident that the eyes are not alone in rendering judgement concerning visible things; but judgement begins in the eyes and is completed by the ultimate sentient power, the source of the visual faculty, [located] in the common nerve. It is equally clear that the eyes do perceive, and not only the common nerve. But since the eyes are connected to the source of the [visual] power, and powers flow from it to the eyes, so that the sensitive power is extended through the whole [optic] nerve from the common nerve to the eyes, as Alhacen says, therefore the visual act is one and

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[229] Lindberg (1996), p. 51. *"... virtus visiva est tantum in eo, secundum Alhacen et ceteros. Alia enim omnia ante ipsum sunt instrumenta eius et propter ipsum. Nam si ipse ledatur, aliis slavis, destruitur visio; et si ipse sit salvus et aliis accidat lesio, dummodo maneat eorum dyaphanitas, non destruitur visio quin fiat*" (*Perspectiva* I.4.2). According to Lindberg, this text was written in the period 1265-1268.

[230] "*sola glacialis proprie est organum visus, et non superficies oculi, que est pars spere cornee*" [Unguru (1991), p. 314 (*Perspectiva* III.18).

> undivided, carried out by the eyes and the common nerve … the eye [itself] necessarily makes judgements and has the power of sight, though incompletely.[231]

On Bacon's account, the eye's contribution to sight is almost completely determined by the connection of the lens to the common nerve.[232] This connection, moreover, matters not because of its material organization, but rather because it is a conduit for the visual powers. And as noted above, these visual powers are only present in an eye attached to a living organism, for an eye that has been severed or whose owner is deceased—an eye reduced to its material organization not just in theory, but in reality—does not produce a visible species.

When one holds such beliefs about the eye, then, there is not much reason to inquire into structural causes of deficient vision. Bacon's treatment of visual errors in the *Perspectiva* may be symptomatic in this regard. Despite the chapter title "Concerning various errors of vision that arise owing to the structure (*compositionem*) and complexion of the eye," all of the causes of error arise from some source external to the eye itself:

1. Strabismus—the inability to direct the visual axes of both eyes to the same point at the same time.
2. Visual judgment is impaired by extremes of heat and cold, which damage or weaken the eyes.
3. People who are intoxicated, infirm, or angry may see double, due to vapors that are released in each case and which disturb the eyes from their natural position.
4. Likewise, head injury, dizziness and vertigo also result in the release of vapors to the eye, which cause the visible object to appear in motion.

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[231] Lindberg (1996), p. 67. "*Et sic patet quod non solum oculi iudicant de visibili ; sed incipitur iudicium in eis, et completur per ultimum sentiens, quod est virtus visiva fontalis in nervo communi. Et similiter patet quod oculi sentiunt, et non solum nervus communis. Sed quoniam oculi ordinantur ad virtutem radicalem et ab illa fluunt virtutes ad oculos, et continuatur virtus sensitiva per totum nervum a nervo communi ad oculos, ut dicit Alhacen, ideo una est operatio visiva et indivisa, que perficitur per oculos et nervum communem ... oculus necessario habet iudicium et virtutem videndi, licet incompletum*" (*Perspectiva* I.5.3).

[232] I say almost, both because of the transparency requirement on the humours, and because of the aforementioned refraction at the posterior surface of the lens that was necessary to avoid image inversion.

5. The vitreous humor, which extends from the lens to the common nerve, the visual spirits flowing from the common nerve to the eye, and the uvea, all easily receive motion. The motion imparted to them can cause a single object to appear multiple.
6. A “foreign humor” can “gather” in the uvea, causing double vision.[233]

In each case, exogenous conditions conspire to create visual error.

In Kepler’s case, on the other hand, the production of the image is due *solely* to the eye’s structure. Therefore, there is a very strong incentive, on this view, to inquire into causes within the eye for errors of vision.

An example of Kepler applying his understanding of the eye to a scientific problem is his treatment of the apparent enlargement of bright objects in Chapter 5.[234] His presentation is a bit confusing, since he starts by stating the problem as if it applied to all observers but then shifts the discussion to those with defective vision. So he asks “why it is that, to all people without exception, all things that are luminous appear greater in proportion to things placed nearby that are less luminous.”[235] Then the discussion shifts to “those who are weak of sight, and who are otherwise blind to distant things.” Perhaps the reason for the shift is that there will always be variations in strength of vision among people, even though the variations might not amount to defects like myopia, presbyopia, and so on that call for treatment. These variations pose a problem for astronomical observation, because they result in discrepancies between estimates of the dimension of celestial objects:

> In full moons, it is occasionally the experience, as may be seen in Tycho’s observations, that when five or six people are observing the same moon, the estimation of the diameter is inclined to vary, ranging from 31 to 36 minutes, according to the vigor of each one’s vision. This is,

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[233] Lindberg (1996), pp. 171-5. “*De variis erroribus visus propter compositionem et complexionem oculi*” (*Perspectiva* II.1.3).

[234] Kepler (2000), pp. 232-236.

[235] Kepler (2000), p. 232. “*cur omnibus adeò hominibus, quaecunque lucida sunt, maiora appareant in proportione, quàm quae sunt iuxta posita minus lucida*.” Kepler (1939), p. 194; (1604), p. 217.

> moreover, the chief controversy about the moon. On 1590 February 22 the moon was observed 22 times: twice at 31’, six times at 32’, seven times at 33’, six times at 34’, once at 36’.[236]

To the near-sighted, like Kepler, the observation of an eclipse differs from those of others with better vision:

> Those who are weak of sight, and who are otherwise blind to distant things, imagine for themselves a rippling series of ten phases in place of one phase … In the beginnings of lunar eclipses, the eclipse is noticed first of all by me, who am laboring under this defect, as well as the direction from which the darkness approaches, long before the beginning, while the others, who are of the most acute vision, are still in doubt, as happened in the month of May of this year 1603. For the rippling of the moon, mentioned above, stops for me when the moon is approaching the shadow, and is in great part removed from the sun’s rays.[237]

The reason Kepler notices lunar eclipses before others with more acute vision is, I gather, that the multiplication of the images of the moon ceases when the illumination of the moon by the sun diminishes. His explanation of multiplication of phases is related to his explanation of myopia. In Proposition 28 of Chapter 5, he had explained myopia as arising from a defect of the eye that causes the cones of light rays coming from distant points to intersect before they reach the retina (the defect being the elongation of the eye along its main axis). As a result, the light rays spread out again before reaching the retina. This causes the different cones coming from the distant object to “disturb and confuse each other.”[238] The same effect accounts for the multiplication of images:

> All these things [errors of vision connected with the observation of bright celestial objects], and whatever others there are, draw their origin from the retina tunic, but in a different respect. First,

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[236] Kepler (2000), p. 233. “*In pleniluniis interdum vsu venit, vt videre est in obseruationibus TYCHONIS, vt quinque vel sex hominibus eandem Lunam obseruantibus, pro cuiusque visus acrimonia, diametri census à 31 in 36 minuta vagabundus excresceret. Quae adèo praecipua de Luna querela est. Anno 1591. 22. Febr. Luna 22 ies obseruata; bis 31. sexies 32. septies 33. sexies 34. semel 36.*” Kepler (1939), p. 194; (1604), p. 217.

[237] Kepler (2000), p. 233. “*Qui sunt imbecilli visu, et qui aliàs ad remota caecutiunt, pro vna phasi decem phasium cristatam seriem sibi imaginantur ... In Eclipsium Lunae primordiis mihi, qui hoc vitio laboro, primum omnium defectus animaduertitur, atque etiam plaga, vnde ingruant tenebrae, longè ante initium, caeteris, qui sunt acutissimo visu, adhuc dubitantibus, vt huius anni 1603. mense Maio. Nam mihi dicta Lunae crispatio sistitur, Lunâ ad vmbram accedente, et exutâ Solis radiorum parte potissimâ.*” Kepler (1939), p. 195; (1604), p. 218.

[238] “*[S]e mutuo turbabunt et confundent*.” Kepler (2000), p. 217; (1939), p. 182; (1604), p. 202.

> whatever of this affects those with defective vision finds its occasion from propositions 26 and 27 above. For the more distant bodies, such as the celestial bodies, gather the radiations from a single point, into a single point, before they touch upon the retiform, and, cutting each other at that point, they now strike spread out upon the retina. Thus, it is not a single point of the retina that is illuminated by a point of the object, and thus it is encircled by many points: white things, however, and bright things illuminate its surface strongly. They therefore bring it about that those things which are depicted less bright in the same place, where they themselves showed their own boundaries … become entirely invisible, and give way to the white things. And so nearly the same thing happens in the eye which, above in chapter 2, with regard to the configuration of the ray, I demonstrated to happen on a wall.[239]

The spreading out of the cones therefore explains both the multiplication of images as well as the obscuration of nearby objects by brighter neighbors. Kepler also seems to think that the multiplied images can mix in such a fashion as to create a single, larger image, thus leading a near-sighted observer to overestimate the size of the object. To what extent this explanation is supposed to extend to observers with normal vision, is not clear to me. In the following pages Kepler argues that a brighter object will appear larger simply in virtue of making a stronger impression on the visual spirit behind the retina, which seems rather *ad hoc.*

In any case, he concludes that

> from this chapter, Astronomers will ponder this, that ocular perception or reckoning is not always to be trusted, however much they are taken into account in the quantity of the diameter of the full moon, or of the defect in an eclipse; and consequently, that other more certain procedures must not only be brought into consideration, but also one must not rashly disagree with them, on the testimony of vision, when it happens that they disagree with vision. For it has been demonstrated

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[239] Kepler (2000), pp. 233-234. "*Haec omnia, et si qua sunt alia, ex retina tunica trahunt originem, sed diuerso respectu. Primum quicquid huius accidit visibus vitiosis, occasionem ex propositione 26. 27. praemissa inuenit. Remotiora nempe, vt sunt corpora coelestia, radiationes ab vno puncto, cogunt in vnum punctum, antequam attingant retiformem, seque mutuò secantes in eo puncto, iam dilatati in retinam impingunt, sic non punctum retinae à puncto rei, sed superficiecula eius à puncto rei, et sic à pluribus punctis cingitur: alba verò et clara fortiter illustrant suam superficiem. Faciunt igitur, vt quae ibidem pinguntur minus clara, quà terminos ipsa suos protulerant ... planè delitescant, locumque albis cedant, itaque penè idem fiat in oculo, quod supra de radii figuratione capite 2. demonstraui in pariete fieri*." Kepler (1939), p. 195; (1604), pp. 218-219.

> most clearly, from the very structure of vision, that it frequently happens that an error befalls the sense of vision, in overestimating the size of bright things.[240]

I note in passing that the last sentence of this passage—truncated of the last clause—was quoted by GCM as if it expressed a general skeptical worry about vision (see section 3.3.3).[241] Here we see that the criticism of vision is anchored in the context of carrying out specific kinds of measurements. Kepler is concerned with measurement, not observation in general. More to the point of the present discussion, the pay-off of Kepler's approach here is similar to that in the case of the eyeglasses. Treatment of the eye as being like an instrument (note the reference to the wall in the passage quoted above) demands that an internal source, a source within "the very structure of vision," be found for the multiplication of images and the overestimation of size.

It might be objected that such an approach increases doubt, and therefore my interpretation winds up conceding GCM's point about the epistemic anxiety produced by the *Optics*. But the objection forgets that the commitment to find an internal source of error is coupled with a second step, which is to correct for it. Kepler makes this point himself when he states his intention to "explain the deceptions of vision arising from the construction of the instrument" and *then* to "accommodate them to astronomical use." The

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[240] Kepler (2000), pp. 235-236. "*Hoc itaque ex hoc capite Astronomi considerabunt, non semper fidendum esse intuitui oculari aut aestimationi, quantumuis consideratae in quantitate diametri plenae Lunae, aut defectus in Eclipsi: quare non tantum in consilium adhibendos certiores modos alios, sed neque temerè ab iis dissentiendum, visus fiducia, si quando fiat, vt dissentiant illi à visu. Demonstratum enim est euidentissimè, ex ipsa visus conformatione, fieri crebrò, ut visui error accidat, dum lucida nimis magna existimat.*" Kepler (1939), p. 197; (1604), p. 221.

[241] The passage I quoted earlier from the introduction to "On the Means of Vision," where Kepler states his intention "to explain the deceptions of vision arising from the construction of the instrument," is similarly anchored in the context of carrying out astronomical measurements. The introduction begins by noting past errors in the measurement of diameters of the luminaries and the quantities of solar eclipses. Moreover, Kepler immediately follows up his comment on the construction of the instrument with the further intention to "accommodate them [the deceptions of vision] to astronomical use" ("*ad vsum astronomicum accommodabo.*" Kepler (1939), p. 144; (1604), p. 158).

overall result is progress, in the form of the development of more reliable observation procedures.

3.3.7 Concluding remarks

As noted in section 3.3.3, GCM claim that the instrumentalization of scientific observation by Kepler came with the cost that the eye was now "as vulnerable to error as the instrument." In essence, I have been arguing that the recognition of the eye's vulnerability is an epistemic strength rather than a weakness. However, in order to see this, we need to be clear about what epistemic problem it helps us deal with. I argued above that the problem of the certainty of measurement is distinct from the problem of the certainty of perception. One of the main challenges with determining the certainty of a measurement is identifying and correcting for systematic error. Considering the eye to be an instrument like any other shifts the focus of research onto causes within the instrument that may be responsible for errors in our observations. This shift results in a form of progress, as more and more discrepancies in our observations become amenable to explanation and correction. Perhaps such an option was available to the perspectivists, though GCM's and Smith's characterization of the latter as holding that visual data were indubitable would seem to preclude that option. Even if they didn't hold visual experience to be indubitable (recall the reservations I broached in section 3.3.3 concerning the indubitability claim), they clearly appear to have been less successful at resolving the errors in vision and observation that Kepler addressed. An interesting historical question is whether this fact was a reason for adopting the retinal theory.

I have also argued that considering the eye to be an instrument like any other is not merely a heuristic, but an approach grounded in the nature of measurement as a process of material representation. On this view, measurement is inherently "instrumental" or "non-anthropocentric" in that the physical interaction and production of a representation that are at its core need not be carried out by a human or involve human perceptual faculties. Of course, Kepler makes no such claims about measurement, but his actual practice in the *Optics* is compatible with an understanding of measurement along these lines. True, given the instruments employed in the *Optics*—*camera obscura*, quadrant, ecliptic, and eye—human participation in the physical process of measurement was still indispensable, especially for the estimation of the parameter of interest (lunar diameter or whatever). We are well before the era of automatic recording instruments. But the sub-process of image formation is treated as something that can be done just as well by an instrument, namely the *camera obscura*.

As noted above, both Kepler's instrumentalization of the eye and his awareness of experimental error have been discussed by previous authors. My interpretation of the *Optics* in terms of measurement shows how these two aspects of the *Optics* are related and non-accidental: both stem from the nature of measurement. On this interpretation, the instrumentalization of the eye is not epistemologically problematic, for it merely amounts to a recognition of the non-anthropocentric nature of the physical process of measurement.

The general moral that is, I think, implicit in the *Optics* is that recognizing the existence of systematic error is an important step forward in developing reliable means of observation and measurement. From the point of view of achieving this goal, then, Kepler's "radical instrumentalism" is empowering rather than undermining. So while Kepler's theory of image formation in the eye may have provided grounds for epistemic anxiety concerning ordinary observation, on the other hand it provides grounds for epistemic optimism concerning measurements employing the eye or visual instruments.

3.4 Conclusion of chapter 2

A common theme of the two main sections of this chapter has been that the emergence of the modern scientific method involved certain discontinuities with previous ways of conceiving the relation between science, labor and instruments. Indeed, the chapter provides grounds for the plausibility of the hypothesis that several features of Aristotelian thought impeded the emergence of the modern scientific method. Of course, when stated at this level of abstraction the hypothesis is nothing new. Champions and commentators of the Scientific Revolution have advanced various versions of such a hypothesis, for example, that Aristotelian thought relied on obscure causal reasoning (occult causes, substantial forms), that it did not make sufficient use of quantitative methods, that its epistemology and cosmology were mutually reinforcing and therefore not really open to test, that it was not experimental, that it placed too much weight on authority, etc. My emphasis has been on the way in which scientific activity was conceptualized. I have suggested that the pre-revolutionary conceptualization of scientific activity tended to involve the neglect of know-how as an important component of scientific method. This neglect was manifested in the privileging of knowledge of necessary connections and the exclusion of know-how from what was considered science proper, as well as by a strict division of labor between theoretical science and the arts. In the second part of the chapter, I suggested that the pre-revolutionary conceptualization of sense experience tended to see a big difference between how the senses (or at least vision) work and how mechanical instruments work. The former were thought to be animated by the soul, and therefore had a mode of functioning that differentiated them sharply from the latter, whose effects are the result of the organization of their material parts. Kepler contributed to the reconceptualization of scientific activity by showing that the eyes function like mechanical instruments. In doing so, he changed the problem the theory of the eye was a solution to. The key epistemological problem for the Perspectivists was, how is it that humans can have perceptions of the world that correspond to the way it truly is, or in other words how is veridical perception possible? Kepler's conceptualization of the eye as an instrument is aimed at a different problem than veridical perception: how to achieve accurate measurements by means of vision.

In both parts of the chapter, a connection was drawn between the reconceptualization of scientific activity and scientific progress. Progress in the first part consisted in the emergence of a full-blown experimental approach, unfettered by previous strictures on what constitutes the proper methods and objects of science. Progress in the second part consisted in improvements in methods of measurement and, in the long run, the improvement of scientists' ability to study nature through technological change.

The theme of how scientific activity is conceptualized will remain important in the following chapter, where we will examine scientific change resulting from the deliberate application of mechanized methods to science. The theme of progress will remain important, for one of the questions to be addressed there is to what extent changes in the labor process can be neutral with respect to the goals of the field, and hence with respect to the possibilities for progress therein.

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4.0 ON "THE APPLICATION OF SCIENCE TO SCIENCE ITSELF:" CHEMISTRY, INSTRUMENTS, AND THE SCIENTIFIC LABOR PROCESS

4.1 Introduction

In this chapter, we fast-forward from the period of the Scientific Revolution to the heart of the 20th century. By this time, the synthesis of mental and manual labor in science has long since been completed. The question is no longer whether manual labor is an activity befitting natural scientists, but what is the best way of organizing the scientific labor process. The notion of "best" is itself at issue, since science is no longer a largely academic activity but one increasingly embedded in complex socioeconomic processes that exert their own "pushes" and "pulls" on science.

The guiding question of this chapter is, what makes the application of scientific knowledge to scientific work possible? This is one of the guiding questions posed in the introduction:

1. How is it possible for knowledge acquired in the past to be used in on-going or future research?

There is a traditional answer to this question within the philosophy of science: the logical framework of experimental design. Hypothesis testing provides the paradigm. Observations, possibly anomalous, give rise to questions. Proposals are put forward in response to these questions, and tested via their observable logical consequences. The logical framework of hypothesis testing is as follows:

Given A1 & … & An, then if H, then, when C1 & … & Cm, then P.

Where P is a prediction whose truth can be established via observation, the C's are conditions whose truth can be established via observation, and the A's are background assumptions, generally from prior science, needed to deduce observable consequences from the hypothesis. Because background assumptions and experimental conditions are needed to deduce observable consequences, these assumptions and conditions provide "slots," so to speak, in which prior science can (and usually must) be exploited.

The topic of this chapter is cases of scientific change in which the application of prior science to scientific work involved far more than simply the design of a new experiment, but rather the transformation of the nature of scientific work itself. The large-scale application of science to scientific work—as opposed to individual tests of hypotheses—typically requires a transformation of labor within science. The photographs in Figure 4.1 illustrate what I have in mind.

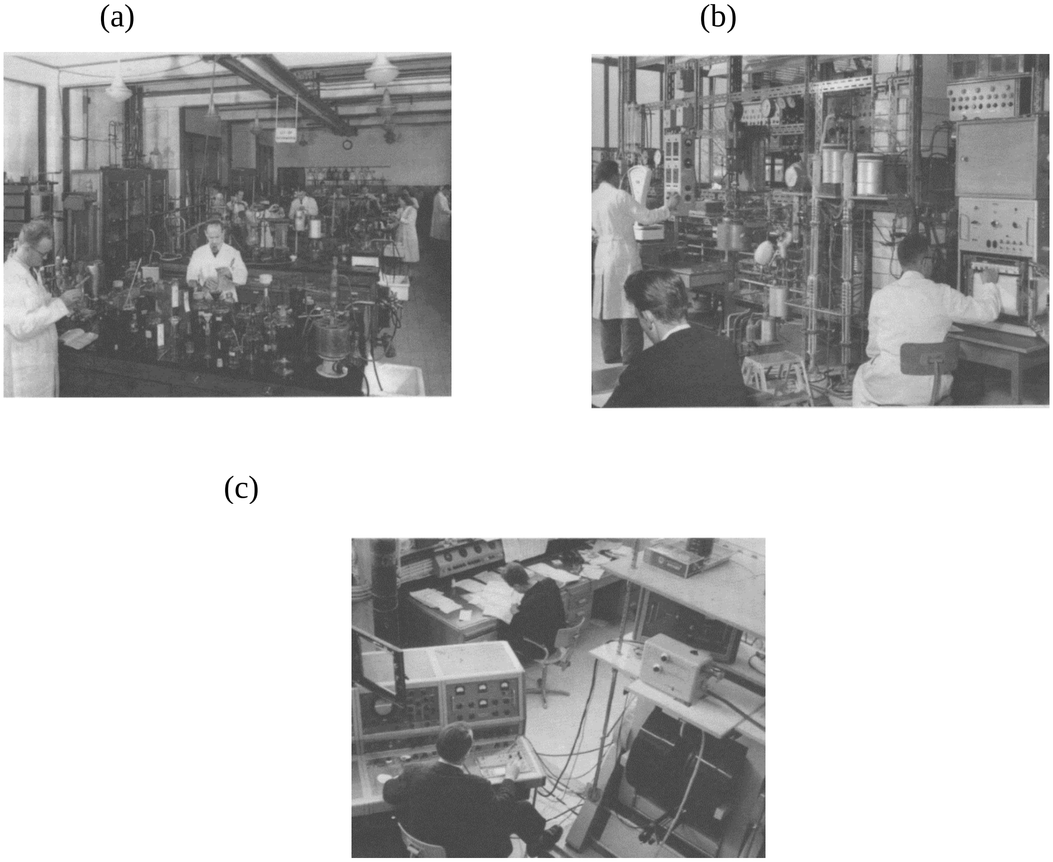

Figure 4.1 (a) A Royal Dutch Shell chemistry lab in Amsterdam, late 1940s. (b) A Shell lab in the mid-1950s. (c) A Shell NMR lab, 1959. Source: Morris (2015), pp. 262-4.

The photographs in the figure depict different labs belonging to Royal Dutch Shell in the mid-20th century. Figure 4.1a depicts a fairly traditional chemistry lab, where scientists are working at benches and manipulating items of glassware and vessels containing chemicals. Each bench is equipped with a sink at the end for cleaning the glassware. Figure 4.1b depicts a new kind of lab, without glassware or benches, where the scientists adjust knobs and switches on large pieces of equipment. Figure 4.1c depicts a lab dominated by a single suite of instruments, here an NMR spectrometer and an electron spin spectrometer. Clearly, a lot of knowledge has been incorporated into the work space and the work taking place in the new labs, over and above that already incorporated in the traditional lab.

The fixed, formal schema of hypothetico-deductive testing depends on there being conditions of action by which the schema could be made concrete. In general, intellectualist treatments of science, of the sort discussed in chapter 2, have a hard time explaining such changes. Such treatments tend to reduce the content of science to ideas and logical relations between them, the history of science to the history of ideas, and the method of science to establishing logical connections between theory and evidence, as in the hypothesis testing framework above. This reductionist approach abstracts from the material context that makes it possible to apply prior science. But it is a mistake to suppose that the application of prior science to ongoing and future work happens automatically. The experimental conditions do not fall into place all at once, and *which* background assumptions it is possible and makes sense to marshal depends on the material context within which scientists work. This context consists in part of instrumentation and the built environment, as illustrated in Figure 4.1. But it also consists of work routines—labor processes—that are the product of a multitude of intertwined social and technological processes taking place over time. Indeed, instrumentation and the built environment tend to co-evolve with the labor processes in which they are employed.

If, as was argued in chapter 2, one grants that science is in certain important ways like ordinary labor, then the question of what makes it possible to apply prior science to scientific work turns out to be a special case of a question that applies to labor in general in the modern era. The economic growth theorist Simon Kuznets claimed that “the epochal innovation that distinguishes the modern economic epoch is the extended application of science to problems of economic production.”[242] As Kuznets went on to note, the Industrial Revolution in particular marked the beginning of an epoch in which science was applied consciously and systematically to material production.

One question this experience raises is, what can be learned from it with respect to the incorporation of prior science into ongoing scientific work?

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[242] Kuznets (1966), p. 9.

This chapter is structured as follows. In the next section, the period known as the Instrumental Revolution in chemistry is introduced. This period is precisely that which witnessed the emergence of the new kind of chemistry labs shown in Figure 4.1b and 4.1c. The views, on whether and in what sense the episode was a “scientific revolution,” of the few historians who have studied the episode are also described. I argue that these analyses are flawed insofar as they ignore the crucial role of machines in this transformation. I describe structure determination before and after the Instrumental Revolution in section 4.3. In section 4.4, I extract certain ideas about the development of technology from Marxist analyses of mechanization processes in modern societies. The Industrial Revolution is their prime example, though the scope of the analyses is intended to be more general. In section 4.5, I focus on analogous and disanalogous structural properties between the Industrial and Instrumental Revolution. In section 4.6, I argue that there were common underlying factors responsible for the analogous properties, factors common to mechanization processes in modern societies. In section 4.7, I address objections to my interpretation of this episode. In conclusion, I suggest an externalist hypothesis according to which the course of science is influenced by the diffusion of principles of organizing labor that originate from outside of science. I also suggest that the cognitive consequences of radical changes in the means of production, as exemplified in the Instrumental Revolution, warrant considering whether the latter is an instance of a kind of revolution in science rather than a singular episode.

4.2 Models and machines of scientific revolutions

Chemists call the activity by which they produce claims about the structures of molecules *structure determination* or *elucidation*. The “Instrumental Revolution,” as it was dubbed by the chemist-historians Dean S. Tarbell and Ann T. Tarbell,[243] refers to a transitional period lasting roughly from the 1940s through the 1960s during which

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[243] Tarbell & Tarbell (1986), ch. 21.

powerful new sources of evidence for molecular structure were introduced in the form of modern spectroscopic instrumentation. The United States was the epicenter of these changes. Techniques such as nuclear magnetic resonance spectroscopy, mass spectrometry, infrared and ultraviolet spectroscopy, gradually displaced the chemical reaction as the principal source of evidence for structure. These techniques permitted a massive increase in the productivity of chemical analysis work and also provided access to new kinds of information on molecular structure and dynamics. Not only did the techniques change, but so did the skills needed to employ them. Cheap glassware was replaced by expensive machinery, and wet chemical skills were replaced by machine operation skills.

Historians writing about the Instrumental Revolution have advanced different views on whether and in what sense it might have been a genuine scientific revolution. On the side of those who think it was, Tarbell and Tarbell (1986) characterize it as the introduction of more powerful methods of purification and structure proof.[244] Morris and Travis (2002), for their part, characterize it in Kuhnian terms, as the overthrow of the ruling paradigm by a new one.[245] Baird (2002), on the other hand, points out that the revolutionary phase of Kuhn's *Structure of Scientific Revolutions* starts with a crisis, when normal science encounters a problem that its established methods cannot solve. Baird argues that at least as far as analytical chemistry was concerned, there was no such crisis. To the contrary, the new methods were developed in order to solve problems the established methods could already solve, but better—more efficiently, with smaller samples, greater sensitivity and lower limits of detection. Hence the Instrumental Revolution does not qualify as a revolution in Kuhn's sense.[246]

Baird goes on to examine other criteria for revolutionary status. First, he considers those advanced by I. B. Cohen (1985) in *Revolution in Science.* Baird finds that the episode does not fit Cohen's model for the stages of a revolution in science, which always begins with a private mental event. Such a model misses the core feature of the revolution, the introduction of an "instrumental-outlook" into the methods of analytical chemistry. Baird

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[244] Tarbell & Tarbell (1986), p. 335.

[245] Morris & Travis (2002) p. 80.

[246] Baird (2002), pp. 47-48.

argues that a more promising model is to be found in Hacking's (1987) notion of a "big revolution," which privileges wide-ranging changes in cultural practices and institutions in the search for scientific revolutions. Baird argues that the Instrumental Revolution fits Hacking's model, and therefore qualifies as a genuine scientific revolution.

On the side of the skeptics, Laszlo (2002) claims that there was no sudden change in the mid-20th century: the origin of organic spectroscopy should be located in the 1880s rather than the 1950s. Reinhardt (2006) concurs with Baird that the lack of anomalies and crises accompanying the changes disqualify it for the status of a Kuhnian revolution. He argues that the notion of an "Instrumental Revolution" neglects the "hidden continuities and step-by-step transition processes" that made the use of the new methods in chemistry possible.[247] According to Reinhardt, the key to assuring continuity was the emergence of a community of scientists, the "method makers," that acted as mediators or "middlemen" for the importation of methods from physics to chemistry, by way of industrial instrument-makers. The upshot of Reinhardt's account is that the Instrumental Revolution failed to be a real revolution because the transfer of technology from physicists to ordinary chemists resulted neither in the reduction of chemical theory to physics nor in a loss of chemistry's disciplinary autonomy.[248]

The general models of scientific revolution that have dominated this discussion single out changes in theories, concepts, cultural practices and institutions, but are silent on how scientific practice is altered by the specific characteristics of machines, usually lumping scientific machines under generic categories like "instrument."[249] But machines are not simply complex instruments. At least since the Industrial Revolution, they have tended to replace and displace human labor, which can have significant effects on the organization, and potential for technical progress, of the labor processes in which they are incorporated. The debate on the revolutionary status of the Instrumental Revolution has so far not considered the possibility that the revolutionary character of this event may lie in

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[247] Reinhardt (2006) p. 9.

[248] More recently, Gerontas (2014) has favored a Hacking-style interpretation and Chamizo (2018) an interpretation in terms of an extended Kuhnian model.

[249] For instance, Cohen (1985) calls both Galileo's telescope and the computer "instruments" in his discussion of their revolutionary effects on science (Cohen, 1985, pp. 9-10).

this specific characteristic of machines. Failure to do so may partially explain why neither Laszlo nor Reinhardt finds that the technology transfer brought about a revolution. Though the Tarbells note that "[t]he paucity of experimental methods and instrumentation available to organic chemists began to change with increasing speed in the 1930s," they do not comment on the causes of the speed-up, other than to point to discoveries in physics underlying the use of the new instruments.[250] Hacking's model posits four characteristics of "big revolutions" that have little to do with machines as such: discipline formation, the establishment of new social institutions like national science academies, large-scale social changes like the rise of capitalism, and changes in the "texture" of the world, such as when a probabilistic world-view displaced deterministic conceptions of the world.[251]

In this paper, I argue that the Instrumental Revolution bears a striking resemblance to the industrial one.[252] I begin by offering grounds for thinking that the resemblance is not fortuitous, but rather reflects a general pattern of development caused by the mechanization of the labor process, drawing largely on evidence from structural organic chemistry. Though my focus will be on the latter, I will also draw evidence from analytical chemistry, which in some ways was more profoundly affected because its professional identity was based on methods of analysis.[253]

My analytical approach here is inspired by two sources. First, I draw on philosopher Maurice Mandelbaum's notion of an 'analogical approach' to comparative historical studies.[254] This approach consists of two complementary subtypes. The 'phenomenological form' "rests on analogies drawn between instances that resemble one another with respect to certain overall characteristics of structure, such as the sequence of stages in revolutions,

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[250] Tarbell & Tarbell (1986), p. 335.

[251] Hacking (1987), pp. 50-52.

[252] For a discussion of the conceptual and semantic difficulties associated with the term "Industrial Revolution," see Cohen (1985), ch. 17. In this paper, I use the term to refer to the transition from the period of manufacture to the period of large-scale industry in the 18th and 19th centuries, as analyzed by Marx in *Capital*.

[253] Baird (2002) argues that analytical chemists experienced a crisis of identity during this period.

[254] Mandelbaum (1984), pp. 135-139. I thank Professor James Lennox for making me aware of Mandelbaum's writings on the philosophy of history.

or some interrelated set of attributes that, taken together, are seen as constituting a specific ideal type.” The phenomenological form can be complemented by the ‘analytical form,’ which invokes underlying relationships in order to explain the similarities the phenomenological comparison merely describes. My ‘phenomenological’ claim in this paper is that the Instrumental Revolution resembles the industrial one with respect to eight structural properties that the two events have in common.

A few remarks on the intended scope of the analogy are in order here. I intend the analogy to apply to routine structure determination in organic chemistry. Since defining precisely what is meant by “routine” can be difficult, I will instead characterize it in terms of “subjective” and “objective” aspects. The subjective aspect of routine structure determination post-Instrumental Revolution is that it does not require, of the organic chemist, research and expertise on the methods, instrumentation, and theory of the instrumentation used to determine structures. I have in mind the kind of chemist who would be intended by the following statement of aims from a textbook on spectrometric identification of organic compounds:

> We aim at a rather modest level of expertise in each area of spectrometry, recognizing that the organic chemist wants to get on with the task of identifying the compound without first mastering arcane areas of electronic engineering and quantum mechanics. But the alternative black-box approach is not acceptable either. We avoid these extremes with a pictorial, nonmathematical, vector-diagram approach to theory and instrumentation. Since NMR spectra can be interpreted in exquisite detail with some mastery of theory, we present theory in corresponding detail—but still descriptive.[255]

The objective aspect is that the instruments have to be black-boxed, in the sense of Latour (1999) who defines black-boxing as

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[255] Silverstein & Webster (1998), p. 1. I note in passing that the vector-diagram approach referred to in the quotation is a classical model of the bulk magnetization and therefore does not provide the accepted quantum mechanical explanation of NMR phenomena in terms of superposition states and product operators. It is nevertheless useful for teaching the kind of qualitative understanding the authors are aiming at.

> An expression from the sociology of science that refers to the way scientific and technical work is made invisible by its own success. When a machine runs efficiently, when a matter of fact is settled, one need focus only on its inputs and outputs and not on its internal complexity.[256]

The invisibility of the machine's internal complexity makes it capable of being operated by someone who is not an instrument expert. Indeed, it is precisely this capability of the instruments discussed in this paper that makes possible the black-box approach resisted by the textbook quoted above. For example, another textbook claims that "[i]t is possible to treat the NMR spectrometer as a 'magic box' and simply memorize a few rules that suffice for deducing the structure of a compound from its spectrum."[257] My focus on black-boxed instruments entails that I will be concerned with the use of standard tools like a tabletop infrared spectrometer rather than with that of a high-end research instrument like a 1 GHz NMR machine.[258]

These constraints on the scope of the analogy exclude, for example, researchers who use NMR to study large biological macromolecules, which does indeed require mastery of and research on the methods, instrumentation and theory of the instrumentation. I will also not be concerned with the methods used to produce the final instrument commodities (e.g., mass production versus custom manufacture) nor with extending the analogy to the social groups involved in the research, development and production of the instruments.

My second inspiration is Marx's analysis of the Industrial Revolution, which I draw on to formulate my 'analytical' claim. Unlike traditional Marxist historiography of science, however, my concern here is not primarily with the social origins of a scientific development.[259] Rather, I focus on changes in the labor process internal to the field. In *Capital*, Marx argues that the extensive, rapid and indefinite application of science and technology to productive processes under capitalism was made possible by the

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[256] Latour (1999), p. 304.

[257] Streitwieser, Heathcock & Kosower (1992), p. 325.

[258] I thank an anonymous reviewer for these examples. The reviewer further points out that the difference between these sorts of instruments is similar to that between the production and use of a Toyota Corolla and a formula racecar.

[259] For an overview of 20th century Marxist historiography of science, see Hadden (1994), ch.1.

emancipation of factory production from the limitations imposed by native human abilities. This emancipation was brought about by modification of the labor process, in particular the modification of what I will call "strategic functions" within the process. The modification of the function of tool-bearing in particular permitted a sequence of further transformations that exploited science and technology. My analytical claim is that something similar happened in chemistry, namely that the Instrumental Revolution also involved the emancipation of data production from the limitations imposed by humans' native epistemic abilities. The strategic function in this case was that of *detection*.

4.3 Structure determination before and after the instrumental revolution

In general, the goal of structure determination is to determine the connections between atoms in a molecule, and often the geometric properties of the molecule as well. With the acceptance of chemical structure theory in the late 19th century, chemists could turn the observations furnished by chemical reactions into evidence for molecular structure.[260] Structure determination became one of the major activities of the field.

The classical era of structure determination stretched from the 1860s to the 1950s, during which time chemists determined the structures of many complex natural products, including dyes, pigments, alkaloids, vitamins and hormones. The determination of complex structures using chemical "wet" methods was extremely time-consuming, often taking decades and sometimes even leading to the award of Nobel prizes.[261] A famous example is strychnine, which was isolated in 1815 but whose structure was not definitively established until 1948 despite intensive efforts to do so: at least 245 papers were contributed to solving it from the time of strychnine's isolation to 1950, and one of the principals in the field,

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[260] Sidgwick (1936) offers a clear and concise description of the theory and its development up to 1936. See Brock (1993), ch. 7 for a treatment of the rise of structure theory.

[261] A list is provided in Morris & Travis (2002), p. 60.

Robert Robinson, was even awarded a Nobel Prize for his "investigations in plant products of biological importance."[262]

Classical chemistry was heavily dependent on the performance of manual work by the chemist.[263] It was also conservative in its methods, as this quote from the chemist-turned-historian David Knight illustrates well:

> The chemistry that I learned in school and at university in the 1950s was essentially nineteenth-century … To someone with my training, the history of chemistry in its golden age … was accessible. It was no surprise that Jacob Berzelius [1779-1848] should have written a whole book about using the blowpipe, or Michael Faraday [1791-1867] a stout volume on Chemical Manipulation [1827] (still full of useful tips to my generation, on weighing, getting ground-glass stoppers out of bottles, and distilling); or that William Ramsay [1852-1916] prided himself on his glassblowing … Physicists might look upon them as upgraded cooks; but chemists knew that they had learned a craft the hard way. They did not work with black boxes but with the transparency of glassware. Buying in apparatus was time-saving but not essential, and the really good chemist could be his own technician … Chemists also perceived the danger that an expensive toy … will be played with in time that, with more thought and less gadgetry, might be used for real discovery.264

In contrast with these 19th-century methods, the Instrumental Revolution ushered in new techniques based on physics, notably quantum mechanics. For example, nuclear magnetic resonance (NMR), one of the most powerful techniques of structure determination to emerge from this period, is the study of the properties of molecules containing magnetic nuclei. A magnetic field is applied and the frequencies at which the nuclei come into resonance with an oscillating electromagnetic field are observed. These frequencies depend on the chemical environment of the nuclei, and so the characteristic

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[262] "The Nobel Prize in Chemistry 1947". Nobelprize.org. Nobel Media AB 2014. Web. http://www.nobelprize.org/nobel_prizes/chemistry/laureates/1947/. (Accessed 24 July, 2015). See Slater (2001) for an account of the strychnine research.

[263] Modern chemistry continues to be dependent on manual chemical manipulations, though the field of application has changed (e.g., to synthesis) and labware is largely purchased rather than made in-house.

[264] Knight (2002), pp. 87-90; cf. Tarbell & Tarbell (1986), p. 335. Knight's comment about apparatus agrees with Jackson's (2015b) claim that "chemistry's move into home blown hollow glassware around 1830 … made it possible for chemists to work independently of professional instrument makers" (p. 189).

frequencies absorbed by a molecule provide evidence for its structure. The technique is of great importance for the structural analysis of organic molecules, like proteins, which contain magnetic ^{1}H and ^{13}C nuclei. A schematic of an NMR spectrometer is shown in Figure 1. At the core of the instrument is a superconducting magnet, into which the probe containing the sample is inserted. Most of the apparatus is devoted to the generation, transmission and processing of a signal. The whole process is controlled by a device known as the pulse programmer ((4) in the diagram). The human operator types instructions at the computer, which are then loaded into the pulse programmer and executed from there. The operator also gives instructions for displaying, plotting and analyzing the data for structural information.

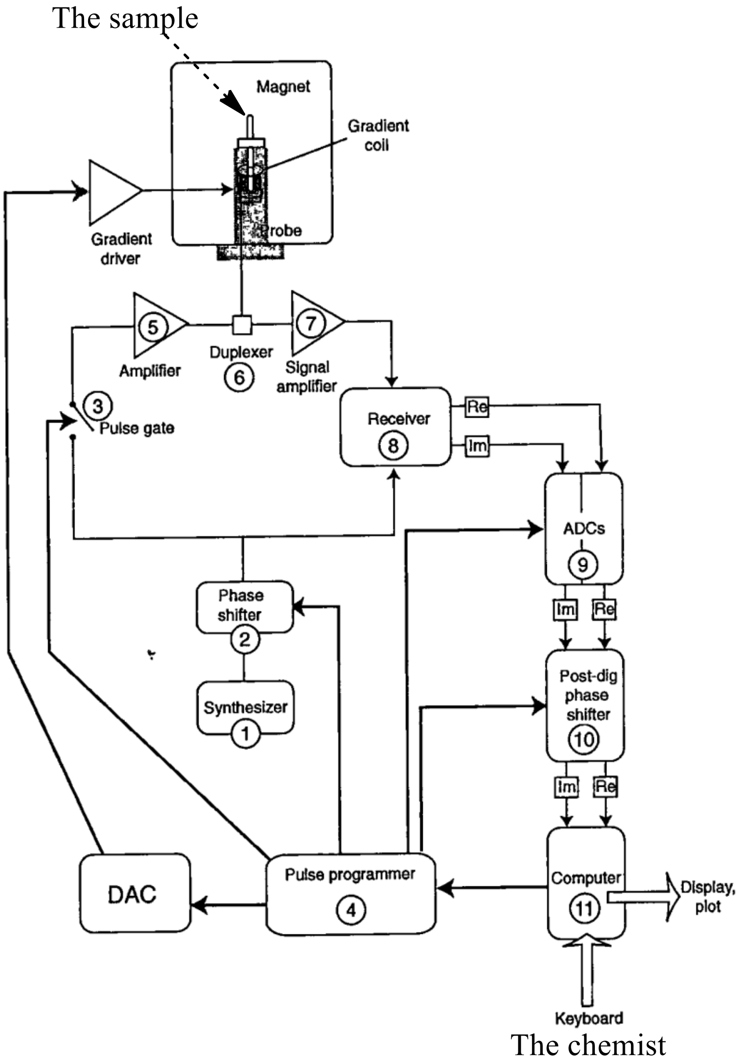

Figure 4.2 The scene of the "crime": a schematic overview of a pulsed-field NMR spectrometer. Source: Levitt (2008, p. 81).

As the schematic suggests, the spectrometer is a very complex combination of scientific principles and technology, drawing on a variety of fields including physics, electronics, computer science and mathematics.

(a)

In 15 minutes, any operator can learn to run

Varian's new T-60, an NMR spectrometer with spin-decoupling capability

and it's priced under $20,000! In that 15 minutes, the value of the T-60 is apparent. Operational ease, for instance. Once the pre-operational checkout is done, operation in the automatic mode merely requires pushing a selector button and adjusting an amplitude control. After that, in routine analysis, you can get 50-100 spectra daily. Sample handling is equally simple. The sample tube, dropped into its outlet, floats on a cushion of compressed air, and pops out for easy removal. Direct-reading spinning speed is another handy feature.

As to sensitivity, the T-60 has a S/N ratio of 12:1. Its resolution is 0.5 Hz. You won't find that level of performance in similar NMR instruments until you part with thousands of dollars more.

Significantly, Varian's T-60 has field- and frequency-swept, spin-decoupling capability, enabling the unit to achieve a complete-system status. The Spin Decoupler gives you simplified, more readily identifiable spectra. Available accessories enhance the system capability. Solid-state construction throughout provides an instrument with minimal maintenance requirements.

The T-60 is a compact, single-console design that houses all basic components of an NMR spectrometer. That means convenience, too. Operating costs are low because the T-60 draws only 200 W of power and because the permanent, temperature-controlled magnet avoids use of cooling water or air conditioning.

When you buy, you also get Varian's complete customer service: applications chemistry, service engineering, workshops, and seminars—to mention a few!

It's all down in the T-60 brochure; request a copy, as well as the new NMR primer, from your local Varian representative or write to Varian, Analytical Instrument Division, 611 Hansen Way, Palo Alto, California 94303. Ask for Datafile N1-3.

varian
analytical instrument division
611 hansen way/palo alto/california 94303

(b)

Figure 4.3 Instrument advertisements are interesting for what they reveal about how the makers conceived the relationship of the user to the machine. (a) A 1968 Varian Associates advertisement stressing ease of operation announces an NMR spectrometer for routine use by the average chemist. Source: Analytical Chemistry, 40, 125A. (b) With the help of a sexist double-entendre, Varian emphasizes that the chief locus of "activity" is in the machine rather than the human operator. Source: Analytical Chemistry, 1979, 50, 933A. It should be noted that, contrary to what the juxtaposition with Figure 4.3b might suggest, the Varian T-60 had neither a superconducting magnet nor a pulse programmer. On the other hand, the XL-200, introduced in 1978, was equipped with both components.

These instruments were based on science and technology with which chemists were largely unfamiliar. They were also very expensive. Nevertheless, there was a significant pay-off for using them. Structure determination became much more efficient, freeing up the chemists' time for other work, such as synthesis (which was not mechanized and where chemical expertise remained absolutely essential) or chemical applications in biology. Moreover, more complex targets could be tackled, for example biological macromolecules. The pay-off is evident in the case of strychnine mentioned above. Whereas over 245 papers were contributed over 60 years towards solving strychnine chemically,[265] only 6 were required over 5 years for the independent solution of the X-ray structure (Figure 3).[266]

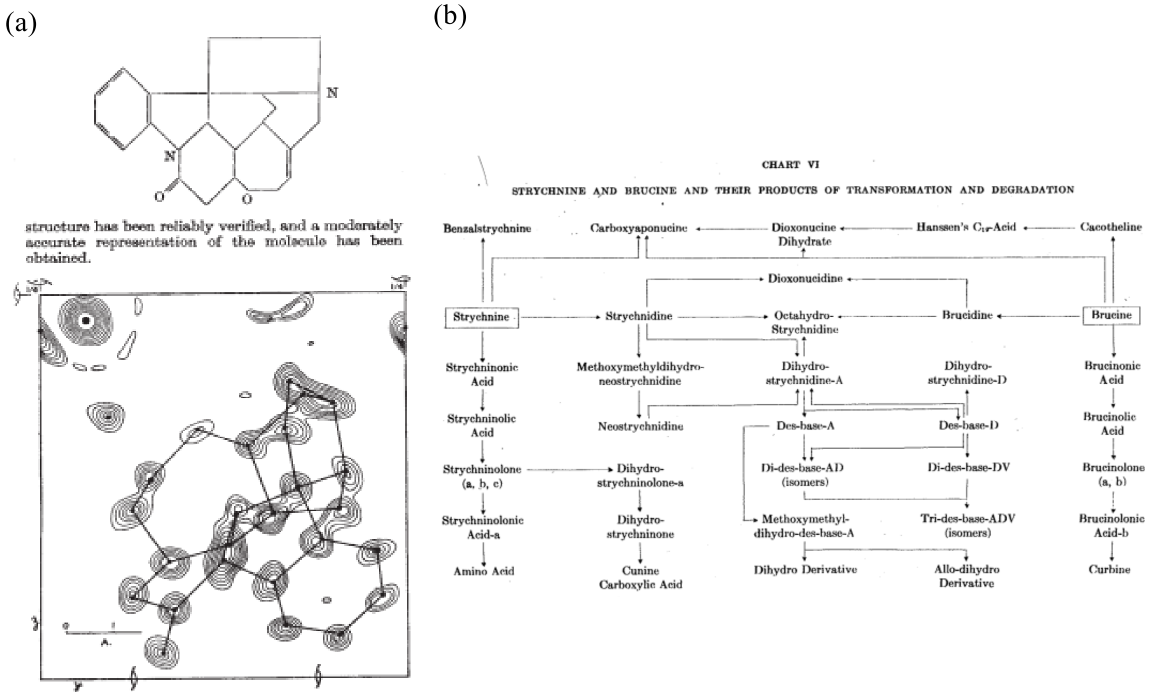

Figure 4.4 (a) These diagrams are from one of the first reports on the crystal structure of strychnine. The top diagram is a two-dimensional representation of strychnine. The bottom structure has been superimposed. Source: Robertson & Beevers (1950), p. 690. (b) The network of chemical transformations observed in the classical determination of the structure of strychnine and the closely related brucine. Source: Holmes (1950), p. 419.m diagram is a contour map of the electron density on which a projection of the

This pay-off was accompanied by a significant change in the way structure determination was conducted. Whereas skills of chemical manipulation lay at the center of the classical methods, the new methods were centered on the interaction of machines with chemical samples.

In classical chemistry, the chemist would develop evidence for the structure of a substance by carrying out a set of manipulations on it. The means he employed were

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[265] Huisgen (1950).

[266] See the primary sources cited in Slater (2001), footnote 78.

chemical reagents, glassware and auxiliary tools like balances, heating sources, stirrers, stills, and pumps. By these means, the chemist would set chemical processes in train by means of various manual operations (weighing, adding, dissolving, heating, filtering, washing, drying, purifying, etc.). A chemist could identify the structure of an unknown by running it through a series of such processes designed to identify the various functional groups and their location in the carbon skeleton. The success of chemical research was heavily dependent on manipulative skills, as noted by Faraday in 1827.[267] The chemist would then interpret the results in terms of hypothesized structures. For example, chemists might accept such a hypothesis on the grounds that it best explained the substance's reactivity.[268] The interpretation of the results was often quite involved, requiring considerable chemical knowledge together with acumen for piecing together the results of reactions in terms of a structure.[269]

How do chemists use physical methods to obtain evidence for structure? Rothbart and Slayden (1994) provide an abstract description of spectrometers as "complex systems of detecting, transforming and processing information from an input event, typically an instrument/specimen interface, to some output event, typically a readout of information."[270] In spectroscopy the input event is the absorption or emission of electromagnetic radiation by molecules. Their response to the radiation generates a signal that carries information about the structure. The signal is transmitted by a "complex causal sequence of physical events from the specimen/instrument interaction to the readout."[271]

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[267] Faraday (1827), iii.

[268] For an example of how evidence for a structural hypothesis was developed in classical chemistry, see Slater's (2001). Sir Robert Robinson has many examples in his (1976) autobiography. The textbooks by Mulliken (1904) and Shriner, Fuson & Curtin (1956, 3rd ed.) provide a systematic overview of classical methods.

[269] As attested in comments by veterans like R. B. Woodward (1963), Max Tishler (1983), and A. J. Birch (1995) (Woodward, 1963, p. 248; Tishler, 1983, p. 12; Birch, 1995, p. 22 and pp. 56-57). Textbooks from the mid-20th century contrasted the intellectual complexity of classical structure determination, which they sometimes compared to solving a jig-saw puzzle, to the simplicity of the new methods. See, for example, Wheland (1949), p. 127 and Allinger & Allinger (1965), p. 36.

[270] Rothbart & Slayden (1994), p. 29.

[271] Rothbart & Slayden (1994), p. 37.

In modern chemistry, structures are determined by inserting an isolated sample into such systems. The chemist is generally not the designer of the instrument. In routine use, she or he must prepare the sample, choose the kinds of experiment to use, operate the instrument, and interpret the spectrum, though in routine cases these operations are fairly standard. Her chemical laboratory skills are limited to sample preparation, for example dissolving the sample in an appropriate solvent or crystallizing the substance.

If the basis of classical structure determination was the chemist's set of chemical laboratory skills together with his knowledge of chemical substances and their reactions, the basis of modern structure determination is the combination and adaptation of natural systems for the purpose of generating a signal that carries information about the specimen's structure. Though chemical skills are involved in sample preparation, the production of information from the sample depends not on them but on whether this orchestration of systems is such as to produce a reliable signal.

Thus the new instruments did not transfer the skills needed for classical structure determination to the machine. Rather, they substituted a new process for the old one. In the new process, the goal of structure determination was attained without the use of chemical reactions. This is an instance of what the sociologists of science Peter Keating, Camille Limoges and Alberto Cambrosio call 'automation.' According to them,

> Successful automation is not … an automated mimicking of human operations by a more efficient machine, but a *substitution* of one process (involving more than humans) by another. What we have is not a deskilling of humans by embedding human skills in an automaton, but the creation of an emerging new field of operations that redistributes actions between humans and machines and between the humans themselves. The emphasis here is on actions, not skills.[272]

The notion of automation advanced by these authors differs significantly from the usual notion, as the use of a machine to substitute for human action by mimicking the latter through mechanical operations. For Keating, Limoges and Cambrosio, the automated process may involve operations that are quite different from—do not mimic—the human one. In addition, they do not simply replace humans but rather change the kinds of actions performed by humans. Automation through mimicry is merely a special case of this more general kind of automation. Nevertheless, I think the usual notion of automation as

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[272] Keating, Limoges & Cambrosio (1997), p. 132.

mechanical mimicry is still useful because it indicates an essential feature of automation, which is that some phases of the production process are delegated from humans to machines. Moreover, these phases must happen "automatically," i.e. the machine must be able to carry out tasks without human intervention. But the manner in which the machines carry out those phases can be quite different. In the chemical case, the thing produced was a structural representation of a compound (as in Figure 1a), and this basically did not change. The Instrumental Revolution brought about a drastic change in *how* it was produced, however, involving a transition from a human-centered process to a machine-centered process.

4.4 Marx's analysis of the labor process and the industrial revolution

As suggested by my reference to the "labor process", my view starts from the assumption that science can be accurately conceptualized as a material labor process, similar in important respects to ordinary labor processes.[273] In this article, I draw on Marx's analysis of the mechanization of industrial labor processes during the Industrial Revolution because the pattern of development of the Instrumental Revolution in certain respects fits rather well the pattern described in his analysis. I will restrict myself here to summarizing the key methodological points required for understanding my position. It should also be noted that what I take from Marx is conceptual rather than empirical. Hence I will be concerned with his way of conceptualizing what made the technical changes during the Industrial Revolution possible, and not with the truth of his empirical claims concerning the course of technical change in capitalist economies during the 18th and 19th centuries, except insofar as these affect the cogency of the conceptualization.[274]

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[273] For a recent defense of this assumption, see Lefèvre (2005).

[274] That said, those aspects of Marx's account of the Industrial Revolution that I will use here appear to be in broad agreement with more recent scholarship. See Allen (2017), especially ch. 3 on "Why the Industrial Revolution was British" and the references cited therein.

The non-Marxist economic growth theorist Simon Kuznets claimed that "the epochal innovation that distinguishes the modern economic epoch is the extended application of science to problems of economic production."[275] One question this claim raises is what makes the extended application of science to production possible? An obvious answer is the growth of scientific knowledge. But as economist Nathan Rosenberg noted in his interesting (1981) study of Marx's ideas on technology, the growth of science is not a sufficient condition for the application of scientific knowledge to the production process.[276] To believe that is to ignore the mediating role of technology in the production process. Marx himself characterizes the labor process in general in terms of three simple elements:

> The simple elements of the labour process are (1) purposeful activity (*zweckmässige Tätigkeit*), that is work itself, (2) the object on which that work is performed, and (3) the instruments of that work.[277]

Technology, in the form of the instruments of labor, mediates the process of transforming the object of labor and hence of realizing the worker's purposes. But

> not all technologies will permit, or will permit in equal degrees, the *application* of scientific knowledge to the productive sphere … It was one of Marx's most important accomplishments to have posed precisely this question: What are the characteristics of technologies which make it possible to apply scientific knowledge to the productive sphere?[278]

Science offers possibilities for enhancing the productivity of labor. The realization of these possibilities depends, however, on how the agents of production assign functions to people and things in the labor process. The distribution of those functions has a determining effect on the technological dynamism of production. In *Capital*, Marx analyzes two different ways of distributing those functions, what he calls "manufacture" (*Manufaktur)*, the predominant mode of capitalist production from the mid-16th century to the last third of the 18th,[279] and what he calls "large-scale industry" (*die groβe Industrie*), the mode of production that succeeded it. Manufacture was based on a division of labor

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[275] Kuznets (1966), p. 9.

[276] Rosenberg (1981), p. 15.

[277] Marx (1976 [1867]), p. 284; (1959), p. 193 for the original German.

[278] Rosenberg (1981), p. 15.

[279] Marx (1976), p. 455.

between specialized workers wielding manual implements, an arrangement Marx calls the "subjective principle" of the division of labor in manufacture.[280] This "principle" encountered the limitation that:

> Whether complex or simple, each operation has to be done by hand, retains the character of a handicraft, and is therefore dependent on the strength, skill, quickness and sureness with which the individual worker manipulates his tools. Handicraft remains the basis, a technically narrow basis which excludes a really scientific division of the production process into its component parts, since every partial process undergone by the product must be capable of being done by hand, and of forming a separate handicraft.[281]

Rosenberg sums up the problem neatly:

> Although … the manufacturing system achieved a growth of productivity through the exploitation of a new and more extensive division of labor, a rigid ceiling to the growth of productivity continued to be imposed by limitations of human strength, speed and accuracy … Science … cannot be incorporated into technologies dominated by large-scale human interventions.[282]

How was this problem solved? By the use of machines, of course, for "machinery may be relied upon to behave in accordance with scientifically established physical relationships."[283] The worker's skills can now be replaced by non-human natural forces, thereby lifting the barrier to innovation posed by the limited abilities of human workers. Doing so permits the continual and free development of production by the "conscious application" of "the whole range of the natural sciences."[284] Innovation in production is all the more accelerated by the fact that science and technology develop synergistically, with advances in the former making possible breakthroughs in the latter, and vice-versa.[285]

It may seem that the causality implied in the last paragraph is the wrong way around. Wasn't it the application of science and technology that made possible the emancipation from native human abilities? But key to Marx's analysis of industrialization is the idea that the labor process has a structure involving functional relationships between

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[280] Marx (1976), p. 501.

[281] Marx (1976), p. 457.

[282] Rosenberg (1981), p. 16.

[283] Rosenberg (1981), p. 16.

[284] Marx (1976), pp. 590, 616-617.

[285] Marx (1976), pp. 505, 508-509.

the worker, the instruments and the object of labor. For him, the key step in the Industrial Revolution was the transfer of the tool-bearing function from workers to “mechanisms”:

> The machine, which is the starting-point of the industrial revolution, replaces the worker, who handles a single tool, by a mechanism operating with a number of similar tools and set in motion by a single motive power, whatever the form of that power.[286]

Though this move obviously depended on prior knowledge, it permitted much greater application of science and technology by allowing modifications of the tool-bearing mechanism as well as connections with other kinds of machinery, like engines. In general, the first steps in a process of mechanization may be fairly crude, as the potential for applying science and technology to it is only realized gradually.

My suggestion, based on this analysis, is that labor processes contain what I will call ‘strategic functions’. What makes a function “strategic” is that its modification makes possible a pathway of transformations that might not be accessible from other starting-points. A relatively simple example is the development of the water frame. In Europe, up to about 1300 CE, fibers were spun into yarn by means of hand spindles. A single worker could manipulate one spindle at a time. In the late Middle Ages, the spinning wheel came into use. Here the spindle was mounted on a post and set in motion by using hand or foot to drive a large wheel attached to the spindle by a pulley. The drawing and twisting of the fiber was done by hand as the spindle rotated. Hargreaves’ invention of the spinning jenny in 1764 made it possible to operate dozens of spindles simultaneously, because both the spindles and the fiber were now manipulated by a mechanical apparatus that was not limited by the number of arms in a human body. The mechanism itself was still driven by human force, however. Arkwright’s water frame of 1769 was based on the same principle of mechanical spindle manipulation, but exploited the fact that human motive power had been made dispensable by the transfer of the spindle to a mechanism. The frame was driven by a shaft that allowed it to be connected to a water wheel, thus allowing water power to be harnessed. Not only did this improve the productivity of the individual machine, but it

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[286] Marx (1976), pp. 497.

allowed many machines to be connected by a transmission mechanism and so powered simultaneously by the same wheel, which further increased productivity.[287]

In this example, manipulation of the spindle played the role of strategic function. It was strategic because control over the tool had to be changed before water power could be exploited. The sequence could not have started with the application of water power, since the human arm is not easily separable from its owner. This priority does not exclude that the two changes could occur simultaneously, in the same invention, say. The priority is logical, not temporal.

There can be more than one strategic function in a given production process. Rather than modify the tool-bearing function, for example, employers in the manufacturing period preferred to modify the operations performed by the worker. According to Marx's account, this modification began with the decomposition of a single process formerly performed by a single craftsman into simpler operations, each performed by a specialized worker. Though the initial effect is simplification, specialization eventually leads to perfecting the methods and skills of the worker. Specialization also increases productivity by eliminating transitions from one partial operation to another. The full exploitation of the specialization of labor requires changes in the instruments of labor, for these must be adapted to the new skills. Furthermore, splitting up the original process into partial operations allows the latter to be carried on simultaneously, leading to a further gain of total productivity. Since the partial operations are performed by different workers, the continuity of the overall process depends on each worker spending no more time than necessary to complete his or her designated function, leading to an increase in efficiency. Finally, specialization allows differences among individuals to be developed, insofar as some will specialize in operations requiring more strength, others more skill, attention, intellectual effort, etc. All of these changes in the nature of the work, however, were made possible by the initial simplifying decomposition.[288]

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[287] Hills (1990), pp. 808-830; Usher (1954), ch. XI, section VI; Fitton & Wadsworth (1958), pp. 211 (photograph facing) and p. 217.

[288] Marx (1976), ch. 14.

Clearly, any significant process of technical change will involve more than is suggested by these sorts of linear descriptions. Moreover, not all changes in work may be amenable to an analysis in terms of strategic functions at all. I merely suggest that, in some cases, it may be a useful analytical concept.

The transformation of a strategic function can make it more feasible to apply science and technology to the labor process. For example, the application of theories of heat and work to production, say in the form of the steam-engine, is made possible by the mechanization of tool manipulation. In section 5, I will argue that detection played the role of a strategic function in chemical analytical instrumentation.

Finally, it should be noted that these changes at the level of the labor process have cognitive counterparts. The "subjective principle" of manufacture involved, at the cognitive level, the assumption that however the production process was to be organized, each partial process carried out within it was to be done manually. The successor principle, which Marx sometimes calls the "principle of machine production", cognitively involved the discarding of this assumption:

> In manufacture, it is the workers who, either singly or in groups, must carry on each particular process with their manual implements. The worker has been appropriated by the process; but the process had previously to be adapted to the worker. This subjective principle of the division of labour no longer exists in production by machinery. Here the total process is examined objectively, viewed in and for itself, and analysed into its constitutive phases. The problem of how to execute each particular process, and to bind the different partial processes together into a whole, is solved by the aid of machines, chemistry, etc. But of course, in this case too, the theoretical conception must be perfected by accumulated experience on a large scale.[289]
>
> The principle of machine production, namely the division of the production process into its constituent phases, and the solution of the problems arising from this by the application of mechanics, chemistry and the whole range of the natural sciences, now plays the determining role everywhere.[290]

It is worth noting that the "principle of machine production" does not refer narrowly to production by means of devices that mimic human action (e.g., by bearing tools), but involves a problem-solving approach that draws on the entire store of scientific and

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[289] Marx (1976), p. 501.

[290] Marx (1976), p. 590.

technological knowledge. This approach was not employed by the direct operators on the factory floor (often relatively unskilled workers, including women and children) but rather by the owners and designers of the instruments: capitalists, inventors, engineers, etc. Thus, the ultimate import of the transfer of the tool-bearing function is that it paved the way for a much broader change in how problems of production were conceived and solved. This new way of thinking about production led in turn to further changes in economic production, which went far beyond the mere transfer of tools from one kind of bearer to another. I will provide evidence that a similar way of thinking, which one might call a "principle of machine production of data", was influential in the Instrumental Revolution.

In short, what I take from Marx's analysis of the Industrial Revolution are the following ideas. First, that the degree in which the technologies used in a particular production process are dependent on native human abilities affects the possibility of applying scientific and technological knowledge to it. Second, that this degree of dependence is reflected in the problem-solving approaches used to address problems of production. Third, that labor processes contain "strategic functions" the transformation of which makes possible a pathway of transformations that might not be accessible from other starting-points. In some cases, the pathway of transformations may involve the extensive application of scientific and technological knowledge.

4.5 Parallels between the Industrial Revolution and the Instrumental Revolution

In this section, I discuss two sets of evidence suggesting a relationship between the Instrumental Revolution and the Industrial one. First, the conceptions of progress of some of the participants in the Instrumental Revolution were formulated in terms of features characteristic of industrial production. Second, the two events share eight common features with respect to how their respective labor processes were altered.

4.5.1 Conceptions of progress

In some cases, the Instrumental Revolution was actually characterized by the participants in terms alluding to large-scale industry and the use of machines outside of science. For example, John K. Taylor of the Center for Analytical Chemistry at the National Bureau of Standards commented in 1985:

> Chemical analysis is undergoing a change of operational mode similar to the industrial revolution of a century ago … The trend is from individual craftsmanship to mechanical outputs, using apparatus and equipment that is often poorly understood by the technical operator.[291]

In the forward to a book on the DENDRAL project, an attempt to automate structure elucidation by mass spectrometry, the noted organic chemist Carl Djerassi wrote in 1980 that "[i]t is [in synthesis] where the use of computers has not been widely accepted because of the fear that thinking man will simply be reduced to an appendage to a machine."[292] Joshua Lederberg, one of the project leaders, dreamt of "mechanizing" scientific thinking in biology and organic chemistry and reducing the human role to one of management:

> If we could give biology sufficient formal structure, it might be possible to mechanize some of the processes of scientific thinking itself … Could not the computer be of great assistance in the elaboration of novel and valid theories? We can dream of machines that would not only execute experiments in physical and chemical biology but also help design them, subject to the managerial control and ultimate wisdom of their human programmer.[293]

Comparisons to instruments used outside of science were also made. For example, Djerassi compared X-ray diffraction to the flash camera, and the analytical chemist H. A. Liebhafsky drew an analogy between the introduction of the new instruments and the mechanization of artillery, judging such "revolutions" to be "necessary."[294]

Such quotations provide grounds for thinking that participants in the Instrumental Revolution were influenced by ideas derived from examples of mechanization in the

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[291] Taylor (1985), p. 6.

[292] Djerassi (1980), ix.

[293] Lederberg (1969), p. 38.

[294] Djerassi (1992), p. 84; Liebhafsky (1962), p. 32A.

broader society. In the next subsection, I will show that their ideas corresponded to structural similarities between the two events.

4.5.2 Common features

I have identified eight features common to both events, considered as transformations of their respective labor processes:

I. The labor process no longer uses means and methods borrowed from an antecedently existing activity. It acquires means and methods specifically adapted to its purpose.

Classical structure determination found its means ready-made in the technology of substance manipulation. These means had several drawbacks for the productivity of structure determination, including drawbacks such as that: large amounts of substance were required; the processes employed were time-consuming; the variety of evidence for structural claims was poor; and the principal evidence used, that provided by chemical properties, tended to underdetermine the structures identified by means of it. Consequently, the development of the productivity of structure determination required that means better suited to this end be found. Spectroscopic methods were very effective for this purpose: they require only small amounts of substance; they are rapid; they come in many varieties; and they are better at uniquely identifying functional groups and connectivities. Analogously, during the Industrial Revolution, capitalists transformed the production processes they had inherited from the medieval handicrafts to fit the needs of a capitalist economy, in particular the need to increase profits without increasing the length of the working day. This was achieved by increasing productivity through technological innovation.[295]

II. The labor process becomes centered around an instrument rather than the worker.

As I discussed in section 2, classical structure determination was essentially based on the chemical laboratory skills of the chemist. These skills are marginalized in modern

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[295] Marx (1976), ch. 12.

structure determination. The labor process is now essentially based on machines. While methods developers focus on optimizing the functioning and extending the scope of the machines, the average user types in standardized instructions at the computer.[296] Experimental design, optimization and execution become based around the ability of a specific kind of machine to carry out a process that associates a specific kind of input with a specific kind of output.

III. The work becomes more capital-intensive.

A report published by the National Academy of Sciences in 1965 estimated the typical cost of a high resolution NMR spectrometer at $45,000, or $355,563 in 2017 dollars.[297] The total approximate investment in instrumentation by all university chemistry departments rose from $5 million before 1954, to $14 million in the period 1954-1959, to $36 million for the period 1960-64 alone, resulting in a total accumulated investment of $55 million. In comparison, the report estimates that a total of $31 million was spent on traditional equipment (glassware, vacuum pumps, variacs, supplies, chemicals etc.) during the same periods.[298] Thus there is evidence that even when the new instrumentation was still novel, expenditures on it outstripped traditional kinds of expenditures by a wide margin. The problem of rapid obsolescence of equipment and the attendant funding burden emerged in chemistry at this time.[299]

IV. The worker is not a specialist of the instrument.

One of the principal themes of Reinhardt's (2006) study of the Instrumental Revolution is that specialists developed methods enabling non-specialists to use the instruments.[300] This development resembles the use of workers in large-scale industry who had little knowledge of the scientific principles embodied in their machines. Methods

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[296] Reinhardt (2006) emphasizes the emergence of the methods developers as a distinct scientific community.

[297] National Academy of Sciences (1965), p. 216. 2017 price calculated using the U.S. Bureau of Labor Statistics inflation calculator, based on the Consumer Price Index.

[298] National Academy of Sciences (1965), pp. 97 and 216. Reinhardt (2006), pp. 382-386, contains a brief discussion of investment trends in research chemistry from the 1950's through the 1970s.

[299] Liebhafsky (1962), 27A.

[300] Cf. also Gerontas (2014).

developers had to create cognitive methods, like the rules of interpretation mentioned in point V below, so chemists, as routine users, could interpret the data without detailed knowledge of the science on which their instrumentation was based. In addition, textbook writers incorporated simplified theoretical treatments, connecting the workings of the machines with chemical concepts, into chemistry textbooks.[301]

V. Specialized labor is replaced by non-specialized labor.

Before the Industrial Revolution, capitalist production was based on the specialized labor of the handicraftsman; afterwards, it was based on the non-specialized labor of the machine-operator. Similarly, before the Instrumental Revolution, structure determination was based on the chemical skills of the chemist, whereas afterwards it was based on her ability to operate the machines using procedures and rules of interpretation adapted for non-specialist use.[302] The training times required to learn how to operate the machines are disproportionately short, compared either to the amount of knowledge embodied in them, or to the amount of time required to train a competent bench chemist (several years). For example, the website of the NMR facility of the University of Pittsburgh states that "a little more than an hour" is required to train a user in running basic 1-dimensional NMR experiments, 45 minutes for familiarization with basic 2-dimensional experiments, and 20 minutes for variable-temperature training.[303] And the Varian advertisement in Figure 2a promises a 15 minute training time …[304]

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[301] Slater (2002) discusses R. B. Woodward's pioneering role in the development of rules of interpretation in the context of ultraviolet spectroscopy. See also Morris & Travis (2002) and Reinhardt (2006) for discussions of the textbooks produced during this period. The simplified theory together with the instruments resemble what Fujimura (1988) calls a "standardized package" of theory and technology whose widespread adoption results in a "scientific bandwagon."

[302] On the process of adaptation, see in particular Rabkin (2002 [1987]), Bigg (2002) and Reinhardt (2006).

[303] Http://www.chem.pitt.edu/facilities/nmr-spectroscopy/training. (Accessed July 22, 2015).

[304] Though advertisements are not impartial sources, that the principle techniques discussed in this paper had either been routinized or were in the course of routinization by the early 1960s (the Varian advertisement is from 1968) is supported by textbooks of the period [Schwarz (1964), pp. 2-3; Silverstein & Bassler (1963), p. 2]. It is also supported by the manner in which the instruments were developed. In NMR, for example, a major impediment to non-specialist use was the instability of

The interpretation of the signal itself often involves no more than the use of rules that can be applied to read off structural features from the spectrum. Figure 4, for example, shows the result of one of the first attempts to correlate spectroscopic properties (here, infrared absorption) with structure. Having obtained the IR spectrum of a substance, the chemist can correlate each peak in the spectrum with functional group constituents by scanning along the abscissa, locating the wavenumber of the peak, and then scanning along the ordinate to identify the functional groups that are correlated with that wavenumber. The interpretation of spectra in this fashion is an instance of what the physical chemist J. P. C. Schwarz called an 'empirical approach' to the use of the new instrumentation. This approach depended on "the empirical correlation of certain physical properties with structural features." He contrasted this way of interpreting spectra with a 'theoretical approach', in which structure is deduced by interpreting the data in terms of the theories justifying the use of the instruments.[305]

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the magnetic fields that could be generated in the 1950s, which required individual calibration and duplication of each spectrum. This problem was overcome by the introduction of the field/frequency lock technique. The first commercial use of this technique was in the Varian A-60 spectrometer, which included a number of other design features that were intended to facilitate routine use by structure elucidation chemists. According to Becker et al. (1995), pp. 35-37, the instrument was a success, bringing NMR "to almost every chemistry laboratory as a standard analytical method." The role of the A-60 in routinizing and disseminating NMR is corroborated by Lenoir & Lécuyer (1995) and Steinhauser (2014), pp. 127-132 and p. 381. I thank two anonymous referees for pressing the point concerning advertising.

[305] Schwarz (1964), pp. 3-4.

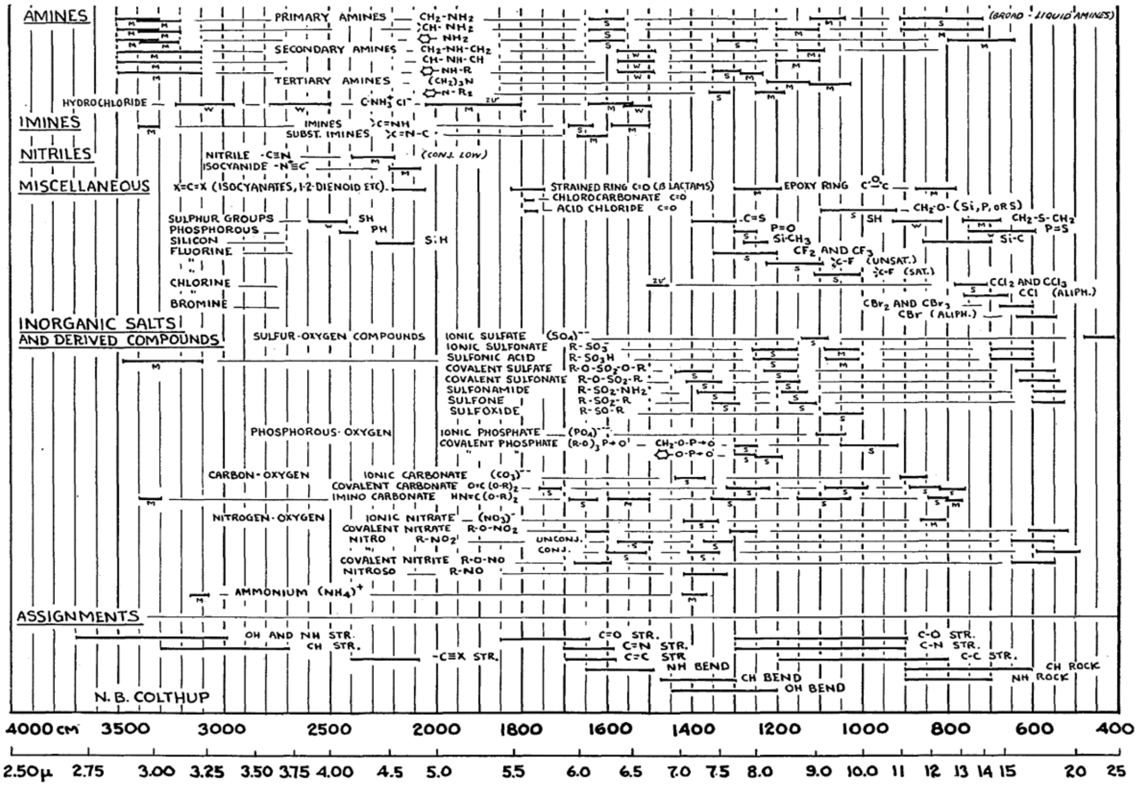

CHART 1. Probable positions of characteristic infra-red absorption bands.

Figure 4.5 An important step for the acceptance of spectroscopic methods by organic chemists was the development of simple rules of data interpretation.306 These rules were often presented in the form of charts allowing the chemist to correlate, at a glance, the observed frequencies with the presence of functional groups in the molecule. Shown above is a portion of the first such chart, published by Norman Colthup of American Cyanamid in 1950.307

VI. Automation becomes a significant feature of the production process (of goods, data)

Though the chemist must still prepare the sample, beyond this, routine use of the machines requires only insertion of the sample and the feeding of standard instructions to the instrument. In 1999, Djerassi commented on the introduction of X-ray crystallography into structure elucidation work in blunt terms: “If anyone can prove a structure with an X-ray analysis, we are nothing. The organic chemist is nothing but a little technician who crystallizes the compound and gives it to someone who sticks it in an X-ray machine, and

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[306] Slater (2002), Reinhardt (2006).

[307] Colthup (1950), pp. 398-399.

even the rest is computerized. So what's your function?"[308] Though Djerassi no doubt exaggerates the degree of automation brought about by the new methods, especially with respect to X-ray crystallography, the context of the quotation is comparative: as discussed in section 4.3, human manual intervention in the production of the data is greatly reduced relative to classical chemical manipulations. Both this feature and IV above are succinctly expressed in the following NMR textbook:

> It is one of the great virtues of NMR spectroscopy that one can use it, and indeed use it to quite a high level, without having the least idea of how the technique works. For example, we can be taught how to interpret two-dimensional spectra … in a few minutes, and similarly it does not take long to get to grips with the interpretation of NOE … difference spectra. In addition, modern spectrometers can now run quite sophisticated NMR experiments with the minimum of intervention, further obviating the need for any particular understanding on the part of the operator.[309]

VII. The cognitive and physical limitations of humans are circumvented by advances in instrumentation design.

Methods developers could now attempt to circumvent the limited cognitive and physical abilities of humans by developing the machines' computational power, automation and versatility as well as the quality and variety of the data. For example, James Shoolery of Varian Associates commented in 1995 that the introduction of programmable computers into NMR spectrometers in the late 1960s allowed a control of the instrument with:

> a speed and precision far beyond the capability of a human operator. Freed from those limitations, the development of NMR as a structural and analytical tool soon entered an exciting new period.[310]

The options for circumventing human limitations were significantly fewer in classical chemistry, resulting in a relatively conservative pattern of methodological development. This conservatism was illustrated in section 2 by the testimony of David Knight.[311] Likewise, once the tool-bearing function was transferred from man to machine,

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[308] Quoted in Reinhardt (2006), p. 170.

[309] Keeler (2010), p. 1. See also Streitwieser, Heathcock and Kosower (1992), p. 325.

[310] Shoolery (1995), p. 44. Grayson (2004) provides evidence that the computer came to play a similarly central role in mass spectrometry.

[311] Tarbell & Tarbell (1986), p. 335 and Taylor (1985) make similar observations.

industry could use more powerful motive powers than humans to drive the machine and its tools.[312]

VIII. The transformation is motivated in part in terms of productivity norms—the speed, ease, simplicity, reliability and automaticity of the new techniques.

An interesting aspect of the Instrumental Revolution is that industry pioneered the use of modern spectroscopic instrumentation in order to boost productivity before the instrumentation became widespread in academic chemistry.[313] Efficiency considerations were also adduced by academic proponents of the new methods. The physicist Paul Klopsteg, to whom the analytical chemist Ralph Müller referred in his influential 1940s column on instrumentation,[314] made efficiency the principal theme of his 1945 *Science* article on "Increasing the Productivity of Research". Commenting on the rapid recent development of instrumental methods across the sciences, Klopsteg argued for the establishment of laboratories of "instrumentology" in universities in order to increase "the output of valuable results per dollar." Instrumentology was to be a science whose goal was "*the application of science to science itself.*"[315] The chemists Silverstein and Bassler, authors of the widely used textbook *Spectrometric Identification of Organic Compounds,* argued in 1962 that the cost of the instrumentation was outweighed by the speed, small sample size and large informational pay-off made possible by it.[316]

Such productivist goals were also supported by university administrations. In the context of the Cold War, ambitious administrators could aim to maximize the output of research and graduate students by drawing on the large amounts of state and industrial

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[312] Marx (1976), pp. 497-499.

[313] See Rabkin (2002), Bigg (2002) and Reinhardt (2006) for accounts of the "detours" taken by ideas originating in physics through industry before reaching chemistry.

[314] Müller (1947), p. 24A. See Baird (2002) for a discussion of Müller's role in the Instrumental Revolution in analytical chemistry in the 1940s.

[315] Klopsteg (1945), p. 571-572. Italicized in the original.

[316] Silverstein & Bassler (1962), p. 547. See also Reinhardt (2006) for comments by John D. Roberts and William S. Johnson (both important proponents of the instrumental approach) as well as Djerassi on the labor-saving virtues of the new methods (Reinhardt, 2006, pp. 20 and 157). The scientific testimony and textbook comments in the references in footnote 270 above all compare the difficulty of classical structure determination to the relative ease, simplicity or rapidity of the modern.

funding that were then available. For example, Carl Djerassi did his pioneering work in mass spectrometry at Stanford, after moving there from Wayne State University at the invitation of ambitious provost of Stanford Frederick E. Terman. Terman was committed to developing the university in directions that would attract funding agencies and industrial companies, and had a new building built to house both Djerassi's group as well as that of Djerassi co-hire William S. Johnson. Djerassi set up a research group organized around an assembly of physical instrumentation and structured by a strict division of labor. Wet chemists supplied compounds, technicians ran the instruments, "computers"—wives of graduate students at first, then artificial computers—processed the data, and senior post-doctoral fellows interpreted the spectra.[317] In his memoirs, Djerassi characterized the vision he had for his lab during the move as that of a "quasi-socialist enterprise" run by a "benevolent dictator", possibly reflecting the influence of external methods of organizing labor on his thinking.[318]

Those who resisted the mechanization of structural chemistry found ammunition in efficiency as well. For example, Sir Robert Robinson thought the time saved by the new methods was illusory, for they revealed no chemical properties.[319] Likewise, some chemists who focused on the pedagogical consequences of the instrumental methods were concerned that the education of chemists would suffer if too much of the curriculum was devoted to the new methods, for the latter saved time at the expense of properly chemical training.[320] Such pedagogical reflections are especially interesting in light of what was said in section 3, for they underscore the fact that some classical chemists did not view structure determination merely as a process for accumulating known structures, but also as a process of apprenticeship.

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[317] Reinhardt (2006), pp. 144-173.

[318] Djerassi (1992), p. 100.

[319] Robinson (1974), p. 57.

[320] E.g., Lingane (1948), p. 2; Shriner et al. (1956), pp. v-vi; and Silverstein & Bassler (1962), p. 546.

4.5.3 Disanalogies

As with any analogy, there are respects in which this one breaks down. The transformation of labor affected only one, albeit important, activity, though there are efforts currently afoot to mechanize synthesis as well.[321] Chemistry never became Big Science, but largely continued to favor small-scale projects. The delegation of expertise to specialists was not total, since some knowledge of how the instruments work remained desirable for data interpretation. And as noted in section 2, there was no attempt to mechanize the tools of classical chemistry.

Perhaps the most important disanalogy has to do with the social groups driving the change. As pointed out under (I) above, during the Industrial Revolution capitalists transformed production processes to increase profits through technological innovation. It is unclear who the equivalent actors to the capitalists might be in the chemical case, or what goal plays the role of profit. With respect to the actors, previous studies on the introduction of the instrumental methods in chemistry show that the actors were small instrument manufacturers, research technologists,[322] scientists working as method makers or lead users, officers of funding agencies, and university administrators. This motley group of actors is very different from the owners of the means of production central to Marxist theory. Though profit certainly motivated the manufacturers, it is doubtful that it was as important a motivation to the other members of the group. Prestige would seem to be more important in these cases.

It might also seem like a stretch to compare 20th century organic chemists to early industrial factory workers. I only claim, however, that in both cases, mechanization allowed the operators to treat the new instruments more or less like black-boxes. The import of the analogy with the Industrial Revolution is that the latter represents a repeating pattern in the development of technology in Western capitalist societies. Moreover, a possible, and sometimes actual, long-term effect of automation in industry is to “free up” workers for labor-intensive kinds of production. This is analogous to what happened in

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[321] E.g., Webb (2015).

[322] On the role of research technologists in 20th century chemistry, see Shinn (2002).

chemistry, in the sense, stated above, that chemists could now spend more time on other kinds of production like synthesis and applications. Of course, there is the disanalogy that in the industrial case, different groups of workers are employed in the labor-intensive versus the capital-intensive industries, whereas in the chemical case it is more complicated: in some research institutions the chemists operate the machines themselves for routine jobs, whereas in others, technicians take care of data production; in all cases there are instrument experts who maintain, improve and in some cases operate the instruments.

Moreover, increasing productivity was not the only reason for adopting the new methods. Scientific norms of accuracy, informativity, and epistemic security were major motivations. That said, scientific justifications for adopting the new methods often seems to have been mixed in with, and even in tension with, productivity-related justifications. A 1958 review of the new instrumentation by physical chemist S. Z. Lewin of NYU is at pains to point out the scientific benefit over and above the increased productivity: "the process of collecting analytical data has been made quicker, pleasanter, and more effortless [by the new instruments]. That is, however, only a part—and a minor part, at that—of the new capacities these instruments have provided to the analyst." It is worth noting that his target audience was analytical chemists.[323]

Furthermore, and as noted in the introduction, I have only been concerned with standard instruments and routine users. Scientists working at the cutting-edge of chemical analysis, say in protein structure determination, have very specific needs that require specialized instruments and the scientists to be experts in their instrumentation.

Despite these disanalogies, I think the eight common structural properties described above are suggestive of common underlying factors responsible for the common properties, and in the next section I will proceed to sketch hypotheses as to what these factors could be.

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[323] Lewin (1958), p. 19A; see also 20A. The article is billed as a "report for analytical chemists." See also Tishler (1983), p. 13 and footnote 59 above.

4.6 Explication of the analytical claim

Why might there be common structural properties between the mechanization processes described by Marx and the Instrumental Revolution? After all, the contexts are very different—one instance of mechanization occurring in the production of commodities, with examples drawn from the 18th and 19th centuries, the other in the production of data for chemists in the middle of the 20th century. It may seem implausible that the two instances have anything to do with each other.

Nevertheless, there is evidence that scientists involved in the Instrumental Revolution were animated by a new way of thinking about data production, one that consciously draws on scientific and technological knowledge as a whole rather than on the specific discipline in which the data is sought.

For example, James Feeney, co-author of a textbook on NMR, has periodized the progress of NMR in terms of alternating phases of science-driven and technology-driven development. The scientific discoveries underlying the method opened horizons for its application to structural analysis, but the technical requirements of the spectrometer entailed that "the full development of the method also relied on borrowing technology already being used successfully in other forms of spectroscopy and measurement."[324] The potential for applying NMR to structural problems other than relatively small molecules was not realized until improvements in the electronics and the magnet, the introduction of Fourier transform algorithms, improvements in computation, and yet other developments had come about.

Texts from the period of the Instrumental Revolution display the principle that analytical problems are to be solved by the replacement of human manipulations by the conscious application of science and technology. Analytical chemistry texts are particularly explicit on this point, perhaps because analytical chemistry became more directly concerned with the design of instrumentation than organic chemistry. For example, a report on the 1960 Pittsburgh Conference on Analytical Chemistry and Applied

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[324] Feeney (1999), pp. 206-207.

Spectroscopy states that the new instruments showcased that year all have in common that "[t]hey eliminate the human element, either partly or almost wholly."[325] Scientific texts also display the principle at work.[326] In his call for the establishment of instrumentology laboratories, Klopsteg emphasized the comprehensive character of modern instrumentation. The disciplines he thought should be represented in these laboratories included physics, chemistry, mathematics, materials science, meteorology, geophysics, thermodynamics, acoustics, various kinds of spectroscopy, optics, and electronics. Lewin's review also emphasizes the instrumentation's eclectic character. Lewin examined trends in analytical instrumentation before and after World War II. He identified a "common feature" distinguishing post-war devices from pre-war, namely that:

> [t]hese devices have been created by the conscious application of the principles of a relatively newly recognized discipline—the science of instrumentation—to the chemical need that was to be satisfied. The science of instrumentation is a hybrid field, drawing its content from optics, electronics, mechanics, circuit theory, computer theory, psychology, and all those aspects of physics and chemistry that treat the interactions of radiant energy and electric or magnetic fields with matter.[327]

For Lewin, the "science of instrumentation" is not just a discipline dedicated to instrument-making, but an approach to "chemical needs" that is consciously eclectic.

Lewin's review is also noteworthy in that it suggests an explanation of the origin of the new instrumentation. According to Lewin, every modern analytical instrument is composed of four fundamental components: a "transducer, or detector", an amplifier, a computer and an output. He likens the detector to "the eyes, ears, and nose of the instrument" and credits modern electronic detectors of radiation with greatly increasing the range of spectrometers, going so far as to claim that "their utilization in place of the photographic plate has been *directly responsible* for the current vigorous flowering of the fields of microwave, infrared, near-infrared, Raman, visible, ultraviolet and x-ray spectrometry."[328]

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[325] *Chemical and Engineering News* (1960), p. 106.

[326] Though space does not permit discussing these texts, the views of Heyrovský & Shikata (1925), Müller (1941), Ewing (1976) could be adduced as further evidence.

[327] Lewin (1958), p. 21A.

[328] Lewin (1958), p. 20A. My emphasis.

Lewin's assessment of the sources of progress suggests that the function of detection may have played a role analogous to that of the tool-bearing function in Marx's analysis. The key change, according to Lewin, was the switch from the detection of a chemical or physical property by exposure of a photographic plate to it, to the use of electronic detectors. According to Lewin, the photographic plate was the characteristic detector of pre-World War II analytical instrumentation. Lewin emphasizes the laboriousness of photographic plate detection:

> Compare, for example, the ultraviolet absorption spectra obtainable by means of photographic instrumentation commonly used in the 1930's with that provided by a modern recording spectrophotometer … With the older type of equipment several exposures of a photographic plate had to be made at different slit settings; the plate had to then to be developed, dried, and microdensitometered; the results had to be compared with more or less laboriously achieved calibration data for the photographic emulsion; finally an absorption spectrum could be computed and plotted. The entire process required one to two days.[329]

In contrast, with the use of the recording spectrophotometer "a pen moving across a paper chart automatically plots a finished absorption spectrum in a matter of minutes."[330] This gain in time is made possible by the use of an electronic detector, in this case a photocell, which converts the incoming light from the sample into an electrical signal that can then power the recording device (what Lewin calls the output). The switch from the photographic plate to the photocell allowed the detector to be electronically connected to the output, which then allowed the recording of the signal to be automated. The signal generated at the detector may not be strong enough to power the output by itself, but since the detector is electronic it can be connected to an amplifier, which increases the signal to a usable level. Moreover, the signal may not be in a form suitable for providing the desired information at the output, and so connection with a computer is needed to transform the primary signal into the appropriate form. Depending on the output needed, the computer will be used to convert a current into a voltage, a direct current into an alternating current

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[329] Lewin (1958), p. 19A.

[330] Lewin (1958), p. 19A.

or vice-versa, modify the wave form of the signal, change the frequency, digitize the signal, etc.[331]

Lewin credits the combination of electronic detectors and amplifiers with bringing about significant scientific progress:

> The greater sensitivity, linearity, and reproducibility of electronic detectors and amplifiers, compared to such "classical" components of instruments as the human eye, photographic plate, and light-beam galvanometer, have now made it possible to sense, and to measure accurately, a vast array of substances for which no specific analytical method had previously been available, and at concentrations ranging from the pure substance down to 10^{-8} to 10^{-10} M and even less in favorable cases.[332]

Throughout his two-part review, Lewin emphasizes the scientific pay-offs, in terms of accuracy, sensitivity, resolution and range of application, that were made possible by the use of electronic detectors and their combination with other kinds of equipment.[333]

For some corroboration of Lewin's claims, I will briefly discuss the strategic role of detection in mass spectrometry.[334] In mass spectrometry, the components of a sample are ionized and then separated by various arrangements of electric and magnetic fields. The mass-to-charge ratio of each kind of ion is measured, and this information allows the components of the sample to be identified. Prior to the 1940s, the photographic plate was the most common method of detection. Starting in the 1940s, the photographic plate tended to be replaced by electronic detectors. This modification enabled automatic strip chart recording of the mass spectrum, which simplified and accelerated spectrum recording compared to the photographic method. Strip chart recorders yielded an analog recording, however, which had to be converted into tabular form through a labor-intensive process.

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[331] Lewin (1958), p. 22A (digitization is my example).

[332] Lewin (1958), p. 20A.

[333] Lewin's emphasis on the importance of the transition from photographic to electronic detection is corroborated by Hardy (1938), wherein the history of the first recording spectrophotometer (invented by the author) is described, and by the historians Morris & Eklund (1997), p. 559, and Thackray & Myers (2000), pp. 149-151. For a skeptical view of the photocell's potential in the 1930s, see Twyman (1931). Twyman was the technical director of Adam Hilger Ltd, producer of the Spekker photometer based on photographic detection by means of a quartz spectrograph.

[334] The following relies heavily on the account in Grayson (2004).

The earliest use of computers (1958) in mass spectrometry was that of a digitizer that could tabulate the data as the spectrum was being generated. The Mascot digitizer was itself fairly crude, in that it was unable to do anything else but digitize the output of the spectrometer to which it was hard-wired. But digitization, in turn, enabled new applications of the computer to mass spectrometry in the 1960s. The DENDRAL algorithm was developed to interpret the spectra of unknown compounds, albeit with limited success. High-resolution mass spectrometry, which allows deduction of elemental composition, relied heavily on computers to digitize the data from the detector and process them into exact mass and intensity information. Library search algorithms were developed to match the spectra of unknowns with those of reference compounds. In the 1970s, techniques and instrumentation were developed that allowed the spectrometer to be coupled with a gas chromatograph and a data system. The GC-MS-DS was capable of generating several hundred spectra per half hour, which could eventually (1990s) be compared via library search algorithms to libraries containing hundreds of thousands of reference spectra. In contrast, only a few spectra per hour could be prepared by an operator using a strip chart recording machine of the 1940s and 1950s.

In this section, I have provided grounds for thinking that intervention on the strategic function of detection played an important role in the Instrumental Revolution. The intervention involved an evolution from processes in which humans were heavily involved in data production (e.g., the production and processing of photographic plates) to ones in which data production was increasingly automated. This evolution made possible the black-boxing of the instruments and hence their use for purposes of routine structure determination by organic chemists. The progress made possible by the intervention required an eclectic approach to methods development in chemistry, one that drew on advances in diverse fields of science and technology.

Once the new methods were adopted by organic chemists, they supplanted the previous approach of solving chemical analysis problems largely through chemical methods. We are now in a position to see why the common structural properties described in section 4.5 should obtain:

I. In both cases, traditional assumptions about how problems of production should be solved were discarded in favor of a more eclectic approach that draws on diverse fields of science and technology, and in so doing, facilitates the development of methods specially adapted to the given problems.

II. In both cases, the eclectic approach was not simply interdisciplinary. The interdisciplinarity was achieved through the construction of machines that exploited advances in different disciplines and that were used to transform the relevant labor processes.

III. In both cases, since the new methods were based on machines, the ability to engage in the work required significantly more capital than before.

IV. In both cases, since the design of the instruments was based on an eclectic approach that itself required dedicated workers, a division of labor arose between specialist instrument-makers and non-specialist instrument users. Automation and black-boxing also facilitated use by non-specialists.

V. In both cases, skills that were crucial for the execution of the production process became marginalized because the new methods made use of different processes than those relevant to the skills.

VI. In both cases, machines made automation possible.

VII. In both cases, the possibilities for modifying and combining the instruments allowed human cognitive and physical limitations to be circumvented.

VIII. In both cases, the new methods tended to increase productivity, and given the importance of productivity norms in the societies concerned, this fact was used to motivate their adoption.

4.7 Objections and replies

One of the basic claims of this chapter is that a process of transformation of the labor process occurred in chemistry that was not only analogous to mechanization processes in the broader society, but was in part caused by similar factors. A confusing aspect of this episode, however, is that it combines mechanization, a change in the kinds of data produced, and the introduction of the new quantum theory to structural chemistry. This combination gives rise to two related objections:

1. Mechanization involves having a machine carry out a process formerly carried out by a human. This episode is not a case of mechanization, because the data produced by means of the new instruments are radically different from those produced by means of the old instruments.
2. The reconceptualization of chemical structure in terms of quantum mechanics and spectroscopic properties is what drove change in this episode, not the opportunities that mechanization offered for the transformation of labor.

In answer to (1), I reply that mechanization is not incompatible with significant change in the nature of the data. The Instrumental Revolution was a case of what Keating, Limoges and Cambrosio describe as a "creation of an emerging new field of actions between humans and machines and between the humans themselves" (see section 4.3). The episode resulted in a change in both the object of labor and how operations were performed on it. Before the revolution, the object of labor was the substance whose structure was to be determined. The chemist discovered its chemical properties by deploying his or her (usually his) mental and manual skills on it in a series of laboratory operations. After the revolution, the object of labor is light energy (or the molecule and its fragmentation ions, in the case of mass spectrometry). In order to obtain data, chemists operate machines that, with the aid of instrumentation specialists and (sometimes) technicians, perform a series of operations on the object. Thus the instrumental methods are more machine-centered than classical methods, and their development involved a reconceptualization of the process of compound identification. They also produce a different kind of output, though the final outcome of the compound identification process—the structural representation of the compound—is the same.

The second objection ignores the historical development of the new methods. The realization of methods based on quantum mechanics required the transformation of labor. Each technique is based on a physical phenomenon. The initial phenomenon, however, was generally useless for other than the physicists interested in the phenomenon itself until the changes described in sections 4.3, 4.5 and 4.6 took place. Mechanization was required to develop methods that had the speed and control needed to produce data informative enough to replace chemical data. Black-boxing, an empirical approach to data interpretation, and a new division of labor were other elements required to make the methods attractive to ordinary organic chemists.

True, the old methods would never be able to provide certain kinds of structural information, for example on molecular conformation. But there was information loss with the new methods as well. Classical structural chemistry had two principal goals: (1) identify a substance in terms of a structural representation, and (2) learn about the chemical reactions in which the substance participates. Classical methods allowed both goals to be achieved simultaneously. Modern structural chemistry has kept (1) as a goal, but not (2), because the spectroscopic properties that are now employed to achieve (1) are of little chemical interest in themselves.[335] Given the monetary and other costs of adopting instrumental methods, the replacement of chemical methods by instrumental methods would only make sense if goal (2) could be abandoned, downgraded, or replaced by some other goal. In the end, the other goal was synthesis.

A third objection starts from the social class disanalogy mentioned in the last section. Class struggle is central to Marxist theory. But there are no clear analogues to capitalists and workers in the Instrumental Revolution. Therefore, Marx's theory is irrelevant to its analysis. The objection fails, however, because it ignores the equally central role of the labor process in the theory developed in *Capital*. There, it is shown that the characteristics of the instruments of labor, and the structure of the labor process more generally, impose constraints on the application of science and technology to production. I have merely extended this line of analysis to the sphere of scientific production itself.

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[335] In a 1974 interview, the Nobel laureate Robert Robinson makes the point forcefully that the empirical knowledge of chemical reactions was an independent goal of classical structure determination [Robinson, (1974), p. 57]. Professor W. von E. Doering regretted the loss of a "nigh inexhaustible" source of unexpected discoveries in the classical approach [(2000), v].

4.8 Conclusion: *The* Instrumental Revolution or *an* instrumental revolution?

The Instrumental Revolution has sometimes been compared, in passing, to the transition from craft-based production to industrial production during the Industrial Revolution, as in the comment by John Taylor quoted above.[336] If this comparison is taken seriously, as I have taken it in this paper, it implies that the nature of chemical analysis changed from a more "craft-like" labor to a more "industrial" type of labor. Starting perhaps with Edgar Zilsel in the 1940s, various writers in science studies have viewed science as a kind of craft.[337] Different authors focus on different respects in which science resembles or has resembled craft labor, but the main focus of this paper has been on the locus of expertise on the instruments employed. On some views of what is involved in craft labor, the craftsman is supposed to be an expert on the tools he employs. For example, the philosopher Etienne Balibar holds that "[b]efore the industrial revolution, a *'technique'* was *the indissociable ensemble* of a means of labour or tool, *and a worker*, moulded to its use by apprenticeship and habit."[338] As a result, the performance of a technique is an essentially individual exercise even if labor is organized collectively, as in the 'manufacture' mode of production discussed above. The complex division of labor needed to operate the machines characteristic of large-scale industry, in which expertise is unevenly distributed between engineers and technicians, or between the machine operators and the scientists who discover the laws that are applied in the machines, is therefore unnecessary in craft labor.

In this chapter, I have provided evidence that chemistry went through a transition of this type. Similar transitions have been observed in other domains, for example

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[336] Cf. also Knight (2002), pp. 90-91; Reinhardt (2006) claims that "the mechanization of chemical practice was closely connected to its handicraft side: Without syntheses of labeled compounds, the mass spectrometer would have stood idle" (p. 169). The title of Peter J. T. Morris's (2015) book on the history of the chemical laboratory is *The Matter Factory*.

[337] E.g., Zilsel (2000 [1942]); Polanyi (1958); Ravetz (1971); Latour & Woolgar (1986) ; Clarke & Fujimura (1991).

[338] Balibar (2009 [1965]), pp. 267-269 (emphasis in original).

physiology and astronomy in the 19th century and microphysics in the 20th.[339] The existence of this pattern suggests an externalist hypothesis that the course of science is influenced by the diffusion of principles of organizing labor, what I will call 'labor-principles' for short, that originate from outside of science. A labor-principle is a principle, or perhaps better, strategy, for organizing and (and hence dividing) labor. It can be thought of in terms of maxims like: "use minimally skilled technicians for repetitive tasks" or "automate as much as possible"[340] or "conception and execution should be carried out by the same [or different] people."

The notion of a labor-principle may be related to the growing philosophical literature on what Philip Kitcher (1990) called 'the division of cognitive labor.' This term reflects the fact that scientific research as a whole is organized according to a division of labor, and that this division seems to have something very important to do with scientific progress. Questions philosophers have been interested in is how this division is effected, how it contributes to scientific progress, and how the former ought to be effected to maximize the latter.[341] The focus of this literature has been on mechanisms of coordination between scientists, which involve things like reward systems, and how these mechanisms affect the allocation of research projects, the selection of research strategies, etc. As noted, the scope of these studies is holistic, having to do with the division of labor across projects and fields. In contrast, the notion of a labor-principle is intended to be narrow, pertaining to how particular labor processes are organized. Though the transformations of chemistry described in this chapter affected the work of all chemists, the focus has been on *how* the work was done rather than on the choice of projects. That said, and as noted in 4.3, the transformation of individual labor processes may have an effect on the allocation of labor across projects, in the case at hand by routinizing a certain kind of work.

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[339] For physiology, see Dierig (2003); for astronomy, see Lankford (1997) and Bigg (2000**)**; for microphysics, see Galison (1997), esp. chapter 5.

[340] According to Perovic (2011), p. 35, the attitude of Lew Kowarski, an influential physicist at CERN in the 1960s, was that "*the evolution of data-handling in bubble chambers leads 'towards the elimination of humans, function by function*'" (emphasis in original).

[341] See Thicke (2016), ch. 3 for a recent critical review of these efforts.

To return to the externalist hypothesis mentioned above: if true, it has a normative edge, insofar as one can ask whether the external principles have a good or bad effect on the quantity and quality of scientific results. For example, the mechanization of work may be a positive development if it extends the reach of human knowledge or makes the results of science more reliable. On the other hand, excessive faith in mechanization may lead to errors of application or missed discoveries.[342] In some cases, it is possible to say fairly precisely what difference the adoption of the principle made to the quantity and quality of the results, perhaps more precisely and with greater certainty than is possible in traditional controversies over scientific method, for example whether Newtonian deduction from phenomena or the method of hypotheses is to be preferred.[343]

The hypothesis also implies that there is no unique model of scientific development. Different labor principles will promote different rhythms and kinds of change. Moreover, similar labor principles applied to different sciences may have different effects with respect to progress. Industrial methods may have been successful in, say, physiology but not in plant breeding.[344] As noted above, various authors have alluded to the craft/industrial labor contrast to describe episodes of scientific change, and in at least one case have developed the contrast in depth, though with a limited aim.[345] It might be worth taking a more systematic approach, and exploring how different labor principles affect scientific progress.

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[342] In chemistry, Nicolaou & Snyder (2005) claim that excessive faith in instrumental methods sometimes causes chemists to make erroneous structural assignments. Perovic (2011) argues that excessive automation of high-energy physics experiments risks missing experimental challenges to the Standard Model of particle physics; Galison (1997), ch. 5 recounts the historical debates over the wisdom of automating HEP.

[343] E.g., Laudan (1981), Smith (2002), Harper (2011).

[344] For an examination of the unsuccessful application of industrial techniques in plant breeding, see Curry (2017).

[345] Ravetz (1971) defends the thesis that science is a kind of craft labor. His purpose is primarily to articulate a critique of Big Science as imposing an industrial style of research on science and thereby distorting it from its optimal form. For the purpose of this paper, I prefer to stay neutral with respect to Ravetz's critical aim and his view that the essence of science is craft.

In this study, the analogy with mechanization processes in the Industrial Revolution, as the latter were conceptualized by Marx, has directed our attention to the cognitive consequences of radical changes in the means of production. These include:

- Changes in the kinds of knowledge and abilities necessary to conduct research
- Changes in who possesses the different kinds of knowledge
- Changes in how problems are solved, in the case of mechanization that a larger body of the available knowledge can be applied to problem-solving than would otherwise be possible
- Changes in the adaptability of the means of problem-solving to specific problems
- Changes in the relations of epistemic dependence (e.g., in the degree of dependence on external experts, or on semi-skilled technicians)
- Changes in training and education
- Risks due to lack of understanding of the instruments[346]
- Path-dependence in research due to costs sunk in instrumentation[347]
- Changes in goals due to biases inherent in the new means
- Changes in the role of "specifically" human qualities, like creativity and flexibility, in research[348]
- Questions about which norms are to prevail: scientific (accuracy, reliability, etc.), pragmatic, productivist, pedagogical, etc.
- Changes in the rate of methodological innovation[349]

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[346] In chemistry, Nicolaou & Snyder (2005) claim that excessive faith in instrumental methods sometimes causes chemists to make erroneous structural assignments.

[347] On path-dependence in science, see Peacock (2009).

[348] Perovic (2011) argues that excessive automation of high-energy physics experiments risks missing experimental challenges to the Standard Model of particle physics; Galison (1997), ch. 5 recounts the historical debates over the wisdom of automating HEP.

[349] The impact of the Instrumental Revolution in chemistry on the rate of methods development and on data-to-phenomena reasoning are discussed in chapter 5 below. On the impact of the revolution on the reasoning employed in structure determination, see also Seeman (2018).

- Changes in data-to-phenomena reasoning[349]

Previous accounts of the Instrumental Revolution have focused on the question of whether and to what extent the episode fits general models of scientific revolution (especially Kuhn's and Hacking's). This focus is understandable, because the categories employed in these models are very valuable for bringing out important features of scientific change. Nevertheless, they are not designed to capture the causes and effects brought about by radical changes in the means of production. Because the latter changes can have the consequences listed above, I think it worth considering whether *the* Instrumental Revolution was also *an* instrumental revolution, that is, an instance of a distinct kind of revolution involving radical changes in the means of production. Given the fundamental role of the means of production in the labor process, this kind of revolution is not limited to cases involving mechanization. Nor is it limited to changes in data-producing instruments, but could involve, for example, means of representation or theorizing. The notion of an 'instrumental revolution' raises questions for future research. Historically, are there other instances of this kind of revolution in science? Answering this question will involve comparisons across the sciences. [350] Conceptually, what is the nature of such a revolution, considered as a kind of revolution rather than a particular historical episode? As it confronts the Instrumental Revolution, the study of scientific change gains new research questions.

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[350] For example, there is evidence that physiology, astronomy, microphysics and mathematics have undergone something like an 'instrumental revolution.' For physiology, see Dierig (2003); for astronomy, see Lankford (1997) and Bigg (2000); for microphysics, see Galison (1997), esp. chapter 5; for mathematics, see Mackenzie (2001).

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5.0 THE INSTRUMENTAL REVOLUTION AND THE HEURISTICS OF CHEMICAL RESEARCH

Laudan (1981) characterizes ‘heuristics’ as the branch of scientific methodology that is concerned with identifying strategies and tactics that will accelerate the pace of scientific advance:

> To ask how we can broaden the range of viable theories about a particular domain, or to ask what rules of thumb can assist in the discovery of new theories is to raise heuristic rather than validational questions. Where proponents of theories of validation tend to be preoccupied with truth, falsity and epistemic warrants, the vernacular of heuristic theorists tends to be laced with terms like ‘scientific progress’ and ‘the growth of knowledge’.[351]

Whereas theories of validation are concerned “to ascertain under what circumstances we can legitimately regard a theory as true, false, probable, verisimilar or close to the truth,” theories of heuristics are concerned with how progress is made and what factors can accelerate it. Franklin (2005) views experimental methods as heuristics and characterizes them in terms of efficiency: An efficient experimental method is one that wastes neither time nor resources. I suggest that in contrast with the epistemological orientation of confirmation theory, the study of heuristics has an “economic” orientation in that it is concerned with how scientific resources are allocated and what allocations are efficient. The productivity of scientific work is therefore a prime concern of the study of heuristics. Though perhaps more philosophically mundane than truth, productivity has everything to do with scientific progress and the growth of knowledge.

Franklin distinguishes between two kinds of instruments based on their efficiency characteristics. ‘Narrow’ instruments afford either a single data point, or “a small collection,” per experiment. Examples are thermometers, the Northern blot, and the magnetometer. ‘Wide’ instruments, on the other hand, “allow scientists to assess many features of an experimental system.”[352] They are conceptually derivative of narrow

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[351] Laudan (1981), pp. 3-4.

[352] Franklin (2005), p. 896.

instruments because they function either as parallelizations of narrow instruments, affording many measurements per experiment, or as serializations that cycle through many different measurements very quickly. Franklin’s examples are DNA microarrays, functional imaging instruments (e.g., fMRI), and combinatorial libraries of objects. As I will show in section 5.2, this classification is helpful for understanding how the new instruments afforded information for chemical analysis.

Wimsatt (2007) views scientific heuristics as a kind of problem-solving procedure for simplifying the modelling of complex systems. He identifies several distinctive properties of heuristics that distinguish them from truth-preserving procedures, of which two are especially relevant for the discussion that follows. First, heuristics are more efficient than the procedures for which they may be substituted, which is why they are used in the first placed. Second, “[t]he application of a heuristic to a problem yields a transformation of the problem into a nonequivalent but intuitively related problem.” A consequence of this transformative property of heuristics is that “answers to the transformed problem may not be answers to the original problem.”[353] The transformative aspect of heuristics raises the possibility that a procedure may be adopted because it is more efficient than the procedures it is substituted for, while transforming the original problem such that the answers obtained are not those sought. Though this would be an extreme case, it makes the point that heuristics involve a trade-off between the pragmatic and the epistemic. Another such trade-off arises from the fact that heuristic procedures are systematically biased in virtue of the assumptions they introduce to simplify a problem. This bias can lead to error when the heuristic is applied to cases in which those assumptions are inappropriate.

Though I will be discussing the heuristics of experimental methodology rather than of scientific theorizing, the idea that the quest for more efficient solutions can lead to transformations of the problems themselves, and not just the methods used to solve them, is relevant for understanding the Instrumental Revolution. The episode did not simply amount to replacing one set of instruments with another, but in fact changed certain epistemic aspects of structure determination work. Namely, they changed the structure of

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[353] Wimsatt (2007), pp. 76-77.

inferential reasoning from data to structure. They also changed the nature of the knowledge produced, from knowledge of chemical reactions to knowledge of molecular structure.

The nature and pace of scientific advance in chemistry were altered in two ways by the Instrumental Revolution. First, chemistry experienced a change in the nature and rate of innovation of methods for chemical analysis. Second, the new instruments allowed for greater efficiency in the identification of unknown compounds. This chapter explores these developments as follows. Section 5.1 is about the dynamics of methodological innovation in chemistry. I argue that the dynamics of methods development in the 20th century does not conform to an evolutionary explanation of innovation advanced by the historian Carsten Reinhardt in his study of the Instrumental Revolution. The reason it does not is the holistic character of the labor process. The holistic character of the labor process poses an in principle objection to evolutionary theories of innovation. In the chemical case, the relation of the chemist to his or her instruments was a crucial determinant of the nature and pace of innovation.

Section 5.2 assesses the overall impact of methodological innovation on the efficiency of problem-solving in chemistry. To this end I will compare the logic of structure determination before the Instrumental Revolution to the logic after. I argue that the latter was more efficient and less error-prone than the former, and that the improvement was due to the restructuring of the labor process. This conclusion inverts a traditional view of the scientific labor process. On this view, the latter is the expression of a set of cognitive considerations. Experimental work, for example, is interpreted as expressing the logic of hypothesis testing (see below). Scientists are envisioned as seeking to test hypotheses, and this end determines the means of its fulfillment. My view is that the reverse is also the case: the means available to scientists constrain their ends, for example the specific nature of the hypotheses that are tested, or their reasoning from data to hypothesis. Focusing especially on the method of inference, I show that the labor process can constrain cognitive considerations.

In section 5.3 I argue for a historicist approach to heuristics. This approach is suggested by reflection on the role of the means available to scientists—instruments, techniques—in making possible, but also constraining, present and future research. The means available at the time a novel research situation emerges may not be optimal for the new situation. Episodes like the Instrumental Revolution may be interpreted as responses to this sub-optimality.

5.1 Methodological dynamism

In this section, I will argue that there was a shift in the dynamics of methods development in structural chemistry associated with the Instrumental Revolution. Previous accounts of the revolution have largely focused on the institutional and individual pathways by which physical methods diffused over from physics and on the ways in which they were adapted to the final user, the ordinary chemist. These approaches view the problem of explaining the importation of knowledge and methods from one scientific field into another, very different one, largely as one of explaining how relations were established between the exporting and importing fields that enabled the technology transfer to proceed.

The most sustained example of such an account is that offered by the historian Carsten Reinhardt in his (2006) study of the period, *Shifting and Rearranging.* Carsten Reinhardt poses a guiding question of his study in evolutionary terms:

> Taking up a metaphor from evolutionary biology: Which processes of adaptation made it possible that some techniques thrived in a new environment while others did not? The first part of the answer lies in the fact that the transfer of these methods was a gradual, step-wise process, taking advantage of disciplinary niches close to the original context.[354]

For him, the development and dissemination of the new methods were partly a result of an interplay between competitive pressures and cooperative strategies. The key players were academic scientists and instrument-makers. Following the innovation theorist Eric von Hippel, Reinhardt calls the academic scientists who initiated the development and use of the methods “lead users.”

> … lead users of scientific instruments can be seen to fulfill their functions best if they lead the actual uses of novel instrumentation … and, moreover, if they are already established scientists (their leadership being further enhanced by the successful use of instrumentation). In this position,

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[354] Reinhardt (2006), p. 16.

> scientific lead users are enabled to create and to direct the market for scientific instruments. Thus, precisely because of their scientific authority, lead users influence the success of the instrument manufacturer; and their participation in academic administration and funding agencies allows them to lobby for the new techniques from the inside of the scientific establishment. Moreover, through specifications of novel custom-built instruments, and their eventual success in the scientific marketplace, lead users supply the instrument manufacturers with crucial ideas for improvements. On the other hand, the exclusive ownership of up-to-date instrumentation is a huge advantage in one's own research, when done in a competitive environment … The course of this cooperative strategy could not be thoroughly planned, neither by the company nor by the scientist. But it was recognized and acknowledged as such on both sides.[355]

The competitive pressures were thus twofold: there was the pressure on instrument manufacturers to survive on the market, and there was the pressure on the scientists to maintain and accumulate credit in academia. Both pressures were handled by means of cooperation between members of the two social groups, in addition to the usual cooperation within the respective firms and research groups. This cooperation led to the replication of the instruments produced by the manufacturers as well as the methods developed by the scientists. The evolutionary story could be taken even further than Reinhardt does, for once the methods and instruments became normative, each ordinary chemist had strong incentives to use them. A chemist who continued to do chemical analyses the old way would quickly lose credit in the face of competitors who could solve the same problems using the faster, more informative and (as I will argue in section 5.2) more secure techniques.

One peculiar feature of this account is that the nature of the instruments, which after all were quite peculiar themselves, seems to have no explanatory role. But the mere ability to transfer technology to a field does not dictate the pattern of technical dynamism characteristic of it, however; it is as compatible with piecemeal introductions of technology as with a sustained and rapid development.

The reason that the ability to transfer technology does not dictate the pattern of technical dynamism is that the transfer presupposes that humans and instruments carry out specific epistemic activities (see section 2.4.1) in the labor process. In a given labor process, it is the function of the scientist or instrument to carry out certain epistemic

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[355] Reinhardt (2006), p. 177.

activities, and the technology must be compatible with these functions in order for the transfer to proceed. In the second chapter, as well as in chapter 4, the labor process was conceived as a system of functional relations between an agent or agents, an object of labor, and instruments of labor. On this structuralist view of the labor process, it is impossible to make major changes to one element of the process—here the instruments—without changing the other elements. There follows from this impossibility a disanalogy with the evolutionary model. The disanalogy is similar to one pointed out by L. J. Cohen in his 1973 critique of Stephen Toulmin's version of the evolutionary theory of science:

> concepts of physics are not, as such, all closely similar to one another, like the members of a species or population. Rather, they are almost all importantly different from one another, like the concept of an electron and the concept of a proton. Each such concept may conceivably have a few variant forms that are in competition with one another. But the great bulk of the 'concepts, methods and aims' within a rational discipline at any one time manage to be quite different from one another and yet not to be in competition, but in systematic association, with one another.[356]

Cohen is here pointing out that the evolution of scientific concepts cannot be explained in the same manner as the evolution of a species, on the grounds that in Darwinian-type explanations, "within any population … of environmentally threatened individuals, the similarities that are selectively perpetuated are those that are favourable to the continued existence of such individuals."[357] This is because the forces of natural selection operate on individuals, enhancing the reproductive fitness of individuals with adaptive traits, resulting in the spread of the trait among the members of the population over time. But on the holistic conception of scientific concepts that Cohen endorses, changes within a discipline involve a restructuring of an "evolving" concept's relations to other concepts and not just a replacement of individual concepts. "Selection" here operates at the level of the system, for a change in one concept requires changes in the other concepts to which it is systematically and differentially related.

The introduction of the new instruments presupposed a restructuring of the whole process. Once chemical reactions were replaced by physical interactions, the functions of the chemist (in chemical analysis) were transformed. Arguably, so was the object of labor,

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[356] Cohen (1973), p. 49.

[357] Cohen (1973), p. 48.

which I suggested in section 4.7 became either light energy or molecular species.[358] The nature of the inferences used to identify compounds, as well as the theories brought to bear, also changed. Rather than a mere replacement of one kind of tool by another, say, one kind of glassware by another kind, an entirely new approach to analysis was adopted. Indeed, glassware presupposes different epistemic activities than spectrometers. The former presupposes activities pertaining to the manipulation of substances. The latter presupposes activities pertaining to machine operation and signal production and processing.

We can think about this holistic aspect of the changes to the labor process in terms of the categories of knowledge proposed in chapter 2. Changes in tools constitute changes in instrumental knowledge (IK). If the change is significant enough, then it might make possible a significant change in the methods used to solve certain problems of the field. This is a change in MK. The rest of this section will argue that the changes in IK that occurred during the Instrumental Revolution enabled massive progress in MK to occur relatively quickly.

The moral of the comparison of the conceptual case with the instrumental case is that the cogency of an evolutionary explanation depends on whether the entities that are claimed to replicate through a selection process are related by similarity or by systematic differences. If the latter, then change affects not just individuals, but the whole system.

I claim that it is such a restructuring that paved the way for greater methodological dynamism in chemistry. What do I mean by this term? Chemistry became methodologically dynamic in the qualitative sense that old spectroscopic methods were continually being scrapped or modified in favor of new methods, or continually enriched by the addition of new ones. By “method” here I intend, as a first approximation, a four-fold combination of theoretical principles, instruments, experimental methods, and data analysis techniques.[359] Experimental methods involve the various ways in which the theoretical principles and instruments can be orchestrated to obtain the desired data. Data analysis techniques include

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[358] Schummer (2002) argues that the concept of chemical species identity used to identify compounds changed during this period, from a concept of pure substance to a concept of molecular species.

[359] I borrow this four-fold distinction from Sternhell (1995), pp. 658-660.

inter alia the interpretational rules that allow the data to be correlated with chemical concepts. These developments depended on the ability of methods developers to apply advances in diverse fields to the instrument of labor. I also suggest that there was a quantitative increase in the rate of methods development relative to classical chemistry, but due to the inherent difficulty of establishing and interpreting such a claim I will focus mostly on the qualitative aspect.

One sign of dynamism is the proliferation of reviews in the late 20th century covering progress in the new methods.[360] For example, the chemist Sever Sternhell noted in 1995 that, after the discovery of the basic NMR phenomenon in 1946, progress took place very rapidly along parallel lines in all four aspects of the method.[361] This pattern of development may be characteristic of spectroscopic methods in general.[362] My reading of scientists' reflections on the nature of progress in these methods suggests that the latter have four general features that contribute to methodological dynamism: (i) complexity, (ii) hybridness, (iii) variety, and (iv) parallel development.

i. *Complexity.* As noted above, a spectroscopic method is a complex combination of theory, instrumentation, experimental techniques and data analysis. Each of these aspects is itself complex. The diagram of Figure 4.2, for example, illustrates vividly the number of components involved in modern NMR instrumentation.
ii. *Hybridness.* The methods are not only complex, but hybrid in the sense that they combine knowledge from distinct domains of inquiry. For example, quantum mechanics generally provides the physical basis, but must be combined with electronics and chemical structure theory in order to afford a chemically useful method.

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[360] The proliferation is noted in their own review by Jonas & Gutowsky (1980) for their field of NMR spectroscopy. (Jonas & Gutowsky, 1980, p. 9).

[361] Sternhell (1995), p. 658-659.

[362] For example, Nier et al. (2016) and Grayson (2004) describe the remarkable progress made in mass spectrometry in the second half of the 20th century. Due to space limitations, I focus on NMR here.

iii. *Variety.* The methods combine knowledge from a wide *range* of distinct domains, including electromagnetism, optics, quantum mechanics, atomic theory, bonding theory, geometry, chemistry, mathematics, and computer science. A significant portion of the physical sciences are brought to bear in spectroscopy.

iv. *Parallel development.* A virtue of the complexity, hybridness and variety characteristic of the methods is that their developers can exploit parallel developments in several independent domains. The history of NMR, for example, shows that progress there depended on exploiting results in physics, electronics, materials science, computing, and mathematics.

These four features of spectroscopic methods increase the rate at which methods are developed, the diversity of the kinds of information that can be obtained by means of them, and their range of application.

In a 1980 review, NMR specialists J. Jonas and H. S. Gutowsky provided some data on the growth of NMR. They observed that publications in NMR had become increasingly ramified and specialized since the initial discovery in 1946. An average of 50 research articles involving NMR were published per year between 1946 and December 1953, whereas the rate had increased to about 550 per month by 1967. A study of publication rates gleaned from the Chemical Abstracts System revealed that 2700 articles, where NMR key words featured prominently, were published in 1967, 4500 in 1971, and 5000 in 1978. The number of NMR researchers grew correspondingly, from about 50 in 1957, to 270 in 1967, and then to 370 in 1977. [363]

This is not to say that there was not active development of methods in classical structural chemistry. Many chemical methods were developed during the classical period, such as acid and base hydrolysis, oxidation conditions, and specialized tests to detect the presence of specific functional groups. Ozonolysis, for instance, was a powerful test for alkenes because it not only revealed their presence but could also reveal their location in

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[363] Jonas & Gutowsky (1980), pp. 9-13.

the molecule.[364] The history of ozonolysis suggests that methods development in classical structural chemistry largely took the form of reaction development, including the study and optimization of important reaction parameters, exploration of work-up procedures, discovery of the range of application of the method (for example, the classes of compounds to which it could be usefully applied), the determination of its potential as a complement or cross-check on other methods, and the identification of useful strategies for combining it with other chemical methods.

These forms of methods development, however, consisted essentially in incrementally deepening the ability of the chemist to manipulate and control the transformation of substances. Advances in other sciences, or in technology far removed from traditional chemical equipment, were brought to bear on structure determination problems in piecemeal fashion. The main way in which advances in other fields affected chemistry was through the improvement of adjuncts to the chemist's work, for example by the development of faster balances, different stirring appliances, or more robust glassware.[365] The transfer of technology from other fields for the construction of apparatuses was the work of exceptional chemists rather than a systematic endeavor.[366]

As the comments by the historians David Knight and Dean and Tracey Tarbell, quoted in chapter 4, suggest, however, the overall rate of development of laboratory tools and techniques was quite slow. In contrast, the development of spectroscopic methods was characterized by a synergistic pattern of development in which advances in different fields could be played off each other. This pattern is well illustrated by the development of NMR. The basic principle leading to the application of the technique in chemistry, that nuclear magnetic resonances could be observed in bulk materials, was discovered in 1946. Though not of much use at first, by 1981 NMR had been developed to the point that *the complete carbon skeleton* of a molecule could be established in a *single* experiment, surely

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[364] The development of ozonolysis up to 1940 is reviewed in Long (1940). See also Morris (1995) and Rubin (2003) for treatments of the early history of the technique.

[365] Ihde (1964), pp. 559 and 616 and Taylor (1985), p. 2. See also Ewing (1976) for a discussion of the development of the double-pan and torsion balances.

[366] Rabkin (1993) discusses several examples of such transfers. See also Russell (2000).

approaching the holy grail of structure determination methods.[367] At the same time it was also being applied to vastly more complex problems, like protein structure, than could be tackled at its inception.[368]

James Feeney, co-author of a textbook on NMR, has periodized the progress of NMR in terms of alternating phases of science-driven and technology-driven development. The scientific discoveries underlying the method opened horizons for its application to structural analysis, but the technical requirements of the spectrometer entailed that "the full development of the method also relied on borrowing technology already being used successfully in other forms of spectroscopy and measurement."[369] The potential for applying NMR to structural problems other than relatively small molecules was not realized until improvements in the electronics and the magnet, the introduction of Fourier transform algorithms, improvements in computation, and yet other developments had come about.

As discussed in detail in chapter 4, texts from the period of the Instrumental Revolution display the principle that analytical problems are to be solved by the replacement of human manipulations by the instrument-mediated application of science and technology. The shift from a conservative to a dynamic pattern of technical change in structural chemistry was made possible by the redistribution of epistemic activities within the importing labor process described in chapter 4. Previously, improvements in traditional chemical labware did little to alter the rate of technical change in chemical research, for they merely aided the execution of the chemist's functions. The introduction of spectrometers, on the other hand, ushered in a new era of technical change, for the new instruments presupposed entirely different functions, ones more amenable to the application of science and technology to the labor process. First, as was argued in chapter 4, chemical manipulation in analysis became largely limited to sample preparation. The main activities now were machine operation for signal production, and processing of the

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[367] Shoolery (1995), p. 48. In principle, "total structure elucidation" was already possible using X-ray crystallography, but in practice that method was limited because it requires crystalline compounds. In contrast, NMR is much more general because it works for compounds in solution.

[368] The original discovery by Bloch and Purcell was made on samples of water and paraffin wax.

[369] Feeney (1999), pp. 206-207.

resulting signal. Second, as was also argued in chapter 4, there are reasons to think that the detection function played a key role in this process. The replacement of the photographic plates used in the early spectrographs, the utility of which depended on ocular scrutiny by humans, by electronic detectors, paved the way for the development of sophisticated instrumentation around these detectors.

In short, there was marginalization of erstwhile central activities (chemical manipulation), and reorganization around new ones (signal detection). The functional positionality of the imported technology in the labor process of the importing field is an essential factor in the explanation of the rate of technical change in that field. In keeping with these considerations, I have here explored what might be called an “internalist” approach to technology transfer, in the sense of viewing the problem of importation as one of identifying the constraints imposed by the labor process in the importing field, and explaining how those constraints had to be dealt with in order for the knowledge produced by the exporting fields to be applied there in the way that it was.

One moral of this episode is that in order to understand how change was possible at the epistemic level, we need to understand how change was possible at the level of material practices, i.e., the labor process. As noted above, the new tools represented new IK. These innovations were profound enough to make possible major changes in the methods used to solve analytical problems. In order to get to that point, however, substantial methodological progress was required. That so much progress was achievable in a relatively short period is due to the fact (among others) that the new instruments presupposed different functions from the ones they replaced. A description solely in terms of epistemic categories would leave out a fundamental feature of this episode.

Though the focus of this chapter so far has been on the nature and causes of changes in the dynamics of methods development, the Instrumental Revolution also, of course, involved changes in the quantity and quality of the data produced. The heuristic impact of these changes will now be discussed.

5.2 The efficiency of compound identification

Philosophers often discuss scientific reasoning as if it were unconstrained by material conditions. Pierre Duhem provides an example in his famous discussion of holistic testing:

> A physicist disputes a certain law; he calls into doubt a certain theoretical point. How will he justify these doubts? How will he demonstrate the inaccuracy of the law? From the proposition under indictment he will derive the prediction of an experimental fact; he will bring into existence the conditions under which this fact should be produced; if the predicted fact is not produced, the proposition which served as the basis of the prediction will be irremediably condemned ...
>
> ... in order to deduce from this proposition the prediction of a phenomenon and institute the experiment which is to show whether this phenomenon is or is not produced, in order to interpret the results of this experiment and establish that the predicted phenomenon is. not produced, he does not confine himself to making use of the proposition in question; he makes use also of a whole group of theories accepted by him as beyond dispute. [370]

But how is the physicist supposed to bring the conditions into existence? The observational conditions do not fall into place all at once, and which background assumptions it is possible and makes sense to marshal depend on the material context within which he works. As was indicated in the previous chapter, the availability of the experimental conditions does not depend solely on the individual scientist's decision, but is usually made possible by a long prior development of experimental capabilities. A more recent account of hypothesis testing provides a further example:

> The term *hypothesis* can appropriately be applied to any statement that is intended for evaluation in terms of its consequences. The idea is to articulate some statement, particular or general, from which observational consequences can be drawn. An *observational consequence* is a statement—one that might be true or might be false—whose truth or falsity can be established by making observations. These observational consequences are then checked by observation to determine whether they are true or false. If the observational consequence turns out to be true, that is said to *confirm* the hypothesis to some degree. If it turns out to be false, then it is said to *disconfirm* the hypothesis.[371]

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[370] Duhem (1982 [1914]), p. 184.

[371] Earman & Salmon (1992), p. 44.

The labor process is here reduced to reasoning followed by "checking by observation." Scientific work would simply be an application of the method of hypotheses. Another example that will be discussed in greater detail in chapter 6 is Laudan's (1984) reticulated model of scientific change. According to this model, scientific change occurs through a process of gradual, piecemeal adjustment among three elements: theories, methodological principles and cognitive aims. Material practices are not discussed, leaving one to wonder how their evolution is related to the changes going on among these cognitive elements. Do changes in material practices merely express changes in the cognitive elements? Or can material practices alter the configuration of cognitive elements?

In this section, I will argue that material conditions of scientific work can constrain the cognitive elements of scientific practice. I focus on the method of inference. The point is to develop a more balanced account of scientific change, as a process in which the cognitive and the material mutually condition each other. In doing so, we will see that some problems in the method of inference can only be solved by altering the material conditions that provide the content of the inference.

This last point may seem obvious. *Of course*, one might say, the method of inference depends on the material conditions. The latter are just the experimental conditions that have to enter into the deduction from hypothesis to observable consequence (see section 4.1). Changing the experiment will obviously change the inference. What does talking about the labor process add over and above talking about different kinds of experiment?

But it should be noted that by "material conditions," I do not mean merely the "experimental conditions." Material conditions encompass a range of possible experimental conditions that scientists can choose from. Change in the material conditions is slower and more difficult than merely switching experimental conditions. The material conditions are akin to what Ackermann (1985) calls an 'instrumentarium,' a collection of kinds of instruments producing data. As Hacking (1988) and Peacock (2009) have observed, instrumentaria tend to be stable unless there is strong reason to replace them. Though I will be focused on instruments and their accompanying techniques in this section, I note in passing that the material conditions include not just observational instruments but also non-observational instruments like computers, as well as labs, divisions of labor between labs and disciplines, institutional arrangements, etc. My ‘material conditions’ are essentially what Pitt (2000) and Edwards (2010) call a ‘technological infrastructure,’ of which instruments are just one part.[372] The stability of the material conditions are part of what gives any labor process its routine, repetitive character, and are in fact what makes the repetition of the labor process possible.[373]

Thus, the point made above could also be stated in terms of instrumentaria or technological infrastructures: we will see that some problems in the method of inference can only be solved by altering the [instrumentarium or technological infrastructure or material conditions] that provide the content of the inference. As chapter 4 attests, such an alteration of material practices involves far more than simply switching experimental conditions.

5.2.1 Classical methods of structure determination

Before describing classical methods, it will be appropriate to describe what is meant by “compound identification” and “structure determination.” For this purpose I draw on work by Joachim Schummer, who in his (2002) provided a philosophical analysis of this topic. Classificatory problems are central for chemists, who have to distinguish and characterize millions of substances with no sensually obvious distinguishing features. How can we distinguish one white powder, or clear liquid, from millions of other like substances? Most of these substances are compounds of elements, not pure elemental substances, and hence are called “compounds.” This classificatory endeavor requires criteria for identifying material samples qualitatively, in terms of the chemical species they

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[372] Pitt (2000, p. 136) defines a ‘technological infrastructure’ as “*the historically defined set of mutually supporting sets of artifacts and structures without which the development and refinement of scientific knowledge is not possible*” (emphasis in original). Edwards (2010, p. 17) defines ‘knowledge infrastructures,’ which are technological infrastructures used to produce knowledge, as “*robust networks of people, artifacts, and institutions that generate, share, and maintain specific knowledge about the human and natural worlds*” (emphasis in original).

[373] The reproduction of scientific labor processes will be the subject of chapter 7.

represent. The chemical species identity of a material sample can be defined in terms of properties that are regarded as essential for sameness of chemical species. Drawing on Leibniz's law of identity, Schummer defines chemical species identity as follows: "Two material samples are chemically identical if and only if they possess all the same essential properties. If they differ in only a single essential property then they belong to different species."[374]

The crucial question is, of course, what properties are to count as essential. Schummer argues that chemists' answer to this question has changed over time. Before the structural approach to chemistry became dominant, the essential properties were taken to consist of a set of canonical properties, for example the method of preparation, the results of elemental analysis (including the empirical formula), the melting point or boiling point, visual characteristics, solubility properties, and exemplary chemical reactivities. Once the structural approach became dominant in the late 19th century, however, each substance was associated with a structure and the latter was taken to be the essential property. This one-to-one association of substance and structure was the basis for the structure determination approach to establishing identity. The chemical identity of a material sample was now to be established by determining its structure, using either classical methods or, later, instrumental methods.

As noted in section 4.7, classical methods in fact accomplished two goals. First, they allowed substances to be identified. Second, they provided information about the chemical reactions in which substances participate. Simplifying somewhat, classical methods of structure determination typically involved a four-step process. First, functional groups were identified by means of specialized chemical tests. Second, the molecule was broken down chemically (hence the term 'chemical degradation' to designate these techniques) into simpler and (one hoped) *known* fragments. These two steps could be conducted in parallel. They were repeated until sufficient information about the constituents of the structure had been accumulated so that it was reasonable to advance to the third step, the proposal of hypotheses for the entire molecule. At this point further

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[374] Schummer (2002), p. 189.

degradative experiments would be conducted to narrow in on the correct hypothesis.[375] Chemists could obtain further confirmation of a structural hypothesis by synthesizing the target structure from known fragments and comparing the chemical and bulk physical properties of the synthetic product to the target.[376]

Classical methods afforded information about specific points (functional groups, small portions of the carbon skeleton) in the molecule rather than information about the molecule as a whole. Indeed, reaction conditions that can fragment or otherwise alter a molecule at multiple points tend to be harsh and therefore less predictable and more likely to result in unexpected products, decomposition and rearrangement, all factors that lead to information loss or complicate the interpretation of the results.[377]

As an example, consider the sequence of reactions shown in Figure 5.1. This sequence was used to determine the structure of strychnine in a particular region of that molecule, represented by the partial structure at the top of the scheme. Strychnine was first subjected to oxidation with potassium permanganate. This reaction revealed the presence of a double bond in the region. The product, strychninonic acid, was then reduced with sodium-mercury amalgam, which revealed the presence of a ketone. The new product, strychninolic acid, was then subjected to basic conditions. These conditions generated two products, a small fragment (not shown) known as glycolic acid that was already known, and strychninolone-a. Reduction of strychninolone-a by catalytic hydrogenation (not shown) revealed the presence of a double-bond formed under the basic conditions. From this series of results, it was inferred that the region of interest had the structure shown at the top.

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[375] Hoffmann (2018) reconstructs several other historical examples.

[376] For example, Woodward & Brehm (1948) used hypothetico-deductivist reasoning and degradation experiments to settle the structure of strychnine, which prior to their work was thought to be one of two possible structures. Corroboration by synthesis only came six years later.

[377] See Wheland (1949), pp. 96-99 for a discussion of some of these complications.

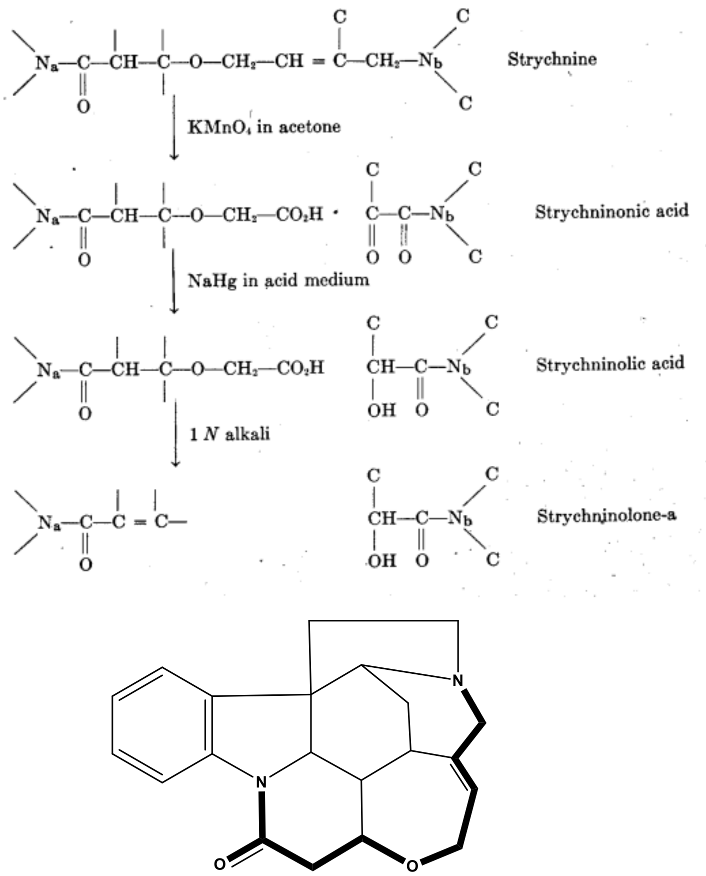

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Figure 5.1 A series of transformations observed in the classical determination of the structure of strychnine and the closely related brucine. The scheme is taken from an early review of the completed strychnine effort. Source: Holmes (1950), p. 388. Strychnine is shown at right, with the region of interest in bold.

Each reaction in this sequence provided a small amount of information about the functional groups and skeletal connectivity in a region of the strychnine molecule. With large projects, many such partial series had to be obtained. They could then be interrelated to provide a synoptic view of the evidence for the structure, as the chart in Figure 5.2 illustrates:

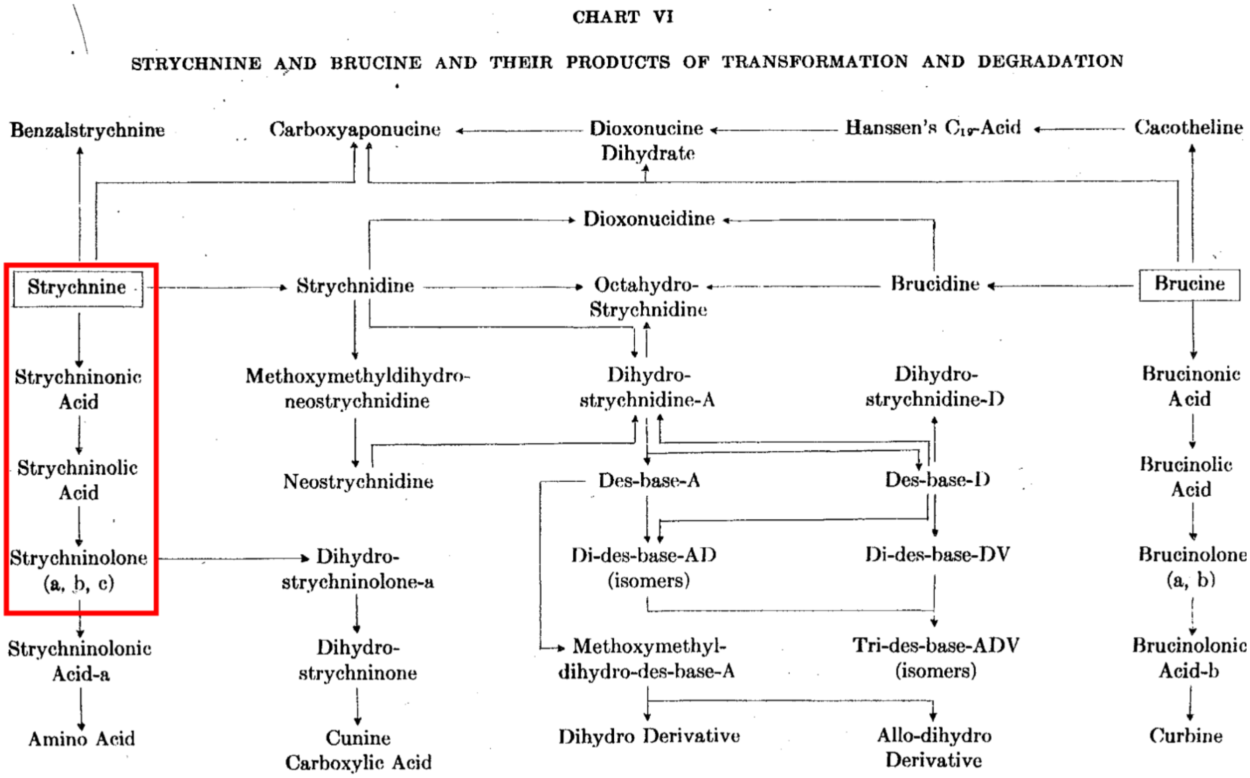

Figure 5.2 The network of chemical transformations observed in the classical determination of the structure of strychnine and the closely related brucine. Note how the partial series in Figure 5.1 reappears in the leftmost column of the chart. Source: Holmes (1950), p. 419.

This chart only displays the key transformations, and therefore in fact greatly underestimates the true number of experiments that were conducted to establish the structure of strychnine.

Though chemists are seldom explicit about the formal structure of their reasoning, sequences such as that shown in Figure 5.1 are easy to reconstruct as hypothetical inductions. For example, the first step in the figure could be reconstructed in hypothetico-deductive fashion: If the molecule has a double bond, then the latter should be cleaved under oxidative conditions and the product contain new oxygen atoms. If the consequent is observed, then the double-bond hypothesis is warranted. For hypotheses concerning larger portions of the molecule, one can find chemists using hypothetico-deductivism augmented by what Norton (2005) calls an “embellishment,” intended to tame the indiscriminateness of hypothetico-deductive confirmation. An English group centered around Robert Robinson, for example, appears to have favored either inference to the best explanation or inference to the simplest hypothesis,[378] and a 1949 textbook also advocates a principle of simplicity for the interpretation of reaction data.[379]

Chemical methods tend to be time-consuming, involving lengthy purifications and experimental processes. These technical limitations were compounded by the fact that individual chemical reactions do not yield much information about structure, even for simple cases, as the same textbook makes clear:

> … the study of the chemical reactions which substances undergo has in practice provided a convenient method for the determination of their structures. Although, as in the reaction between ethyl alcohol and hydrogen chloride, the possibility of a molecular rearrangement prevents the assignment of a definite structure to any substance on the basis of a single reaction, reliable conclusions can usually be reached, nevertheless, from a study of a number of related reactions. The aim of such a study is to obtain a self-consistent picture of the whole series of reactions …[380]

Many reactions had to be performed before reliable conclusions could be drawn about the whole structure. The unreliability of intermediate conclusions is illustrated for strychnine in Figure 5.3. Six hypotheses for the overall structure were at one time or another taken to be correct between 1910 and 1946, with the 1946 proposal being the final version. Each revision was forced by the acquisition of new information.

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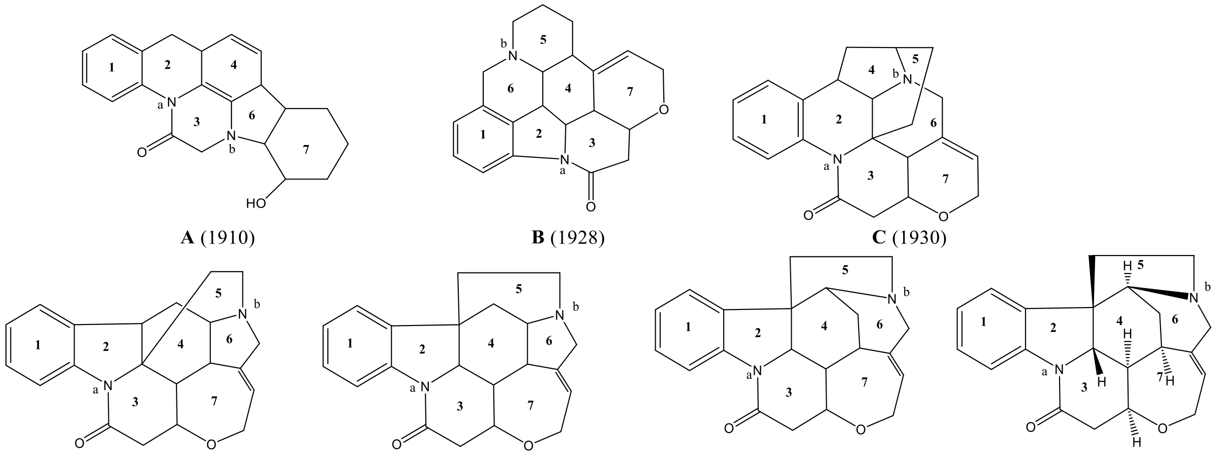
[378] Slater (2001) has noted the English group's appeal to simplicity in arguing for their preferred strychnine hypotheses. In some cases, though, they explicitly invokes IBE [Menon et al. (1930, 1932)].

[379] Wheland (1949), 95.

[380] Wheland (1949), p. 96.



A (1910) **B** (1928) **C** (1930)

D (1932) **E** (1932/1939) **1**, 1946 **1**, 1956 (stereochemistry solved)

Figure 5.3 Strychnine hypotheses over the years. The numbering of 1 follows the scheme proposed in R. B. Woodward and W. J. Brehm (1948). The rings of the other structures are numbered by analogy with this scheme.

Classical methods can be understood in terms of the distinction between narrow and wide instruments mentioned at the beginning of this chapter, at least if the distinction applies to methods as well as instruments.[381] Because of their specificity, they seem like a good fit for the narrow category, and their narrowness was to be preferred given the difficulty of interpreting reactions that produce multiple products. A principal epistemic disadvantage was that the accumulation of evidence was slow. This fact meant that the scope of a hypothesis that could be supported by the evidence early in the investigation might be quite narrow relative to the target structure. Attempts to solve the whole structure ran the risk of overreaching the available data, in some cases leading to a lengthy process of hypothesis revision, as illustrated above for strychnine.

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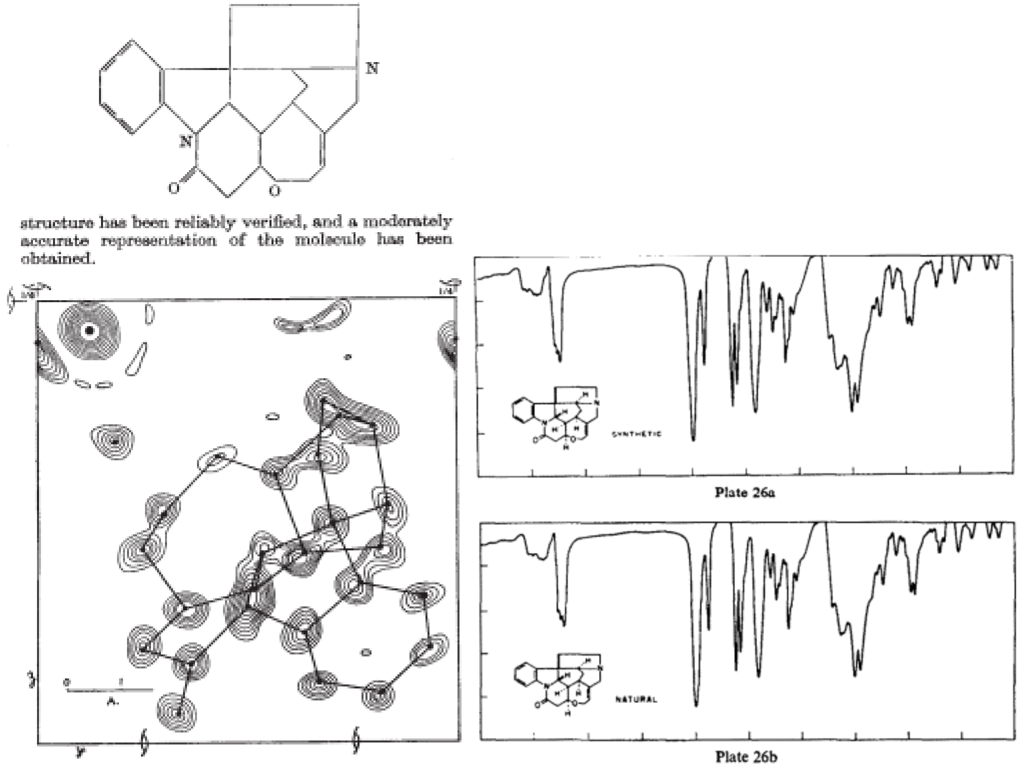
[381] My reason for extending the concept is that a chemical experiment typically requires several different tools and pieces of glassware to enable the activities described in section 3.2.

5.2.2 Instrumental methods

In contrast, the new instrumental methods seem like good candidates for the wide category. For example, at about the same time that the chemical investigation of strychnine's structure came to an end, the structure was also determined by the single-crystal X-ray diffraction technique. In this experiment, a crystal of a substance is inserted into a diffractometer, bombarded with a beam of X-rays, and the crystal is rotated until a reflection is detected. The complete set of data consists of the list of angles at which reflections are observed and their intensities. The latter are used to calculate an electron density map of the molecule. A model of the structure can then be constructed and fit to the map (Figure 5.4).

(a) (b)



structure has been reliably verified, and a moderately accurate representation of the molecule has been obtained.

Figure 5.4 (a) These diagrams are from one of the first reports on the crystal structure of strychnine. The top diagram is a two-dimensional representation of strychnine. The bottom diagram is a contour map of the electron density on which a projection of the structure has been superimposed. Source: Robertson & Beevers (1950), p. 690. (b) Infrared spectra of synthetic (top) and natural (bottom) strychnine. Source: Woodward et al. (1963), p. 283.

The method counts as wide because each experiment yields many measurements. The electron density map provides a synoptic kind of evidence that allows structural hypotheses to be confirmed relatively quickly. The map itself is not produced by the experiment, of course, and many inferential steps are needed to make use of the data. But the gain in productivity is undeniable: Whereas over 245 papers were contributed over 60 years towards solving strychnine chemically,[382] only 6 were required over 5 years for the X-ray structure.[383]

All of the new techniques were based on the same basic principle: A substance is to be subjected to some energy probe and its response recorded as a spectrum (or a diffraction pattern for X-ray crystallography). A single experiment yields all of a substance's responses to the probe which, with the exception of X-ray crystallography, can then be analyzed using the rules of interpretation discussed in sections 4.5.2 and 5.1. Thus in addition to X-ray crystallography and infrared spectroscopy, of the other techniques that came into common use at the time ultraviolet spectroscopy records the response to ultraviolet light, NMR to radiofrequency radiation in the presence of a magnetic field, and mass spectrometry to bombardment by an electron beam of a definite energy.

In order to organize the large quantities of data produced in spectrometric experiments, representational techniques like spectra and tables were used. For example, in 1962 Robert Silverstein and G. Clayton Bassler of the Stanford Research Institute published a short primer on the "Spectrometric Identification of Organic Compounds" in the *Journal of Chemical Education.* The article explicitly contrasts chemical and spectrometric methods, arguing that the latter are superior in terms of the informational pay-off, the amount of time required, and the quantity of compound needed for a structure determination. They present two examples of structure determination using the new methods. In each case, only four experiments were needed to produce a large number of data points, sufficient to conclusively determine the structures. The data are presented in four spectra or tables, as shown in Figure 5.5:

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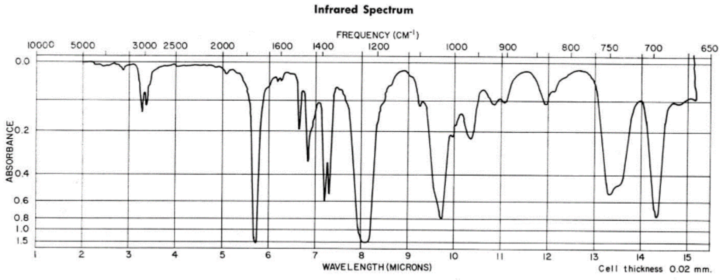
[382] Huisgen (1950).

[383] See Slater (2001), footnote 78.



Mass Spectral Data (Relative Intensities)

m/e	% of base peak	m/e	% of base peak
27	6.	77	22.
38	5.	78	6.
39	20.	79	26.
41	4.	89	13.
42	3.	90	47.
43	73.	91	71.
50	11.	92	6.
51	23.	105	6.
52	6.	106	3.
62	3.	107	20.
63	9.	108	100.
64	3.	109	8.
65	18.	150	28.70
77	22.	151	2.84
		152	0.26

Isotope Abundances

m/e	% of P
150 (P)	100.
151 ($P + 1$)	9.9
152 ($P + 2$)	0.9

Ultraviolet Data

$\lambda_{\max}^{\mathrm{EtOH}}$	$\epsilon_{\max}$
268	101
264	158
262	147
257	194
252	153
248 (shoulder)	109
243 (shoulder)	78

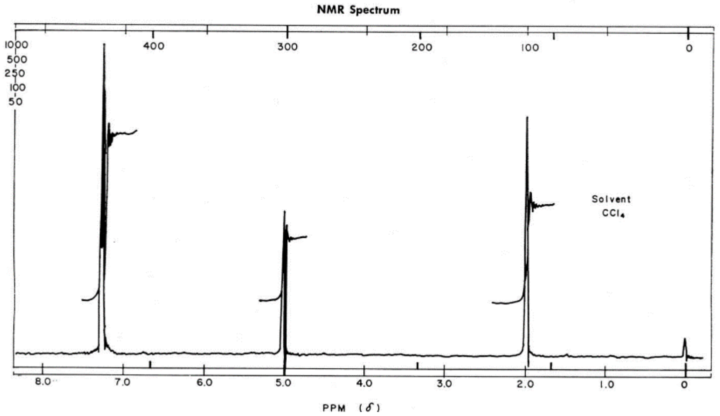


Figure 5.5 Data for benzyl acetate. Source: Silverstein & Bassler (1962), p. 548.

Silverstein and Bassler make use of virtually no theory, chemical or physical, in their interpretations of the data. Rather, they employ simple rules, such as those embodied in the correlation chart shown in Figure 4.5, in order to infer from data to structure. Since mass spectrometry results in a fragmentation pattern, somewhat more theory is needed in order to understand the mechanisms of fragmentation, but even so the data can generally be interpreted by applying a few "general rules:"

> The procedure for obtaining an empirical formula will be demonstrated as we work through the sets of spectra presented below. We found it advisable to remind students of the "nitrogen rule": an odd-numbered molecular weight permits only an odd number of nitrogen atoms, and an even-numbered molecular weight permits only an even number of nitrogen atoms (including zero).

> Now let us consider the fragmentation pattern. A number of general rules for predicting prominent peaks can be written and rationalized using concepts of statistics, resonance, hyperconjugation, polarizability, and inductive and steric effects. For example:
>
> 1. Cleavage is favored at branched carbon atoms
> 2. Aromatic compounds generally give a larger parent peak than do aliphatic compounds
> 3. Double bonds favor allylic cleavage
> 4. Saturated rings lose side chains at the α-carbon; special case of branching
> 5. In alkyl substituted aromatic compounds, cleavage is most probable at the bond beta to the ring
> 6. A heteroatom will induce cleavage at the bond beta to it.
>
> A feeling for these modes of cleavage, plus a reference library, form the basis for use of mass spectrometry for identification purposes.[384]

So was the difference between the old and new methods, as far as the cognitive aspect of structure determination was concerned, simply that chemists could accumulate structural information faster? In 1983, the celebrated head of process research at Merck & Co., Max Tishler, made an intriguing reminiscence about the classical period:

> in those days, as you well know, structure determination was quite different—so different that young people today just haven't the slightest idea how this was done. Yet all the important chemical work that developed was done by methods which today are no longer useful. We have such better tools today. Spectroscopic methods: NMR, IR and mass spec just change the complexion of chemistry completely. *It's amazing how we got information by deduction. By means of the logical application of thinking and deduction we were able to establish structure.* Most of the time we were right.[385]

This statement is astonishing: surely "thinking and deduction" are still applied "logically" in structure determination work? The new methods did not automate the *interpretation* of the data. Yet if we take Tishler's reminiscence seriously, there was a significant change in the cognitive aspect of structural chemistry. What was it? He himself does not provide the answer in the interview from which this quotation is taken.

I think the key is the sentence "it's amazing how we got information by deduction." G. W. Wheland's thoughtful 1949 textbook, *Advanced Organic Chemistry*, may provide a clue. In the chapter on "Structural Isomerism," Wheland discusses both chemical and

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[384] Silverstein & Bassler (1962), p. 550.

[385] Tishler (1983), p. 13. Emphasis added.

physical methods. By “physical methods,” he means primarily the instrumental methods discussed in this chapter, but also older techniques like boiling and melting point measurements. In 1949, the instrumental methods were still in their infancy, and had to be used in conjunction with chemical methods. Nevertheless, Wheland considers them superior to chemical methods for the following reason:

> they have the great advantage of dealing with single molecules and not with the much more complex interaction between two or more molecules. *In fact, all the chemical methods discussed above require that deductions be drawn from chemical reactions, or, in other words, that the structure of a molecule be inferred from the way it is derived from, or transformed into, a different molecule. Thus, even the methods based upon isomer number require the assumption of the principle of minimum structural change*. The physical methods, on the other hand, involve much less drastic changes in the molecule being examined, and they leave the molecule in the same state after the experiment that it was in before. As a very rough analogy, which greatly exaggerates the advantages of the physical methods, the chemical methods of establishing structure might be compared to the determination of the shape of an egg by examining either the hen that laid it or its fragments after it had been hit by a hammer, whereas the physical methods might be compared to the determination of the shape by looking at the egg itself.[386]

According to Wheland, chemical methods require an inference from “a different molecule” to the target. The structure determination of ethyl alcohol provides an example of this process:

> One of the first “proofs of structure” to be given in courses in elementary organic chemistry is often that of ethyl alcohol. The argument frequently runs as follows. The molecular formula of ethyl alcohol is found, from its analysis and from a determination of molecular weight, to be C_2H_6O. By trial and error, one can easily convince himself that there are two and only two structures, namely, I [Figure 5.6] and II, which correspond to this molecular formula and which satisfy the requirement that the carbon atoms be quadrivalent, that the oxygen atom be bivalent, and that the hydrogen atoms be univalent … Consequently, the structure of ethyl alcohol must be either I or II, and the problem is to decide which one of these is right.[387]

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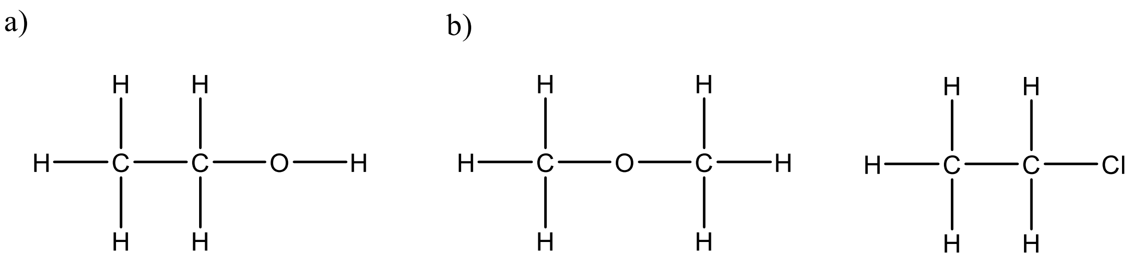
[386] Wheland (1949), p. 127. Emphasis added.

[387] Wheland (1949), p. 93.



I II III

Figure 5.6 (a) Candidate structures for ethyl alcohol. (b) Ethyl chloride. Adapted from Wheland (1949), p. 92.

Wheland then illustrates how chemical reaction data can be used to choose between I and II:

> The decision between structures I and II for ethyl alcohol is now made on the basis of the chemical reactions of the substance. In the first place, although altogether six hydrogen atoms are present in the molecule, only one can be replaced by an active metal like sodium. The inference is then drawn that one hydrogen atom is essentially different from the other five. Since this condition is satisfied by structure I but not by structure II, ethyl alcohol can therefore be assigned structure I, so that methyl ether must be assigned structure II. Further evidence leading to the same conclusion is provided by the reaction which occurs between ethyl alcohol and hydrogen chloride, and which leads to ethyl chloride C_2H_5Cl. For this latter substance, if the chlorine atom is assumed to be univalent, only the one structure III can be drawn to satisfy the molecular formula. Consequently, since ethyl chloride must therefore have structure III, ethyl alcohol must have structure I, inasmuch as this structure is much more closely related to III than is II.[388]

Wheland goes on to point out some weaknesses of these arguments. The argument from the reaction with sodium depends on the assumption that if ethyl alcohol had two equivalent hydrogen atoms, then if one of them can be replaced by sodium, then one of those remaining must react similarly. Since there are compounds with two equivalent hydrogen atoms, like H_2O or oxalic acid, in which the corresponding assumption is false, it follows that the conclusion in the case of ethyl alcohol is at best tentative. On the other hand, the argument from the reaction with hydrogen chloride assumes a principle of minimum structural change by assuming that the precursor to III must be the most closely related to it of the candidate structures I and II. As mentioned above, however, this

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[388] Wheland (1949), p. 93.

assumption presupposes that no rearrangements intervene in the transformation of ethyl alcohol into ethyl chloride. As a general rule of structure inference, however, the no-rearrangements requirement has many counter-examples.

My suggestion is that the need for assumptions like the principle of minimum structural change fundamentally arises from the nature of chemical evidence. As Wheland's egg analogy indicates, chemical evidence always involved at least two structures, that of the target and that of the precursor from which it was formed or the product into which it was transformed. Once the structure of the product or precursor was known, it could be combined with the experimental data to suggest a target structure. Thus, chemical evidence essentially involved a *structure-to-structure* inference. This kind of inference is vulnerable to the fact that multiple isomers can be drawn for most chemical formulae. It is also vulnerable to the fact that the known structure might enter into, or be produced by, more than one reaction pathway under the conditions used to produce the data. Moreover, an error in the "known" structure could propagate to the deduction of the target structure.[389] In this regard, it is worth keeping in mind that in many cases, a known stucture could only be arrived at after several degradative steps, requiring a chain of inferences back to the target structure. Error could be introduced at each inference in the chain. In order to deal with these vulnerabilities, structure-to-structure inferences had to be constrained by the experimental data together with sometimes fragile assumptions about reaction mechanism, reactivity, etc.

Modern methods dispense with the structure-to-structure inference. They are thus more "direct," as noted in one textbook,[390] because they involve an inference from the data to the target without bringing in other structures. The absence of a second structure is due to the non-destructive nature of the methods. Though *data-to-structure* inferences are vulnerable to the multiple isomer problem, they are, of course, not so to the multiple reactions problem or the possibility of propagating errors in the "known" structures.

On the other hand, there is always a risk that a structure will violate the rules of

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[389] There are many examples, but see Smith (1965), p. 596 for a significant one in the strychnine effort.

[390] Streitwieser, Heathcock & Kosower (1992), p. 322.

interpretation, since these are based on statistical correlations. Moreover, each instrumental technique has its weaknesses. For example, X-ray crystallography does not reveal the positions of hydrogen atoms (those shown in any crystal structure have always been drawn in), a lacuna that can lead to errors in the interpretation of atomic and functional group identities. Technique-specific deficiencies can be overcome to a large extent by using multiple techniques to check for convergence. In any case, whatever the weaknesses of modern methods, they seem to pale in comparison to the uncertainty introduced when data are produced by destroying the target.[391]

One exception to the non-destructive character of the modern methods is mass spectrometry. The exceptionality, however, is mitigated by two circumstances. First, mass spectra very often have a peak for the molecular ion, which is not a fragment. This peak is used to infer the molecular weight, and from this the empirical formula, of the target. Second, the complicating effects of fragmentation on structure determination is mitigated by the spectrometer's *wideness* itself—many fragments are produced and detected in a single experiment. So if the molecular ion peak is missing, then with the aid of rules of interpretation the fragment data can be correlated with each other and other spectra to infer the molecular weight.[392]

Since the theme of this chapter is heuristics, the distinction between structure-to-structure inference and data-to-structure inference raises the question of how this distinction is related to heuristics. In logic, an inference is defined as a process of linking propositions by affirming one proposition on the basis of one or more other propositions.[393] In structure-to-structure inferences, possible target structures were affirmed on the basis of known structures, which were themselves affirmed on the basis of experimental data.[394] In data-to-structure inferences, on the other hand, possible target structures were affirmed on the basis of experimental data directly. These processes concern the manner in which hypotheses are generated. I have provided grounds for thinking that data-to-structure

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[391] For a recent review of the pitfalls of modern structure determination, see Nicolaou (2005).

[392] Silverstein & Bassler (1962), p. 547.

[393] Copi & Cohen (2005), p. 7.

[394] I am here assuming, of course, that a structural representation is equivalent to a proposition.

inference was less error-prone than structure-to-structure inference. Since heuristics are concerned with efficiency, it follows that data-to-structure inference is more efficient than structure-to-structure inference. That is, the odds of generating the correct hypothesis through data-to-structure inference are greater than with structure-to-structure inference.

The switch from a structure-to-structure mode of inference to a data-to-structure mode was accompanied by the change of goals noted earlier. Classical structural chemistry had two principal goals: (1) identify a substance in terms of a structural representation, and (2) learn about the chemical reactions in which the substance participates. The latter goal was important in its own right and not just as evidence for theoretical claims. It was important partly because acquiring this knowledge suggested biogenetic relationships between different naturally occurring substances, that is, relationships concerning how the substances are produced in nature. It was also important because knowing how a substance behaves chemically is useful for synthesizing new substances starting from known ones. Classical methods achieved both goals simultaneously. Modern structural chemistry has kept (1) as a goal, but not (2), because the spectroscopic properties that are now employed to achieve (1) are of little chemical interest in themselves.

Summing up, it seems to me that the key cognitive differences between the classical and modern methods, with respect to structure determination, are (1) the rate of information accumulation and (2) the necessity or lack thereof of structure-to-structure inferences. Modern methods provide more information, and allow for more secure reasoning from data to structure. For these reasons, they are more efficient. Both of these differences with respect to classical methods stem from differences in material practices—the switch from narrow to wide methods, and from destructive to non-destructive ones.

At this point, one might be tempted to doubt a central feature of my argument thus far. If the superiority of the new methods lies to a large extent on their non-destructive nature, does this feature call into question the emphasis I have been placing on the transformation of labor?

In reply, I will point out that the difference between structure-to-structure and data-to-structure inference is rooted in how the data are produced. That is, the change in the data production process changed the nature of the inference from data to hypothesis. (I speak of a “production process” here because, as was point out at the beginning of section 5.2, my argument concerns whole groups of instruments and their conditions of use (“material conditions” or “technological infrastructure” or whatever) the stability of which gives the scientific work its characteristic features in a given period.) It is also worth noting that the content of the inference in either case is not an abstract logical schema like hypothetico-deductivism or demonstrative induction (though such schemata provide the form of the inference) but refers to what happens in data production. The content of the inference is determined by the content of the production process. In the classical period, the content of the process was the transformation of one substance into another. The content of the inference was therefore a logical relationship between structures. In the modern period, the content of the process is the absorption of energy by the molecule. The content of the inference is therefore a logical relationship between the frequency of absorption and structure (or mass-to-charge ratio and structure).

The data production process is part of the labor process. Only the transformation of the latter made possible the transformation of the former, which in turn made possible a different mode of inference.

There are other examples in the literature of the labor process determining the logical features of experimentation and discovery, though to my knowledge I am the first to make this point explicitly. For example, Franklin (2005) argues that narrow instruments are most efficient when their use is directed by a specific hypothesis about the system under investigation. Wide instruments allow scientists to take a more inductivist approach, because of the large number of data points they afford per experiment. Perovic (2010) examines the evolution of high-energy physics experiments in the second half of the 20th century and documents a tendency towards greater automation in data processing. He argues that fully automated data processing is most efficient for experiments intended to confirm specific hypotheses, for example the existence of the Higgs boson. But they are less useful for exploratory experiments—experiments intended to discover novel phenomena not predicted by theory; for such experiments, Perovic argues, a semi-automated regime is preferable. Griesemer (1991) shows how the experimenter’s causal agency—*via* laboratory manipulations—structured inferences in evolutionary biology experiments. In one kind of experiment, an unobserved cause was inferred based on an appeal to the scientist’s agency. In another kind of experiment, an analogical inference was made from the process studied in the lab to the process as it occurs in the wild, again based on an appeal to the experimenter's agency.

5.3 Conclusion

In conclusion, I suggest a historicist approach to the question of heuristics. Thus, rather than identify a set of abstract procedures for simplifying modelling work, as does Wimsatt, or develop a general taxonomy of instruments based on their efficiency characteristics, as does Franklin, I propose to view heuristics as historically specific solutions to historically specific problems encountered in specific disciplines. This proposal reflects what we have observed for the history of chemistry covered in this dissertation. My analysis suggests that the progress of early 20th century chemistry was impeded by two historically specific obstacles. One was the insular nature of chemical methods and know-how, the reliance on chemical reactions to solve analytical problems. The other was the cognitive correlate of this reliance: the use of structure-to-structure reasoning to make analytical claims.

In what sense were chemical reactions and structure-to-structure reasoning "obstacles"? Recall one of the principal goals of classical chemistry: to identify a substance in terms of a structural representation. This is not the only way to identify a substance. A fixed canon of properties can be used instead, for example description of the preparation method, empirical formula, melting or boing point, visual characteristics, solubility properties, etc. But after chemical structure theory was accepted in the 19th century, chemical species came to be identified in terms of their structures. The problem was how to develop evidence for the structures, given that the observational basis of the field (chemical reactions) involved their destruction. These methods were indirect—requiring an inference from a different molecule to the target—and subject to the difficulties, described above, arising from the specific features of this indirectness. The indirectness constituted the obstacle. True, the indirectness was desired, in a sense, since, as mentioned earlier, besides structure determination one of the goals of substance identification was to learn about the substance's characteristic chemical relations to other substances. The means for satisfying the latter goal were not optimal for satisfying the former (see below). Scientists' goals do not always co-exist harmoniously.

In suggesting a historicist approach to heuristics, I draw on two sources for inspiration. The cognitive psychologist Howard Margolis (1993) distinguishes two kinds of scientific revolutions, depending on which kinds of problems they solve. Some revolutions bridge *gaps*. Others overcome *barriers*. Margolis focuses on barriers. The kind of barrier he is interested in is deeply ingrained "habits of mind." Such habits are necessary for efficient scientific work within any specialty discipline, and therefore have a positive function in science. On the other hand, they constitute barriers to alternative conceptions. More broadly, deeply ingrained cultural habits of mind can close off opportunities that seem obvious with the hindsight of later generations. The phlogiston case provides an example of a habit of mind. According to Margolis, the difficulty that many chemists found in accepting Lavoisier's oxygen theory of combustion was due to a habit of mind derived from the familiar experience of seeing something burn, where one sees flames leap up and dissolve in the air until the burning substance is gone. Such familiar experiences become deeply entrenched in our intuitions and for that reason entangled in scientists' theorizing. In another example, Margolis argues that the formal models of Ptolemy, Tycho and Copernicus had a tight family relationship despite great differences in cosmology. Kepler's models are radically different, in particular the use of elliptical orbits and the equal-areas rule governing speed instead of the traditional apparatus of interacting epicycles. The barrier crossed by Kepler but not his predecessors was the assumption of uniform circular motion in the heavens, which was derived from familiar experiences of circular or cyclical motion (a wheel turning, the Sun rising) that appears to result from uniform circular motion. These familiar experiences produced a habit of mind that then became entangled in astronomical practice.

The theme of cognitive barriers in science can also be found in the writings of the philosopher Gaston Bachelard, who initiated the French tradition of historical epistemology. A key term in Bachelard's philosophy of science is that of 'epistemological obstacle.' Roughly, epistemological obstacles are pre-scientific ways of knowing that stand in the way of acquiring genuine scientific knowledge. These pre-scientific ways of knowing are unconscious, but are weeded out through the recognition of errors in the succession of theories of a domain (e.g., the succession of theories of mechanics in physics). As with Margolis' habits of mind, these pre-scientific ways of knowing are rooted in everyday experience. Examples of such obstacles are direct observation (which can lead thought astray by directing excessive attention to the sensual qualities of a phenomenon), hasty generalizations, verbal obstacles (in which the image associated with a word substitutes for an explanation), pragmatic knowledge (in which phenomena are explained according to their utility), substantialism (the invocation of a material support to explain a phenomenon), animism (the attribution of properties of living organisms to inanimate objects) and quantitative knowledge (insofar as it is used to mask subjective whims and errors, e.g. those induced by direct observation).

According to Bachelard, the overcoming of epistemological obstacles leads to "epistemological breaks" in the history of a science, which involve not just the rectification of errors but also significant reorganization of knowledge. Epistemological obstacles introduce discontinuities into science. In addition, each obstacle is "polymorphous," having a structure specific to the state of a science at a given time. Unlike Kuhn's thesis that scientific revolutions have an invariant structure, the structure of a Bachelardian epistemological break depends on the specific history of a science. One should not expect to find a general pattern underlying the histories of the different sciences.

Both Margolis' habits of mind and Bachelard's epistemological obstacles are *cognitive* impediments to scientific progress; they concern ways of thinking that hinder progress. In this chapter and the preceding one, I have been concerned primarily with *material* impediments to progress, by which I intend the conditions of action in the field. The conditions of action include the instruments, methods, knowledge and social relations that are normative in the field. The pre-Instrumental Revolution chemists were unable to improve the observational basis of chemical analysis due to their dependence on chemical reactions. The impediment here was a way of doing rather than a way of thinking. This way of doing involved a historically specific relation of the chemist to his instruments, namely that the know-how required to use, understand, and to some extent make them fell within his realm of expertise.

In terms of the categories of knowledge introduced in chapter 2, one might say that the EK chemists used to support their claims to TK (claims about chemical structures) was produced by the exercise of MK and IK, the basic principle of which was the chemical reaction. This principle had to be discarded in order to acquire EK that could support the TK differently—more directly and efficiently.

This way of characterizing the change should make clear that by 'impediment' or 'obstacle,' I intend a feature of the circumstances in which scientists are working whose elimination or overcoming is a necessary precondition for specific further changes. The idea is that earlier developments produce the necessary preconditions for later ones, in this case by overcoming such features of the circumstances. Thus the notion of an impediment highlights a form of dependency of later developments on earlier ones. In the case at hand, the initial circumstances were the methods of classical structure determination. Their indirectness was a key feature that prevented more efficient structure determination. The development of instrumental methods made possible more efficient structure determination, which in turn enabled the turn to synthesis rather than analysis as the main task of organic chemistry.

To call the relation of classical chemists to their instruments an "impediment" might seem counterintuitive, for normally expertise in the instruments one is using is considered epistemically beneficial. But, it turned out, this way of doing impeded progress in MK and IK. But removing the impediment required establishing a new historically specific relation to their instruments, which involved a relation of epistemic dependence on experts and knowledges external to the field.

It is worth noting here that in order to see how progress was made in this particular episode it is necessary to invoke not just epistemic categories like know-how or methods, but also social ones pertaining to the labor process, such as the inter-disciplinary division of labor.

If we adopt Laudan's characterization of heuristics as the branch of scientific methodology that is concerned with identifying strategies and tactics that will accelerate the pace of scientific advance, then we see that in this case, at least, the strategies and tactics that accelerated scientific advance were solutions to a historically specific problem encountered in chemistry: once chemical structure theory was accepted in the 19th century, the problem was how to develop evidence for chemical structures, given that the observational basis of the field (chemical reactions) involved their destruction? The solutions adopted were also historically specific. Until the development of a theory of bonding and a technology that permitted exploitation of the theory, chemists were forced to accept the observational basis they had developed internally. Once external circumstances changed, however, a new solution became feasible.[395]

Such material impediments result from the simple but underappreciated fact that scientists do not always, and often cannot, develop techniques and instruments out of whole cloth to fit novel theoretical situations or research areas. Instead, they use the means available to them until better means are developed. The old means have a positive effect, insofar as they allow research to proceed in the novel situation. But they also have a negative effect, insofar as they keep the field on a sub-optimal developmental path. This is a question for the study of heuristics: do the means available at a given stage of a science keep the field on a sub-optimal path? This question leads to others. For one, it seems to imply a great deal of contingency, in that the results of the field seem to depend on the path imposed by the old means.[396] And what constitutes “sub-optimality?” For the chemical case, I have suggested an answer: the nature of the evidence and method of inference used in classical chemistry was sub-optimal relative to that used in modern chemistry. But these features need not be the only criteria for judging optimality.

This last point is related to a further question, why the means available have this effect. This requires not only study of the means, but also of the ends. If the field is pursuing multiple goals, improving the means to better attain one goal may not be optimal for attaining another goal. Moreover, which goal is chosen will affect the future modes of making progress. Once the instrumentation has been chosen, future progress will be constrained by that choice. Progress is thus path-dependent. In addition, the multiplicity of

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[395] That said, I am not denying the possibility of identifying general strategies for accelerating scientific advance. The value of the historical approach, however, is that it is sensitive to the specific impediments imposed by a scientific domain. Understanding these impediments is crucial for understanding the history of the science.

[396] See Soler et al. (2015) for a discussion of the issues involved in determining whether the results of science are contingent or inevitable.

goals poses a problem for assessing overall progress. If the goals are not comparable, then there can be no assessment. Whether or not the goals were comparable in the case at hand, I leave for future research.[397]

Finally, we can also ask how and why the field was able to overcome its material impediments. This was the subject of chapters 4 and 5, for the chemical case. I will have more to say about such overcoming in the dissertation conclusion, where I identify a process I call ‘rationalization’ that scientists engage in to overcome such obstacles.

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[397] Kitcher (2017) discusses the path-dependence of progress and the problem of the multiplicity of goods in the context of rehabilitating the concept of social progress.

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6.0 LABOR AND MODELS OF SCIENTIFIC PROGRESS

In this chapter, I will revisit the question of theories of scientific progress and how well they fit episodes like the Instrumental Revolution. From a philosophy of science perspective, this episode has the peculiarity of having (i) a massive effect on the way chemistry was done but (ii) not involving the overthrow of one theory by another. This peculiarity poses a *prima facie* challenge for philosophical theories of scientific change and progress, which have been focused on disruptive theory changes. Nevertheless, the episode did involve some theoretical adjustments, namely the introduction of quantum chemical and physical organic concepts into chemical structure theory. Moreover, it was disruptive in some ways, requiring chemists to adopt a new *instrumentarium*, change their research priorities, and accept a new dependence on the availability of capital and the work of other actors, like instrument-makers and methods developers. So in this chapter, I propose to take a closer look at a few prominent philosophical theories of scientific change and progress and assess how well they fit this episode. I have selected four theories that (i) have been influential, (ii) are well-worked out, and (iii) look promising, *prima facie*, for explaining the Instrumental Revolution: Kuhn's theory of scientific revolutions, Laudan's reticulated model of scientific change, Lakatos' methodology of scientific research programs, and Hull's evolutionary theory.

I argue that none of these models of scientific change are adequate for the case of the Instrumental Revolution because none of them incorporates changes in instrumentation and systems of labor. In section 6.4 it is argued that the accumulation of knowledge is intimately connected with the nature of human labor, in contrast to theories of science that take scientific progress to be analogous to biological evolution. In the conclusion I make use of the connection between labor and knowledge accumulation to advance an augmented reticulational model that is based on, but significantly modifies, Laudan's original model. The augmented model presents elements of a theory of scientific progress. First, the model interprets progress as an accumulation of knowledge, in the broad sense of knowledge discussed in chapter 2. Second, the model presents a more complete mechanism responsible for this progress than the models discussed in the earlier sections. In this mechanism, the accumulation of knowledge figures not only as a goal of science, but as a starting-point for scientific research. The mechanism suggests ways in which the accumulation of knowledge and the labor process mutually condition each other. I also argue that the mechanism is not without frictions, and that these frictions are themselves explanatory of certain kinds of scientific change.

6.1 Was the Instrumental Revolution a Kuhnian revolution?

As is well-known, Kuhn suggested that scientific revolutions involve a distinctive sequence of stages. The pattern begins with a period of "normal science," during which scientists apply and extend a dominant "paradigm." Though the latter term is notoriously polysemous in Kuhn's writings, the dominant meanings are either that of "exemplar" or "disciplinary matrix." The former is a narrow sense of "paradigm," standing for an influential example of scientific work. Examples are Newton's discovery of the law of universal gravitation, Lavoisier's discovery of the oxygen theory of combustion, or Darwin's discovery of the theory of evolution by natural selection. A "disciplinary matrix," on the other hand, signifies something broader and includes shared beliefs, values, instrumentation and techniques. The paradigm in the narrow sense shows how to combine some of the components of the paradigm in the broader sense so as to conduct successful research. For example, Lavoisier showed how the balance, combustion apparatus, commitments to precise measurement and a "building-block" view of matter, together with the laws of conservation of mass and the constancy of chemical composition could all be effectively combined to do chemical research.

In the course of doing normal science, "anomalies" or phenomena that resist explanation by the prevailing paradigm might accumulate. Under certain highly context-specific circumstances, these anomalies might provoke a 'crisis,' that is, a period in which the paradigm is thrown into doubt. This is the second stage of a Kuhnian revolution. If there is a rival paradigm available, the crisis might lead to a period of conflict during which supporters of the second paradigm seek to overthrow the dominant one. If they are successful, then the discipline enters a third stage, namely a new period of normal science under the victorious second paradigm.

As noted above, there is disagreement among historians as to whether the Instrumental Revolution fits this model. Morris and Travis (2002) think it does, drawing on Kuhn's claim that paradigms in the narrow sense "provide direction to a scientific activity by 'implicitly [defining its] legitimate problems and methods."[398] Since the methods of chemical analysis underwent radical change in the period we have been concerned with, Morris and Travers suggest that it amounts to the overthrow of the ruling paradigm by another.

In contrast, Baird (2002) argues that the episode involved no crisis of the sort described by Kuhn, when normal science encounters a problem that its established methods cannot solve. Baird concludes that the Instrumental Revolution does not qualify as a revolution in Kuhn's sense. And the evidence does seem to support the view that chemistry did not experience a Kuhnian crisis.

On the other hand, it does not follow that there was no crisis at all. Baird himself documents a profound crisis of identity within analytical chemistry. The new instrumental methods were very different from the traditional chemical methods of analysis, necessitating a different training, entailing different research priorities, and involving a different relationship of the chemists to their work (for example, Liebhafsky (1962), p. 29A observes that analytical chemists are increasingly required to act as personnel managers directing the technicians running the instruments). As I noted above, some organic chemists also resisted the new methods on the grounds that they completely neglected the chemistry of substances. Though the resistance fell short of a "crisis," there was at least potentially a conflict over values, over what kinds of chemical knowledge were worth pursuing and what kinds of training were important.

If one wanted to maintain a Kuhn-like interpretation of the Instrumental Revolution, despite the absence of anomalies, it might be useful to adopt a pragmatic interpretation of 'paradigm' articulated in Rouse (2003. Rouse holds that "paradigms

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[398] Morris & Travis (2002), p. 80; Kuhn (1996), p. 10.

should not be understood as beliefs (even tacit beliefs) agreed upon by community members, but instead as exemplary ways of conceptualizing and intervening in particular situations." To accept a paradigm, on Rouse's view, is "more like acquiring and using a set of skills than it is like understanding and believing a statement." [399] In the context of this discussion, a virtue of this approach, for a Kuhnian, is that it takes the pressure off discrepancy between theory and observation as the driver of scientific change. Since agreement about what to believe about nature is not essential for a paradigm, challenges to theory need not be the only driver of change:

> Scientists *use* paradigms rather than believing them. The use of a paradigm in research typically addresses related problems by employing shared concepts, symbolic expressions, experimental and mathematical tools and procedures, and even some of the same theoretical statements. Scientists need only understand *how* to use these various elements in ways that other would accept. These elements of shared practice thus need not presuppose any comparable unity in scientists' beliefs about what they are doing when they use them. Indeed, one role of a paradigm is to enable scientists to work successfully without having to provide a detailed account of what they are doing or what they believe about it.[400]

One might even put Rouse's point in terms of labor. A paradigm is an example of how to combine the elements or conditions of scientific labor and carry out the work itself. In Marxian terms, it represents a "mode" or "way" of producing; one might also relate the idea to Miguel Garcìa-Sancho's (2012) conception of molecular sequencing, based on sequencing instruments, as a "form of work," a conception itself inspired by John Pickstone's (2000) idea that a scientific discipline at a given time is characterized by a "way of working."[401] Though Rouse does not offer a different account of crisis formation from Kuhn's, he does describe the crisis itself in a way that can easily be connected with my emphasis in this chapter on the organization of work:

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[399] Rouse (2003), p. 107.

[400] Rouse (2003), p. 108.

[401] One could also relate Rouse's point to the notion of a labor-principle introduced in section 4.8. This is a principle or strategy for organizing labor. A paradigm, as described by Rouse, might involve a labor-principle, but it is broader, entailing not just an organization of labor but also theory, problem-solving techniques, and standards of solution.

> The more basic issue [than agreement on theory] between proponents of alternative paradigms concerns how to proceed with research: what experimental systems or theoretical models are worth using, what they should be used for, what other achievements must be taken into account, and what would count as a significant and reliable result. The conflict is not so much between competing beliefs as between competing forms of (scientific) life … Such conflicts can be difficult to resolve precisely because the protagonists now *work* in different worlds.[402]

If what is at issue are competing forms of scientific life, then surely this could also include the nature of the labor process. The recognition of scientific activity as a form of labor requires that the question of "how to proceed with research" include as a sub-question what ways of organizing research—which include both relations between humans and human-instrument relations—are conducive to successful research, given the experimental systems, theoretical models, goals, etc. that must also be taken into account.

The last sentence of the last quotation from Rouse echoes, of course, Kuhn's claim that "the proponents of competing paradigms practice their trades in different worlds."[403] Given his pragmatic interpretation, Rouse understands the "different worlds" trope not in terms of perception, as Kuhn did,[404] but rather in terms of the possibilities for action offered by the paradigms:

> [Paradigms] reorganize the world as a field of possibilities, offering differently configured challenges and opportunities. If proponents of different paradigms do not fully communicate, it is not so much that they cannot correctly construe one another's sentences or follow one another's arguments. The problem is more that they cannot grasp the *point* of what the others are doing or recognize the *force* of their arguments.[405]

Rouse does not seem to think that Kuhn's theory of scientific change would be disconfirmed by the failure to observe the periodization the theory is usually associated with. Normal science and crisis are "ways of *doing* science" rather than consecutive stages mediated by the emergence of anomalies.[406]

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[402] Rouse (2003), p. 112. Emphasis in original.

[403] Kuhn (1996), p. 150.

[404] "Practicing in different worlds, the two groups of scientists see different things when they look from the same point in the same direction" [Kuhn (1996), p. 150].

[405] Rouse (2003), p. 113.

[406] Rouse (2003), p. 113. Emphasis in original.

This pragmatic interpretation of the paradigm concept suggests the possibility of a sort of "practical" incommensurability between paradigms, where the incommensurability consists of a clash between incoherent "fields of possibilities" for action. Applied to the Instrumental Revolution, it might go something like this: Though practitioners after the revolution could have continued to do chemical analysis the old way, the field of possibilities offered by the pre-revolutionary paradigm was very different from the post-revolutionary one. Here are some examples:

- Focus on analysis rather than synthesis
- microstructure was de-emphasized
- division of the field by substance class (sugar chemistry, terpene chemistry, alkaloid chemistry, etc.) rather than by the type of pursuit (methods oriented versus target oriented; synthesis of natural products versus designed molecules; synthesis of biologically interesting molecules versus theoretically interesting molecules, etc.)[407]
- training in pure chemistry rather than in physical methods and theory
- an insular approach to doing science rather than an interdisciplinary one
- a focus on pure chemistry rather than applications (e.g., chemical biology, nanoscience, materials, energy, etc.)
- traditional lab skills rather than machine operation skills
- a reduced role for instrument-makers and "service disciplines" (e.g., methods developers)
- weaker ties with capitalist and state actors
- much smaller capital requirements

In light of these differences, a Kuhnian might argue that the pre- and post-revolutionary ways of doing chemistry were incoherent in a practical sense, for to do it one way would make it difficult to do it the other way. For example, to make classical structure determination a major part of the research program would leave little time for synthesis, while conversely the use of physical methods to determine structure renders chemical

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[407] On the division according to type of pursuit, see Nicolaou & Sorensen (1996). On the division by substance class, see Woodward (1956), p. 157.

degradation largely superfluous and even wasteful. Structures solved using the new methods could not be directly compared to those solved using the old because the data used for the former would be almost completely different from the data used for the latter. In addition, some kinds of inquiry, such as conformational studies, were simply impossible with the older methods. Ultimately, as was hinted above, the choice between the two paradigms might come down to values: though for the practitioners of one paradigm what the practitioners of the competitor are doing is intellectually comprehensible, it does not make much sense from the point of view of the former paradigm's cognitive aims, efficiency norms, or professional self-conception.

In short, there are grounds for thinking this episode could be made to fit into Kuhn's theory *modulo* some adjustments to the standard interpretation of the latter. On the other hand, it is not clear that doing so would explain much. Novelty is the driving force of paradigm change for Kuhn. But given that the successful application of a paradigm does not generate novelty, the latter must occur "inadvertently," through failures in the application. Though Kuhn focused on the discovery of anomalies during the elaboration of the paradigm,[408] the more general point is that the mechanism of change is *internal* to normal science. But even if, as I have been suggesting in this section, a paradigm can be conceived as an example of a successful way of organizing and conducting scientific labor, there are no resources within the theory for explaining how a new way of organizing scientific labor could come to replace it unless there is some breakdown in the application of the paradigm. Granted that challenges to theory need not be the only drivers of change, the paradigm still has to be challenged internally in some way.

The root of the problem is in Kuhn's insistence on the "unparalleled" insulation of a particular scientific community from external influences.[409] It follows that a revolution within a particular science will be relatively insulated in its origins and in its effects from events and processes occurring outside it, unfolding according to the internal mechanism of normal science and crisis. As far as labor is concerned, Kuhn's scattered remarks on instrumentation point to what Peacock (2009) calls a "path-dependence thesis in the

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[408] Kuhn (1996), p. 52ff.

[409] Kuhn (1996), pp. 164-165.

production of scientific knowledge."[410] The costs of retooling, which have been described above for the case of chemistry, reinforce the conservative nature of normal science:

> So long as the tools a paradigm supplies continue to prove capable of solving the problems it defines, science moves fastest and penetrates most deeply through confident employment of those tools. The reason is clear. As in manufacturing so in science—retooling is an extravagance to be reserved for the occasion that demands it. The significance of crises is the indication they provide that an occasion for retooling has arrived.[411]

But, I have been urging, the Instrumental Revolution cannot be adequately understood from a purely internal perspective.

6.2 Laudan's reticulational model of scientific change

The philosopher Maurice Mandelbaum remarked that a basic difficulty with Kuhn's account is that he is primarily interested in a contrast between the "before" and "after" of a scientific revolution, not in the details of the actual processes of change. According to Mandelbaum, this focus led Kuhn to overemphasize the internal sources of change at the expense of possible external causes.[412] Moreover, it might account for Kuhn's holism, in which theories, methods and cognitive aims are replaced all at once—hence the "conversion" metaphor Kuhn employs to describe how an individual scientist switches paradigm.[413]

Laudan's reticulational model was motivated by a desire to combat the irrational view of science that seems to be implied by Kuhn's holism. Since theories, methods and aims change all at once, there appears to be no common standard between competing paradigms by which the overthrow of one paradigm by another could be assessed. If it is

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[410] Peacock (2009), p. 109.

[411] Kuhn (1996), p. 76.

[412] Mandelbaum (1984), chapter 9.

[413] Kuhn (1996), chapter X.

necessary to have such a standard in order to determine which paradigm is superior, then in its absence the decision to choose one paradigm over another is unjustified and therefore irrational.

Laudan's model proposes that scientific change occurs through a process of gradual, piecemeal adjustment. The process consists of a dynamic equilibrium between three elements: theories, methods and cognitive aims. As tensions arise between the elements, adjustments can be made to any of them in order to resolve the tensions; no one element is taken as foundational. Theories can be modified to accommodate cognitive aims and vice-versa, as can theories and methods or cognitive aims and methods.

Laudan represented his model diagrammatically as in Figure 6.1:

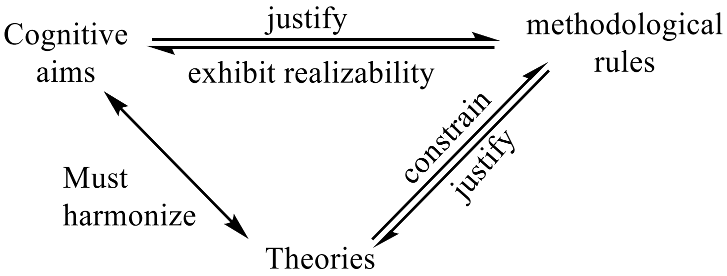

Figure 6.1 Laudan's reticulational model. Adapted from Laudan (1984), p. 63.

The arrows in the diagram indicate the justificatory structure of scientific practice. The use of a particular method is justified by the practitioners' aims. Conversely, the methods employed exhibit the realizability of the aims. The latter point is important, for it shows that the aims are not "utopian," i.e. that they are not such that "we do not have the foggiest notion of how to take any actions or adopt any strategies which would be apt to bring about the realization of the goal state in question."[414] To adopt such an aim would be irrational, according to Laudan, and so methods play an important role in justifying the reasonableness of scientific aims.

Similarly, the theories scientists accept put rational constraints on their methods. If an accepted theory invokes unobservables, then the methods used to support the theory

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[414] Laudan (1984), p. 51.

should be capable of providing evidence for unobservable entities. Conversely, in providing the appropriate kind of evidence, the methods justify the theories.

The reason cognitive aims and theories must harmonize is that the aims Laudan has in mind are ones that specify attributes that the theories should possess. So, for example, the 17th century mechanist ideal that all explanation should be based on contact action would be a cognitive aim on Laudan's view. Harmony between aims and theories is not automatic, however, for conflicts sometimes arise between the explicit values of the community and the values implied by the theories the community accepts. According to Laudan, for example, 18th century scientists explicitly proscribed explanations involving unobservable entities. But this aim came into conflict with some of the most successful theories of the time, which did invoke unobservables. Scientists in the 19th century eventually adopted the method of hypothesis and abandoned Newtonian inductivism, which excluded the possibility of using observation to establish scientific knowledge of unobservables.[415] Though the inductivist principle was widely held to be responsible for the successes of Newtonianism, the only successful theories of well-known electrical, chemical, gravitational and other sorts of observed phenomena in the 18th and 19th centuries posited the existence of an unobservable aether and thus violated the principle. In the face of the success of theories that violated inductivism, scientists eased the tension by adopting a different methodological principle.

Laudan's gradualist approach addresses the rationality problem posed by Kuhn's holism. Since at any given moment, changes in one of the elements will be accompanied by continuity in the others, the elements of continuity provide a standard against which changes in the others can be appraised and justified.

Of particular relevance to *progress* is the adjustment of cognitive aims to new methods or theories. Since progress is a goal-relative notion, the fact that Laudan's model allows goals to shift complicates the assessment of progress. His solution is to drop the assumption, underlying many discussions of progress, that judgements of progress must be relative to the goals of the agents who performed an action. Instead, judgments of progress can be made relative to the observer's view about the goals of science since, in any case,

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[415] Laudan (1984), pp. 56-59; (1981), pp. 111-141.

historians and philosophers of science are typically interested in scientific progress precisely because they value certain cognitive aims that science might further. [416]

I will add that from a labor-process perspective, the fact that the model allows goals to shift is a strength, for descriptive purposes, because it shows how means and ends reciprocally determine each other. It is usually assumed that ends determine the means of their fulfillment, but in reality the opposite is just as common, that ends are determined by the means available. This point was discussed above in section 2.3.2.3, in the context of Freudenthal and McLaughlin's rehabilitation of the Hessen-Grossman thesis. Scientists are not free to impose ends arbitrarily on the given means; the latter affect the formulation and pursuit of ends as well. As I will show below, this point is illustrated by the case of the Instrumental Revolution.

One caveat to keep in mind when applying Laudan's model to a concrete case is that the model's components are very abstract. The cognitive aims are not aims specific to the content of the field, but concern logical features scientific theorizing should aim at, such as whether theorizing should be realist or instrumentalist, reductionist or non-reductionist, aim at simplicity, allow teleological explanation or restrict itself to efficient causation, permit or proscribe action at a distance, aim at certainty or only probability, and so forth.[417] The "methods" of the model are also intended in an abstract sense. Besides inductivism (in the sense of a proscription on inferring nonobservables from observation) and the method of hypotheses, one could also mention various forms of inductive generalization (enumerative, eliminative, demonstrative), inference to the best explanation, falsificationism, analogical inference, etc. Laudan's "methods" are perhaps more perspicuously called "methodological rules," and that is the term he uses in the text. These are rules of theory acceptance, which state in very general terms what the relationship between theory and evidence has to be for acceptance of the theory to be warranted.[418]

The abstraction of his model can make historical analysis difficult because these logical considerations are not always what is (apparently) at issue in specific scientific

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[416] Laudan (1984), pp. 64-66.

[417] Laudan (1984), p. 42.

[418] Laudan (1984), pp. 33-34.

debates and decision-making, which are often primarily concerned with questions of content.

In the case of the Instrumental Revolution, there was undoubtedly adjustment at the level of theory. Chemical structure theory was modified to incorporate the quantum chemical theory of bonding and physical organic concepts like electronegativity and mechanisms. As Reinhardt (2006) shows, the physical theories underlying the new instrumentation also had to be modified to include chemical concepts so that the data could be interpreted in a chemically useful way. Nevertheless, there were elements of continuity.

First, despite being a pre-quantum theory, the chemical structure theory developed by Kekulé, Cannizzaro, van 't Hoff and others in the second half of the 19th century was retained. Both pre- and post-revolutionary theorizing about specific molecular structures had as aim the production of a structural representation of a compound, using the same basic set of rules for formulating structures.[419] Given this element of continuity, a Laudanian question to ask here is whether the theoretical changes that did take place were sufficient to create tension with either cognitive aims or methodological rules.

It can be argued that the function of structural representations in classical chemistry was to reproduce the chemical relations of a substance at a theoretical level—the structural representation was used to represent the various transformations that were observed. This was a non-reductionist approach to theorizing in that the representation was not taken to represent a more fundamental entity than the substance itself, nor did it presuppose a more fundamental theory than chemical structure theory.[420] The new quantum chemical concepts

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[419] See Sidgwick (1936) for an eloquent and clear account of the structure theory and its durability by a practicing chemist, as well as Steinhauser (2014) for a more recent description by a historian.

[420] Schummer (2002), p. 196. Not everyone agrees that the reductionist approach was new; Needham (2004) locates its beginnings at the turn of the 20th century, when atomism began to be used in chemical explanations. Hendry (2010) is also reluctant to accept a non-reductionist reading of pre-quantum chemical theorizing. For my part, I would distinguish between the function of *identification* and the function of representing chemical reactions. Schummer's own criterion of identity for chemical species, which I adopted at the beginning of section 5.2.1, does not logically require any reference to other chemical species besides the one being identified. All that is required is a one-to-one relationship between the set of properties used to identify the species and the species itself, for example between the structure and the compound. The fact that structure was *established* by

were in tension with this approach, since their use seemed to imply two forms of reduction: (i) of substances to molecular species and (ii) of chemical theory to quantum mechanics. Chemists responded to the tension by dropping the aim of non-reductionism. Henceforth, the representation would be construed as that of a molecular species, even though after as before chemists were still largely concerned with classifying and manipulating chemical substances. On this reading of what happened, the function of structural representation in modern chemistry is to represent the microstructure of the compound, which microstructure is understood in terms of a quantum theory of bonding.

In Laudan's terms, we can say that the introduction of quantum chemical concepts created tension with the cognitive aim of non-reductionism. The tension was eased by abandoning the aim.

It is worth noting how, in order to fit the episode into Laudan's categories, we have had to ignore the role of instruments in bringing about the change. The triad of theory, cognitive aim, and methodological rule does not make it obvious how instrumentation fits into the picture. By way of contrast, the philosopher Joachim Schummer (2002) and the historian Leonard Slater (2002) argue that the new instruments played a crucial role in the switch to a reductionist approach to structural representation.

For example, I do not think the question of reductionism was what animated the majority of organic and analytical chemists whose work was transformed during the Instrumental Revolution. Their decisions were more pragmatically inspired than philosophical. An alternative Laudanian interpretation that takes this pragmatic spirit into account is to view the episode as driven by methodological norms rather than by cognitive aims. These norms had to do with the nature of their methods, understood in the sense of

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examining the chemical reactions of the compound does not entail that it amounted only to a representation of those reactions. The structure was an (essential) property of the compound, not a representation of the latter's reactivity. It is also difficult to see how chemists could have coherently applied spectroscopic methods to structure determination, given that those methods presuppose that structure is an intrinsic property of a substance. Coherence was important, since at the beginning of the Instrumental Revolution, the new methods could only be used in combination with the classical ones to determine structure. All that said, I am here entertaining an instrumentalist interpretation of structure in order to make a point about Laudan's reticulational model.

techniques. A plausible interpretation of the episode is that the following norms for the assessment of methods informed the direction of methods development during this period:

a. X is a better method than Y if X requires less compound than Y
b. X is a better method than Y if X requires less time than Y
c. X is a better method than Y if X is more reliable than Y.
d. X is a better method than Y if X is more informative than Y.
e. {X, Y, Z etc.} is a better set of methods than {A, B, C etc.} if {X, Y, Z etc.} provides a greater variety of evidence than {A, B, C etc.}

Together, these norms encouraged the development of methods in the direction of decreasing compound and time requirements, greater reliability, informativity and variety of evidence. It was claimed in chapter 4, section 4.4.2 under Roman numeral I that classical methods were deficient with regard to these norms. On this view, the norms and the techniques employed were in tension with each other. The tension was relaxed by adopting the new techniques. Though Laudan does not include "techniques" in his model, this interpretation is at least Laudanian in spirit.

That said, it is worth noting that my "methodological norms" are quite different from Laudan's methodological rules. The methodological norms affected judgments of the efficiency of compound identification, not choice of theory. Unlike Laudan's methodological rules, they concern the heuristics of inquiry rather than the validation of its results. Scientific debates over technique, as opposed to theory choice, often concern heuristics rather than validation. What is at issue in such debates is not necessarily the goals to be pursued, or whether the rules of theory acceptance are consistent with them, but what is the best way of achieving the goals. Abstract cognitive aims and rules of theory acceptance can usually be implemented, in practice, in more than one way; they do not uniquely determine their concrete implementation. As a result, debates will occur as scientists sort out how to implement them.

Moreover, the tension between methodological norms and the techniques employed is a driver of scientific change that is absent in Laudan's model. As noted above, in the latter cognitive aims plays a key role in driving scientific change, due to tensions that arise between the explicit aims of the community and the aims implicit in the theories or methodological rules accepted by it. One question this absence raises is how to locate methodological norms and techniques with respect to Laudan's triad. I will answer this question at the end of this section.

The new methods, besides satisfying these norms better, allowed for faster self-improvement and more secure inferences, as discussed in chapter 5. From this perspective, the episode looks progressive. On the negative side, however, the new methods did not satisfy the goal of acquiring knowledge of the chemical relations of substances. This kind of knowledge was important in its own right and not just as evidence for theoretical claims. In terms of the four-pronged classification of knowledge discussed in chapter 2, it was a kind of empirical knowledge (EK) that had practical applications (PK), for example in synthesis. The change of methods was not progressive relative to that goal. But it wasn't regressive either, since the old results were not fundamentally affected by the change of methods. On the other hand, this last claim might have to be qualified if one were to take into account the "opportunity cost" of the knowledge not acquired because the old methods were abandoned.

Grounds for an additional negative assessment may be found in Slack (1972), where it is argued that the focus on synthesis in contemporary organic chemistry reflects the industrial need for fast, high-yielding reactions. This focus amounted to an abandonment of the consecrated goal of creating a "science of carbon compounds" that would classify and make intelligible the immense variety of carbon compounds in the world. Slack finds evidence for the abandonment of this goal in textbook presentations of the subject: whereas earlier textbooks structured chemical facts according to the known classes of chemical substances or to basic reaction types, he claims that recent textbooks focus on fast, high-yielding reactions, thus emphasizing what is profitable for industry at the cost of comprehensiveness and understanding. If true, Slack's claims suggest the possibility that there might have been other, scientifically fruitful ways of structuring chemical knowledge that were not pursued for economic reasons, and in fact that the transformation of chemistry might have been regressive relative to the pursuit of more "intellectual" goals like a science of carbon compounds.

In short, there are grounds for thinking that some goals were retained, new ones adopted and old ones abandoned. Was the overall result progress? The answer will depend on how the goals should be weighted and how opportunity costs should be factored in.

Perhaps a compelling weighting scheme can be supplied, though I suspect it is difficult. In any case, it is beyond the scope of this chapter to supply one.

The point about more secure inferences is also related to Laudan's methodological rules. In section 5.5.2, I distinguished between the structure-to-structure mode of inference characteristic of the classical methods and the data-to-structure mode of inference characteristic of the modern methods. The distinction is based on a difference in the content of the techniques, namely whether hypothesized structures give rise directly to the data or via the structures they are produced from or transformed into. The content of the data production process is essential here. But the nature of that process depends on the means available for manipulating compounds.

Are these modes of inference methodological rules, in Laudan's sense? If these modes of inference were rules of theory acceptance, they would have been used to accept theories. But they were not. As shown in chapters 4 and 5, they were instead used to generate hypotheses, by providing a pattern of reasoning for data interpretation. Hypotheses generated by means of these patterns were then assessed using recognizably canonical rules of hypothesis acceptance, for example IBE or inference to the simplest hypothesis. Thus, I think it is more accurate to call these modes of inference "generative patterns" rather than rules of theory acceptance, because they were used to generate structural hypotheses from the data, not to assess the hypotheses thus generated.

The nature of these generative patterns is highly dependent on the nature of the techniques employed because data interpretation has to take into account the process of data production. As argued in chapter 5, the new generative patterns that accompanied the new techniques of the Instrumental Revolution were a crucial part of the impact of that revolution on chemistry. Since these patterns are "methodological" but do not fall under Laudan's category of a methodological rule, it follows that in order to understand what was at stake in the Instrumental Revolution we must broaden our understanding of methodology to include more concrete forms of scientific reasoning based on changes of technique and instrumentation.

This brings up two further points. First, the structure-to-structure mode seems counterproductive to the aim of structural representation, since the material process on which it is based involves the destruction of the very structure to be represented. So the switch to the data-to-structure mode looks like progress. But note that there is only progress here for the reductionist—the non-reductionist would see the “destruction” as an essential part of what is to be represented. Again, the goal-relativity of progress leads to conflicting normative assessments of the same phenomenon.

More importantly, the role of the available means in determining the data production process suggests a way in which Laudan’s model might be augmented:

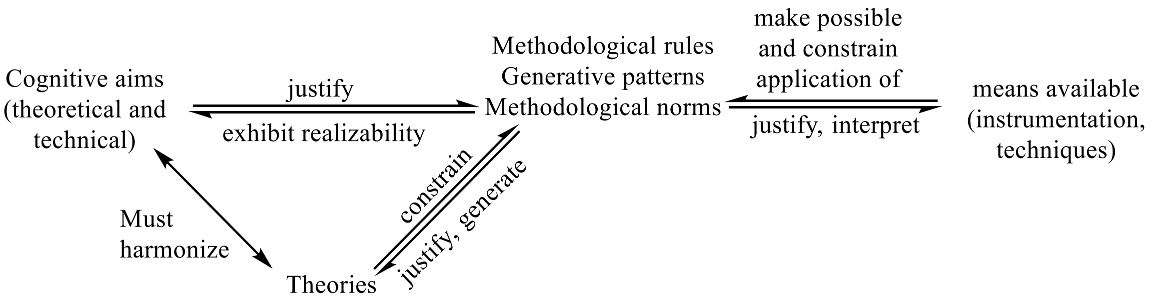

Figure 6.2 An augmented reticulational model.

I have added an extra node to the original model, the “means available,” which includes instrumentation and techniques. Methodological rules justify the employment of certain means, and the latter make possible, realize and constrain the application of the methodological rules. I have also augmented the ‘methodological rules’ node in two ways. First, I have added ‘generative patterns’ because they play a different, but complementary role to the rules. They are used to generate theories and to interpret the data produced by means of instrumentation and techniques. Second, I have included the ‘methodological norms’ discussed above, which are necessary for the comparative assessment of the heuristic properties of techniques. I will have more to say about this augmentation in the conclusion of this chapter.

It is worth noting that the addition of methodological norms implies an augmentation of the cognitive aims. For Laudan, cognitive aims specified attributes that theories should possess. But methodological norms imply goals for the development of techniques (*vide supra*). The application of such norms can impart a direction to technical development, and exhibit the realizability of the corresponding aims.[421] Thus the category of a ‘cognitive aim’ has to be broadened to include aims that affect technical, and not just theoretical, development.

Here is how this model might play out in the chemical case. Classical chemistry had the cognitive aim of constructing structural representations of substances’ chemical relations to other substances. This aim justified the generative pattern of proposing structures based on chemical reaction data. It also justified the application of rules of theory acceptance (hypothetico-deductivism, IBE, etc.)[412] to the proposed structures. Known structures were produced by means of wet-chemical instrumentation and techniques. During the Instrumental Revolution, the material practices of chemistry were changed in the manner described in chapter 4. These changes dictated different generative patterns, which involved inferring structures directly from spectroscopic data. But the justification of these patterns required a different cognitive aim, that of representing microstructures. The adoption of the new aim made a difference to the representations themselves, which came to incorporate features that could only be inferred from microstructural properties, for example conformational isomers or secondary and tertiary structure in biological macromolecules.

If we look at Figure 6.2, we see that, on this reconstruction, changes in the “means available” node—in instrumentation and technique—forced revision in the “cognitive aims” node of Laudan’s original model

Alternatively, one might reconstruct the episode in terms of the methodological norms discussed above. Chemists select techniques in accordance with certain methodological norms and the aims corresponding to them. The Instrumental Revolution made a new set of techniques available that satisfied the norms better than the old. In order to adopt the new techniques, however, chemists had to make certain adjustments in their

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[421] A recent review of advances in geochemistry claims that “many of the discoveries made in geochemistry over the last 50 yr [*sic*] have been driven by technological advances that have allowed analysis of *smaller* samples, attainment of *better* instrumental precision and accuracy or computational capability, and automation that has provided many *more* data” (italics in original). See Johnson et al. (2013), p. 1.

theories and cognitive aims. As a result, quantum concepts were accepted and a reductionist interpretation of structural representations became standard.

With either way of reconstructing the episode, we see that what scientists do in the cognitive realm of aims, theories and methodological principles depends on the material realm of instrumentation and techniques. This is contrary to what Laudan's model might suggest. Indeed, his tripartite model relegates much of what makes this episode interesting, and important in the history of chemistry, to the background—the changes in techniques, instrumentation and material practices in general. In order to fit the episode to Laudan's categories, we have to conceive of scientific method in abstraction from the heart of the change, the new kinds of instruments. Moreover, the categories leave out drivers of scientific change besides cognitive aims.

In contrast, the attempt to fit the Instrumental Revolution to Laudan's model in this section has required the addition of several new elements to the picture. These are: methodological norms used in the evaluative comparison of techniques; generative patterns of reasoning used to interpret data in terms of structural hypotheses; and the fundamental role of the available means in such revolutions. If we compare Figures 6.2 and 6.1, we see that, *contra* the original model, more elements are involved in exhibiting the realizability of cognitive aims than just rules of theory acceptance. Moreover, since methodological norms are goal-directed, the aims in question include more than attributes of theories but also heuristic aims involving attributes of techniques.

6.3 Lakatos' scientific research programs

The fact discussed in the last section, that there were theoretical adjustments in chemistry without theoretical overthrow, suggests that a different unit of analysis than the individual theory may be appropriate for cases like the Instrumental Revolution. Such a unit of analysis was proposed by Imre Lakatos. According to Lakatos, a 'scientific research program' consists of a central core of axioms and principles and an evolving collection of auxiliary hypotheses adopted in the course of applying the core. The central core is taken to be inviolable by those working within the research program, whereas the auxiliaries form a 'protective belt' that can be modified in the light of negative evidence. The development of the auxiliaries is guided by a 'positive heuristic,' a methodological recommendation that directs investigators to formulate auxiliaries that expand the range of application of the core principles.

A theory, on this view, is a combination of core principles and protective belt. Thus each significant modification of the latter produces a new theory related to the previous one via the central core. As the positive heuristic is applied, theories will succeed each other. The series of theories constitutes the research program.

Lakatos cites post-Newtonian planetary astronomy as an historically important scientific research program. The core consisted of the three laws of motion and the law of universal gravitation. Discrepancies between calculations and observations were removed by making changes in the protective belt. A famous example of this is the discovery of Neptune. When the motion of Uranus was observed to deviate from the orbit required by theory, another hypothesis was added to the protective belt. This hypothesis posited the existence of a trans-Uranic planet, and the hypothesis was subsequently confirmed.

Lakatos maintained that the replacement of theory T_n by theory T_{n+1} within a research program is justified provided that (i) T_{n+1} accounts for the previous successes of T_n; (ii) T_{n+1} has greater empirical content than T_n; and (iii) some of the excess content of T_{n+1} has been corroborated. If these conditions are met, then the replacement of T_n by T_{n+1} counts as progress.

Theory replacement is not the only kind of transition recognized by Lakatos's theory. There is also the phenomenon of transition between research programs. As with theory replacement, the successor program has corroborated excess content. On the other hand, the successor theory accounts for most, but not all, of its predecessor's successes. An example of incomplete explanatory overlap is the transition from Descartes' vortex program to Newton's gravitational attraction program. The latter achieved corroborated excess content over the former, because it accounted (approximately) for Kepler's laws of planetary motion whereas the vortex theory was inconsistent with these laws. On the other hand, the vortex theory could explain why all the planets travel in the same direction around the sun. An invisible whirlpool of aether drove the planets in the same direction. There was no corresponding explanation of the unidirectional motion in Newton's original program.

Lakatos maintained that competing research programs should be appraised with respect to their relative rates of progress. If one program is stagnant, having failed to generate new confirmed consequences over a period of time during which a second program has been fertile, then the second program is superior to the first. In cases where both programs have been progressive, the relative importance of the achievements should be assessed.

For our purposes, one question the theory of scientific research programs raises is whether the Instrumental Revolution represents a transition between theories in a research program or a transition between research programs. From the point of view of theory, the episode involved the addition of important new theoretical elements, in the form of quantum chemical and physical organic concepts, with retention of chemical structure theory. Without a doubt, the latter belongs to the core, and so, I think, do the additions. But does this transformation of the core amount to a transition between research programs?

I do not think so. The fact is that classical chemical structure theory had no theory of bonding; it was essentially a set of rules, underpinned by the barest assumptions about how molecules are structured, for associating a structure with every compound that could be isolated.[422] There was thus an empty "slot," so to speak, that was readily filled by the 20th century concepts. From the theoretical perspective, then, the case looks more like an *enrichment* of the research program rather than its overthrow. Moreover, the enrichment was clearly progressive in Lakatos' sense, for it allowed many new predictions to be made and confirmed.

Stating the question of transition solely in terms of theory, however, is not appropriate, since the transition also, of course, involved new instruments and methods. I will therefore take the liberty of replacing 'theory' with 'method', where the latter is understood in the broad sense of chapter 5, section 5.1 that includes theory,

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[422] Sidgwick (1936), p. 533.

instrumentation, experimental methods, and data analysis techniques. The Instrumental Revolution consisted in the replacement of one set of methods (so understood) by another.

From this perspective, the case looks somewhat different than from a purely theory-focused perspective. The new analytical methods did not yield information about the chemistry of substances. There was thus little overlap between the two sets of methods in terms of the empirical knowledge (see chapter 2) that they yielded.[423] From this perspective, the transition looks more like one *between* research programs than an enrichment. If so, was the transition progressive? Based on the criterion of relative rates of progress, and on what was said in chapter 5 about the new dynamism and efficiency of analytical work, the answer seems to be "yes." Even if the correct answer is more ambiguous, as the considerations in the last section suggest, there does not seem to be a problem with assessing the transition using Lakatos' metholodogy.

Prima facie, then, the Instrumental Revolution seems to fit with a normative assessment based on Lakatos' methodology. On the other hand, we have had to modify the usual content of a 'research program' in order to account for the case. If we grant that the episode was primarily about the adoption of a new set of methods, what might be a plausible positive heuristic for the reconstructed new research program? One of Lakatos's examples of a positive heuristic is Newtonian: Lakatos formulates the positive heuristic guiding Newton's program as "the planets are essentially gravitating spinning-tops of roughly spherical shape." Lakatos argues that this heuristic guided Newton's responses to various anomalies he encountered in developing his program in celestial mechanics.[424] Another example is Bohr's development of the theory of the hydrogen atom. Here, the positive heuristic was that atoms are analogous to planetary systems and that the theory should be developed accordingly.[425]

These heuristics are theoretical in the sense that they are ontological assumptions about the system under study. Such a heuristic is inadequate for episodes like the

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[423] I write "little" because mass spectrometry did yield information about chemical reactions, albeit of a very different sort than traditional solution-phase reactions, and other techniques, like NMR, make use of chemical knowledge for data interpretation.

[424] Lakatos (1970), pp. 136-137.

[425] Lakatos (1970), pp. 146-147.

Instrumental Revolution, where the transformation of material practices is a key feature. On the other hand, we can find a positive heuristic readily in the analogy with the Industrial Revolution (chapter 4): the principle of machine production of data, which recommends the emancipation of data production from humans' natural epistemic abilities. As documented earlier, chemists and instrument-makers put this positive heuristic to considerable use in the mid-20th century. Such a heuristic directs the development of methods, and so is not a method itself. It also entails a certain kind of organization of labor, one centered around machine production. To distinguish it from Lakatos' 'positive heuristic,' I will call it a 'labor-principle,' in keeping with section 4.8.

We can illustrate this new kind of heuristic by augmenting the augmented reticulational model of Figure 6.2:

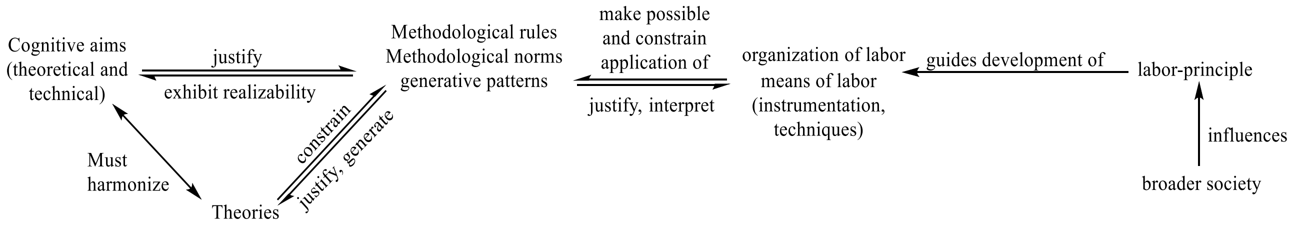

Figure 6.3 An augmented reticulational model.

In addition to adding the labor-principle at right, I have also included the organization of labor along with the means available. I have also indicated the influence of the broader society on the selection of labor-principles, as discussed in section 4.6.

In conclusion, this reading of the Instrumental Revolution in terms of Lakatos' categories shows that there is fit with respect to form, but not content. The episode can be construed as a transition between research programs, but the content involves a transition between methods rather than a theoretical core. A positive heuristic can be imputed to explain the change, but it concerns material practices rather than ontology. As with Laudan, so with Lakatos: the problem is not so much that cases like the Instrumental Revolution cannot be made fit their models, but that we have to look beyond the theory-centric categories they employ to make the fit descriptively adequate, in the sense of capturing the distinctive content of such episodes.

6.4 The Instrumental Revolution as the outcome of a selection process

Laudan's and Lakatos' theories are both instances of gradualist theories of scientific change and progress. Selectionist theories are another kind of gradualist theory. Moreover, selectionist theories try to come to grips with two remarkable features of science: its progress and its stochasticity. An influential version of such a theory is David Hull's attempt to show that natural selection and conceptual change are special cases of a general theory of selection processes.[426] The general theory specifies a mechanism by which entities pass on structure over time. The mechanism involves an interrelation among 'interactors' and 'replicators.' Interactors compete with one another in response to environmental pressures. The resultant competitive differential adaption of interactors causes differential success rates among replicators.

Replicators are entities that give rise to copies of themselves. In the natural world, the entities consist of genetic material. Interactors are entities subject to competition within some specific environment. In the natural world, the interactors include living organisms, cells and kinship groups. Within the history of science, the replicators are concepts and beliefs, where the latter include not just theoretical propositions but also commitments to methodological principles and standards of appraisal. The interactors are individual scientists and individual research groups.

Hull claimed that the history of science is the result of selective pressure operating on a set of variants, namely variants among the replicators. As scientific concepts and beliefs change over time, so do the theories constituted by them. In a given discipline, the succession of theories constitutes a lineage. The relation between theories is determined by whether or not they are phylogenetically related, not by whether they share a common structure or properties.

Hull noted that the phylogenetic understanding of the history of theories has for consequence that "unappreciated precursors do not count."[427] On this view, Patrick Matthew's unnoticed formulation of the principle of natural selection in 1831 is not part of

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[426] Hull (1988).

[427] Hull (1989), p. 233.

the lineage of natural-selection theory. According to Hull, a phylogenetic reconstruction of the history of Darwinian evolutionary theory reveals a tree of descent whose branches include "Darwin's Darwinism, late nineteenth-century Darwinism, neo-Darwinian Darwinism, the new synthesis Darwinism, and so on."[428]

On Hull's view, the dominant interactions between scientists consist of cooperation and competition for credit. Selection in the form of citations is therefore an important part of the mechanism by which scientific ideas are replicated. Conceptual replication is a matter of information being transmitted by means of different vehicles, for example books or journals. Scientists are also vehicles, but because they can test and change the transmitted ideas they cause replication to be differential, which makes scientific change possible.

What causes some lineages of ideas to cease and others to continue? First, scientists tend to behave in ways that increase their conceptual fitness. The competition for credit is important here, for scientists want their work to be accepted, which requires that they gain support from other scientists. Scientists whose support is worth having are likely to be cited more frequently. Second, the competition for credit also engenders cooperation. Scientists tend to organize into tightly knit research groups in order to develop and disseminate a particular set of views. Due to the individual scientist's inherently limited abilities and knowledge, scientists tend to form research groups in order to solve the problems they confront. Cooperating scientists often share ideas that are identical in descent, and transmission of their contributions can be viewed as similar to kin selection. Individual research groups may compose a community. Scientists tend to use the ideas of scientists within their community much more frequently than those of scientists outside the community.

The first criticisms and evaluation of an idea come from within the research group, notably in the form of testing. Once published, the idea may be subject to further criticism by scientists outside the group, because some outsiders will have different perspectives on the problem and different career interests. Science acquires its self-correcting character through this external criticism.

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[428] Hull (1989), pp. 234-7.

I have already discussed Carsten Reinhardt evolutionary interpretation of the Instrumental Revolution in section 5.1. As mentioned there, the competitive pressures were twofold: there was the pressure on instrument manufacturers to survive on the market, and there was the pressure on the scientists to maintain and accumulate credit in academia. In Hull's terms, both pressures were handled by means of cooperation between the two interactors, in addition to the usual cooperation within the respective firms and research groups. The "replicators" in this case also appear to be twofold, consisting of the instruments produced by the manufacturers and the methods (in the broad sense I have been using) developed by the scientists. The evolutionary story could be taken even further than Reinhardt does, for once the methods and instruments became normative, each ordinary chemist had strong incentives to use them. A chemist who continued to do chemical analyses the old way would quickly lose credit in the face of competitors who could solve the same problems using the faster, more informative and (I have argued) more secure techniques.

A point of difference that emerges here between Reinhardt's account and Hull's theory is that the sets of interactors and replicators have to be expanded to explain such changes in material practices. Hull's sets are theory-centric: the replicators are concepts, beliefs, and cognitive commitments, and the interactors are individual scientists and research groups. These sets cannot completely account for the transformation Reinhardt describes; one has to take into account not just the crucial involvement of the manufacturers, but also background social conditions like capitalism, the earlier development of the methods in industry, the funding available for instrument manufacturers and consumers in the context of the Cold War, the level of technological development, etc. Moreover, the mechanisms of competition and cooperation for academic credit are insufficient; one must take into account competitive pressures and cooperative strategies specific to a capitalist economy.

Another point of difference with evolutionary accounts of science like Hull's is that the replicators are no longer just ideas, but also material instruments and the practices based on them. This makes it difficult to ignore the labor process. In the first chapter of this dissertation, as well as in the historical part of this chapter, the labor process has been conceived as a system of functional relations between an agent or agents, an object of labor, and instruments of labor. On this structuralist view of the labor process, it is impossible to make certain kinds of changes to one element of the process—here the instruments—without changing the other elements. There follows from this impossibility a disanalogy with the evolutionary model. The disanalogy is similar to one, discussed in section 5.1 above, that was pointed out by L. J. Cohen in his 1973 critique of Stephen Toulmin's version of the evolutionary theory of science: the evolution of scientific concepts cannot be explained in the same manner as the evolution of a species, on the grounds that in Darwinian-type explanations "within any population … of environmentally threatened individuals, the similarities that are selectively perpetuated are those that are favourable to the continued existence of such individuals."[429] But on the holistic conception of scientific concepts that Cohen endorses, changes within a discipline involve a restructuring of an "evolving" concept's relations to other concepts and not just a replacement of individual concepts.

A similar argument may be made for the labor process. The introduction of the new instruments could not occur without restructuring the whole process. Once chemical reactions were replaced by physical interactions, the functions and activities of the chemist (in chemical analysis) were transformed. Arguably, so was the object of labor, which I suggested in section 4.4 became either light energy or molecular species.[430] The nature of the inferences used to identify compounds, as well as the theories brought to bear, also changed. Rather than a mere replacement of one kind of tool by another, say, glassware by spectrometers, an entirely new approach to analysis was adopted.

Cohen pointed out a further disanalogy between conceptual evolution and biological evolution that I think is also relevant here. The former, unlike the latter, is "coupled" in the sense that there is a connection between the factors responsible for the generation of variants and the factors responsible for the selection of variants. Conceptual evolution lacks the random character of biological mutations: "Conceptual variants are for

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[429] Cohen (1973), p. 48.

[430] Schummer (2002) argues that the concept of chemical species identity used to identify compounds changed during this period, from a concept of pure substance to a concept of molecular species.

the most part purposively thought up in order to solve the intellectual problems that beset a discipline."[431] By contrast, in biological evolution, mutation and selection are "uncoupled" in the sense that the selection of variants is due to different factors than the generation of variants: "The gamete has no clairvoyant capacity to mutate preferentially in directions pre-adapted to the novel ecological demands which the resulting adult organisms are going to encounter at some later time." I have provided evidence that the Instrumental Revolution was purposive. First, there were acknowledged weaknesses of the classical methods. Second, as noted the champions of the instrumental approach had a will to use science and technology not only to solve chemical problems, but also to eliminate the human element where it was seen as problematic or constraining.

Most importantly, from the standpoint of science as labor, is that the analogy with natural selection amounts to a misrecognition of the peculiar character of the evolution engendered by the labor process. This peculiar character has been analyzed by Peter Damerow with a view to developing a materialist historical epistemology. Since this will be one of the main themes of chapter 7, I will not say much about it here. For the time-being suffice it to say that the labor process can result in a surplus, and this fact introduces *a new mechanism of change, over and above the random variation and natural selection of biological evolution*: reproduction by means of the expanded means of production. Each iteration of the labor process differs from the preceding one insofar as it incorporates the surplus generated previously. So the biological analogy at the heart of the evolutionary theory of scientific change fails to the extent that it evacuates the specific characteristics of labor that are responsible for the different trajectories of human and animal history.

How might this mechanism manifest itself in science? My suggestion is that an important way in which instruments contribute to scientific progress is by making possible a *dialectic of discovery and embodiment*. Scientists start from a store of knowledge (in the broad sense described in chapter 2). Using this knowledge, they produce instruments that they then use to discover new things about the world. If successful, new items will be added to the store of knowledge. The augmented store can then be used to build improved instruments, thus renewing the cycle.

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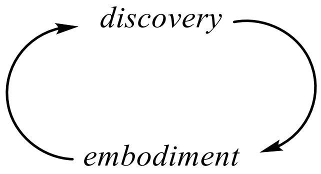
[431] Cohen (1973), p. 47.

Schematically, the process may be presented thus:



$$1)\ k_1, k_2, \ldots k_i \xrightarrow[\text{(a)}]{\text{production of}} \text{instruments} \xrightarrow[\text{(b)}]{\text{discovery}} k_{i+1}, k_{i+2}, \ldots k_n$$

$$2)\ k_1, k_2, \ldots k_n \xrightarrow{\text{repeat (a) and (b)}} k_1, k_2, \ldots k_n, k_{n+1} \ldots k_m, \text{etc.}$$

Figure 6.4 The dialectic of discovery and embodiment.

Here, each k_j is some instance of knowledge. The initial store of knowledge is represented by k_1, k_2, … k_i, the "surpluses of knowledge" by k_{i+1}, … k_n and k_{n+1}, … k_m. An essential feature of this process is that it concerns collections of instruments and discoveries, not individual instruments, since the discoveries made by means of an individual instrument cannot necessarily be used to improve that very instrument (the knowledge of the moon's surface afforded by Galileo's telescope could not be used to improve the telescope itself). The complementary features of extension and improvability are both involved in this scheme. The extension of our observational and computational powers yields new knowledge, which can then be embodied in new instruments by way of their improvability.

Something similar can happen with humans. For example, Hacking (1983) argued that observation is a skill. Knowledge of how to observe is acquired through scientific practice. These skills can be transmitted from master scientist to apprentice, and when this happens the sense perception of the apprentice is augmented by the new skills. Similarly, human computational powers can be improved by the discovery of algorithms like the rules of arithmetic (this would be a case where a discovery made by means of an "instrument"—the mind—could be used to improve the power of the instrument itself). So the native human abilities can engage in this dialectic as well, though not to the same extent as artifacts due to the constraints imposed by humans' natural endowment.

How does the dialectic of discovery and embodiment help us understand the Instrumental Revolution? This was an episode in which knowledge produced in diverse disciplines was combined, following the principle of machine production of data, in the form of new instruments for chemical analysis. Various scientific and social developments—the discovery of quantum phenomena, the emergence of computer science and physical organic chemistry, the needs of the petrochemical and rubber industries during the Second World War, the output-oriented administration of university departments, etc.—converged to make the embodiment of scientific and technological knowledge possible and sought after as a means for solving chemical problems. To be sure, the variation of scientific ideas played an important role—I by no means intend to downplay the stochastic effects of human creativity and of empirical results in this process—but so did the surpluses of different kinds of knowledge and their application to the analytical labor process. Variation of ideas and natural selection are insufficient to explain this episode; the mechanism of change through expanded reproduction must also be taken into account.

By analogy with the schemes of Figure 6.4, the epistemic component of the convergence process might be illustrated, in a highly simplifying way, as follows:

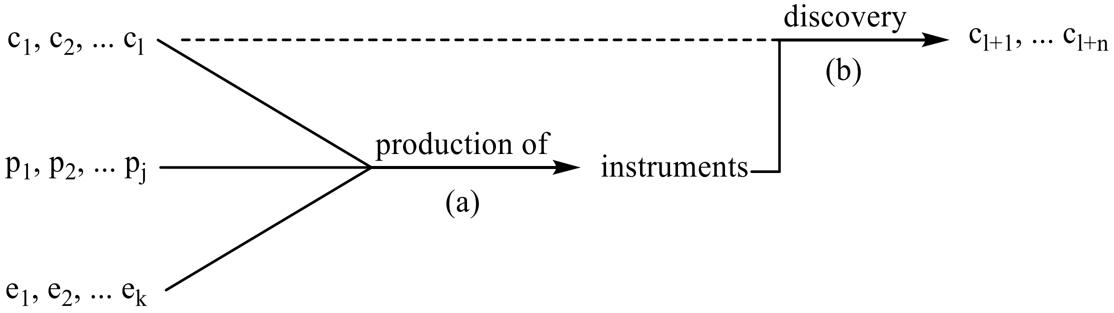


Figure 6.5 The Instrumental Revolution as expanded reproduction through combined development.

The series on the left represent instances of a kind of knowledge: the c_i's chemical knowledge, the p_i's physical knowledge, and the e_i's “engineering knowledge,” a broad notion intended to cover computer science, electrical engineering, various forms of know-how, etc. There is expansion, as chemistry acquires more instruments than it had at the beginning. There is also reproduction, as the new instruments allow chemists to continue doing chemical analysis. In addition, most of the old knowledge was retained, since the new methods were compatible with the old. There is thus accumulation not just of the stock of instruments, but of chemical knowledge.

The diagram also illustrates a mechanism of expansion: the combination of different kinds of knowledge in the production of the new instruments, which would not be possible with a more insular mode of development. This mechanism anticipates two features of mid-20th century chemistry that have been noted in the literature. First, it asserts that chemical knowledge entered into the production of the instruments. This theme is emphasized by Reinhardt, who argues that the "chemicalization" of the instruments protected the disciplinary autonomy of chemistry from physics. Second, it anticipates the phenomenon of "concept amalgamation" identified by Andrea Woody. In Woody (2012), she describes the emergence of conceptual tools from the "amalgamation" of physical and chemical concepts. Her main examples are molecular orbitals and molecular orbital diagrams, both of which are based on quantum mechanics but neither of which can be derived from it, since they incorporate assumptions from chemical structure theory as well. The diagram suggests the possibility of some such amalgamation in the conception of the instruments.

In conclusion, I think the evolutionary approach yields valuable insights into processes of innovation in science. But it runs the risk of reductionism, insofar as it evacuates features of science that are specific to human evolution. In particular, an evolutionary approach must take into account how humans' relationship to nature is mediated. Implicit throughout this chapter is the idea that labor mediates that relationship. Labor makes possible the dialectic of discovery and embodiment. From this perspective, knowledge appears not just as the aim of science, but as a resource for future science. The integration of prior knowledge into future practices has a transformative effect on science, as the Instrumental Revolution illustrates. In the concluding section of this chapter, then, I will propose a further augmented reticulational model that acknowledges this role of prior knowledge in the dynamics of scientific change.

6.5 Conclusion

In an attempt to synthesize the morals of this chapter with respect to scientific change, I offer the following diagram, a further augmented version of the reticulated model provided in Figure 6.3:

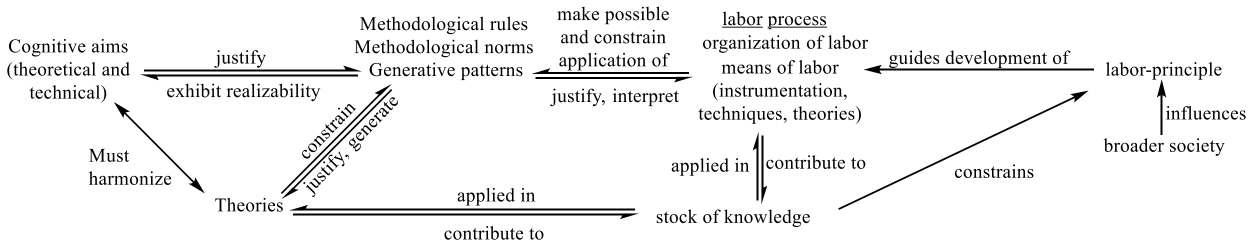


Figure 6.6 A further augmented reticulational model.

The diagram is intended to capture the main positive insights of sections 6.1-6.4. The difference with Figure 6.3 is that the stock of knowledge has been added. The externalist hypothesis of section 4.6 is represented on the right. Laudan's original reticulational model is retained on the left, *modulo* the modifications made in section 6.2 due to the inclusion of methodological norms and generative patterns with the original methodological rules. The process of expanded reproduction is represented by the double arrows connecting the stock of knowledge with theories and the labor process. Here the stock-of-knowledge node serves as both the product of and starting-point for scientific work. More nodes and arrows could be added. For example, the organization of labor and the means of labor are structurally related, in that certain means require certain organizations and vice-versa. The relation to nature is left implicit. I don't want to so complicate the picture that it becomes confusing.

According to the diagram, the labor process makes possible, and constrains, the application of methodological rules. Theories appear twice, first as apex of Laudan's original triangle and second as part of the means of labor. The constraining effect of theories in the original triangle resides in the fact that the content of theories can sometimes lead scientists to abandon methodological rules if the content is in tension with the rules.

In the example sketched earlier, the success of theories positing unobservables like the æther led scientists to abandon Newtonian inductivism. This kind of constraint, however, does not tell us how the theories are produced in the first place. But theories put constraints on how new theoretical claims are produced. For this reason, I include them in the labor process (there is the additional reason, noted in section 5.1, that techniques often have a theoretical component). A simple example is the charge of an electron. The charge, $^{-}1.6 \times 10^{-19}$ C, is not specified by fundamental theory. If it were, it could be treated as a hypothesis, observable consequences deduced, and experiments performed to see if they are observed. Since it is not specified by a theory, however, it has to be inferred from measurements of observable quantities. Moreover, it must be assumed that the charge inferred from any set of measurements holds for all electrons, not just the ones measured. In other words, we have to use enumerative induction, and we use it simply because a deductivist approach is barred by the available theory. This example also shows how the means available, in this case the theory of fundamental particles, constrain the application of methodological rules.

The labor-principle directs the application of the stock of knowledge. It does so by recommending certain applications of the stock in the development and use of techniques and instrumentation. Conversely, only certain labor-principles will make sense given a certain stock of knowledge. In the absence of knowledge of how to automate machinery, for example, it is impossible to automate data production. Likewise, certain organizations of labor make sense in light of prior knowledge. For example, the Standard Model of particle physics predicts that certain fundamental particles will be detectable when certain very high energy collisions take place. Since these collisions produce a massive amount of data, it makes sense to automate the data analysis process, and indeed automation may be the only way to process the data efficiently. But this organization of work around automated processes is justified by the aim of confirming the predictions of theory. This sort of aim is not a cognitive aim in Laudan’s sense, but rather a methodological rule along the lines of “experiments should aim to confirm theory.” A different rule might call for a different organization, a point to which I return below. Thus, though the stock of knowledge constrains the labor-principle and organization of labor, it does not dictate these features of the process; the methodological rules and norms must also be specified.

The diagram also incorporates the four-pronged conception of knowledge discussed in chapter 2. Theory contributes to the stock of knowledge, but so do techniques and instruments. The latter are the source of empirical, practical and methodological knowledge, the former of theoretical knowledge.[432] According to the broad conception of progress discussed in the first chapter, progress is made when either of the two sources contributes to the stock of knowledge.[433]

It is worth noting that if the labor process weren't included as a node, it would be harder to see how the non-theoretical kinds of knowledge make their way into the stock because the only kind of knowledge of which Laudan's model admits is theoretical. This nexus between the labor process and the stock of knowledge is a surprising result of the analyses of the preceding sections of this chapter. It is a commonplace that science builds on what it learns, but the dependence of this building on the transformation of the means of labor in light of what science has learned is less often appreciated, perhaps due to the belief that science has a static "Method" that remains external to the expansion of knowledge. The nexus has important implications for the growth of scientific abilities and the nature of scientific progress. Some of these implications will be explored in chapter 7.

This model suggests a kind of progress, the accumulation of knowledge. This observation raises the possibility that, even if specific cognitive aims change, progress can be made so long as there is an overall accumulation of knowledge. This view of progress seems to be descriptively accurate, if we accept Mizrahi (2013)'s description of how scientists judge their own progress. He discerns the following pattern in the way scientists make such judgments:

1. Survey the body of knowledge *B* in field *F* at time *t* prior to discovery *D*.
2. Estimate what was known (*B*) in *F* at *t*.
3. Identify a lacuna, imprecision or error in *B* at *t*.

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[432] Though there is some overlap, since theories contribute to methodological knowledge as well. Recall that in section 5.1, I described chemistry's new instrumental methods as four-fold combinations of theoretical principles, instruments, experimental methods, and data analysis techniques.

[433] I assume here that new contributions are not made at the cost of old contributions, i.e. that worries about phenomena like Kuhn-loss can be dealt with.

4. Spell out how *D* improved on *B* by adding new knowledge, correcting imprecision or exposing errors and correcting them.[434]

According to this pattern, scientists assess progress relative to features of the stock of knowledge—the lacunae, imprecisions or errors in (3). In many cases they also direct their ongoing research in response to these features as well. Presumably, these features need not all satisfy the same cognitive aim. In my discussion of Laudan's reticulational model, for example, I suggested that chemists' cognitive aims have changed, from a non-reductionist approach to representing chemical substances to a reductionist one. But even if this is correct, this change did not invalidate the earlier chemists' contributions.

The stock of knowledge is the starting-point, and ultimate product, of the process depicted in Figure 6.6. Though this process may look like a hodge-podge of moving parts, this fundamental role of the stock gives it a certain directionality. As starting-point, it imparts a certain path-dependence to science, to the extent that the direction of ongoing research is determined with respect to the state of prior knowledge. This makes the direction of current science dependent on the historical contingencies that occurred during the production and propagation of the prior knowledge.[435] As product, it gives science an overarching goal, the accumulation of knowledge.

Figure 6.6 suggests a synoptic approach to explaining the history of science. It does so by suggesting connections between sets of facts usually considered to be the purview of different disciplines. Facts of interest to philosophers of science, about the relations between cognitive aims, theories and methodological rules, are shown to be connected to facts usually considered to be of interest mainly to historians and sociologists of science.

The particle physics case may again serve as an example of how this works. In the second half of the 20th century, particle physicists chose to confirm predictions of the Standard Model about the detectability of certain fundamental particles. To this end, data analysis was heavily automated, especially at CERN. In the terms of Figure 6.6, that part of the stock of knowledge, consisting of the Standard Model and its predictions, provided a starting-point. Combined with the rule that experiments should confirm theory, it now

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[434] Mizrahi (2013), p. 379.

[435] Peacock (2009).

made sense to automate data analysis. Automation was accompanied by a considerable fragmentation of tasks between theorists, experimentalists and engineers, and between physicists and technicians. The principles of automation and a strict division of labor did not pop *ex nihilo* into the physicists' heads, but were informed by the example of automation and division of labor in industrial production as well as the experience of large-scale applied scientific research during the Second World War.[436] The drive to automate led to the development of fully computerized data analysis techniques and encouraged the development of experimental techniques and instrumentation for the attainment of ever higher energies. The confirmation of the existence of the Higgs boson is probably the best known result to come out of the automated approach.

It turns out that there was in fact significant disagreement among physicists over the degree to which data analysis should be automated. Though CERN was run by the pro-automation camp, LBL, Fermilab and SLAC were run by proponents of a semi-automated regime, which required more direct involvement of the physicist in data analysis. The semi-automated organization of labor involved different relations to the other nodes. Though confirmation was important to these physicists as well, they also thought experiment should aim to discover novel phenomena. As a result, their experiments were less dependent on background theory and more exploratory in nature. The methods of data analysis developed by this camp involved hybrid systems based on human-computer interaction. The experiments themselves tended to be aimed at exploring broad energy domains rather than ever higher energies. The organizational ideal motivating the leaders was that of the physicist as an independent explorer who would handle both the conception and the execution of the experiments. Unexpected phenomena, like the J/psi particle, were discovered as a result.[437] Both Galison (1997) and Perovic (2011) think that the semi-automated approach was more fruitful overall.

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[436] Galison (1997), ch. 5 provides an account of the deliberate "industrialization" of bubble-chamber physics.

[437] For a history of this rivalry, see Galison (1997), ch. 5. Perovic (2011) argues that the organization of labor made a big difference to the results achieved by the competing labs.

However that may be, the upshot of this example for our purposes is that the confrontation of standard theories of scientific change in the previous sections of this chapter with the labor process perspective of chapter 2, over the case of the Instrumental Revolution, suggests an analytical framework for integrating certain kinds of facts pertaining to social structure—both that of science and of the broader society—with facts about the relations between cognitive objects (cognitive aims, theories, methodological rules, etc.) that philosophers have focused on. As the sketch of particle physics history suggests, the integration allows us to broaden the explanation of scientific progress beyond standard epistemic categories. What results is a more complete explanation of how progress is made. Perhaps more importantly, the contrast between the fully automated and semi-automated approaches in the high-energy physics case suggests that this framework allows us to explain why some ways of doing science are more successful than others in cases where descriptions merely in terms of relations between cognitive objects would be explanatorily inadequate.

Another feature of the model represented in Figure 6.6 is that the content of an individual node depends on the content of the other nodes. This is revealed in the case of the two physics camps. They disagreed over whether the primary goal of experiment should be confirmation of background theory or discovery of novelty. As a result, they disagreed over the types of experiment to run, and therefore over the appropriate degree of direct human involvement in data analysis. Conversely, this debate over the goals of experiment presupposed that the means to realize them were available, in the form of high-energy experiments and the technology and organizational know-how necessary to run them. So the analytical framework is not purely formal, but suggests that the content of each node is related to that of the others.

The relational dependence of node content is holistic, but the nodes must not be seen as being in lock-step with each other. I accept Laudan’s idea that the different components of science can change in a relatively autonomous fashion. This relative autonomy can introduce frictions into the system because as individual components change, its relations with the others will not necessarily remain harmonious. What the augmented model does is suggest more points of potential friction arising from the important role of the labor process. Here I will point out four such points.

1. The juncture between the labor process and methodological rules. This juncture can become a point of friction, depending on what rules are supported by the labor process. This was illustrated in the particle physics case. Data processing was automated in order to boost productivity, a change that, viewed in isolation, seems positive. But if the arguments of some physicists and of Perovic (2011) are correct, the automated regime does not support all uses of experiment to the same extent. There is friction between the dominance of the automated regime and the goal of discovery, especially since the experiments to which the automated regime is applied tend to drain resources from discovery-oriented experimentation.
2. The juncture between the labor process and the stock of knowledge. This point of friction was illustrated in the analysis of the Instrumental Revolution. In general, the labor process tends to be biased towards the acquisition of some kinds of knowledge at the expense of others. In the chemical case, the use of the new instrumentation as the main method of chemical analysis was incompatible with the goal of acquiring knowledge of the chemical relations of a substance. For proponents of the old methods like Robert Robinson, the abandonment of analysis by chemical reactions represented the loss of an important contribution to the stock of knowledge.
3. The relationship between theory and practice. In Figure 6.6, this relationship is implied by tracing the arrows from the “Theories” node to the “Labor process” node. Theory and practice don’t always mesh. A practical consequence of the relative autonomy of nodal development is that scientists will sometimes seek to combine theories and practices developed in very different contexts from each other. For example, Woody (2012) argues that chemists in the second quarter of the 20th century were confronted by a “chasm” between the empirical realm of chemistry and the new quantum theory that could, in principle at least, be used to explain the phenomena in that realm. This explanatory task required techniques for organizing and understanding the complexity of chemical phenomena in a way that supported aims like the synthesis and analysis of substances. Classical chemical structure theory was useful for classifying

substances, but could not explain features of the periodic table or chemical bonding. Quantum mechanics seemed promising in that regard. The chasm consisted in the fact that, whereas chemical practice was centered on macroscopic substances, quantum theory based on first principles could not explain anything larger than the hydrogen atom. In order to realize the explanatory potential of quantum theory for chemistry, “amalgamated” concepts that combined classical and quantum ideas as well as qualitative representational practices (molecular orbital diagrams) had to be cobbled together. Only a simplified and semi-classical version of quantum theory was applicable to chemical phenomena. This hybrid was essentially an outcome of the difficulties of meshing the original quantum theory with chemical practice.

4. The juncture between the labor process and labor-principles. This juncture is perhaps especially prone to tension, because scientists tend to see their work as the embodiment of certain values and attitudes. New ways of doing scientific work may seem threatening to these values and attitudes. I will quickly mention a few examples. Baird (2002) shows that analytical chemists went through a professional identity crisis because the automation of analytical work seemed incompatible with the widespread view that analytical chemistry should be about studying chemical reactions, not developing instrumentation or learning physical theory. Bigg (2000) describes the transformation of astronomy at the end of the 19th century with the advent of photographic telescopes. The large amounts of new data encouraged astronomers to adopt a factory-like division of labor, employing semi-skilled workers (largely women) to process it. Some astronomers, like the eminent Karl Schwarzschild, resisted such practices on the grounds that astronomical work could only be done successfully by specialists. Garcìa-Sancho (2012) documents divergences within the molecular biology community on whether and how sequencing work should be automated. According to Garcìa-Sancho, these divergences arose from different values and attitudes towards scientific work. Proponents of a more thorough-going automation tended to view manual work as a waste of time and put special value on productivity and the scientific and commercial opportunities associated with

> development of instrumentation that was relatively autonomous from human involvement. Proponents of greater human involvement tended to have a more positive view of manual work and stressed values of accuracy, human control, cooperation rather than competition, independence from business, the public nature of scientific work and universal access to its results. Finally, Dick (2015) recounts an early attempt by Herbert Simon and Allen Newell to model human reasoning by having a computer prove theorems. In the implementation of the model, Simon and Newell found that they had to deviate from the model in response to material constraints intrinsic to the nature of the machine but foreign to the human experience of logic. Here, human reasoning provided the "labor" principle—the machine was to carry out a cognitive task like a human reasoner—which principle encountered frictions arising from the material characteristics of the machine. Alternately, this example could be viewed as a conflict between theory—the model of human reasoning—and material practice, the implementation of the model in a machine.

These kinds of frictions are both effects and causes of change. They can therefore be sources of innovation. The resistance from the machine encountered by Simon and Newell led them to create a new programming language in order to implement their model, which changed their model as well. In Woody's case, chemists developed new concepts and diagrammatic practices in order to apply quantum mechanics to their field.

I end with a proposal for future research. Though the discussion of the augmented model has been largely focused on descriptive accuracy, it might also have normative implications with respect to Laudan's original model. Though the latter seems descriptively accurate as an account of how certain kinds of scientific change occur, a major criticism is that it is normatively inadequate because it leaves the choice of how to establish equilibrium between the three components underdetermined.[438] The reticulational model requires that a change in one component that disturbs the equilibrium with the other two be accommodated by a compensating change in at least one of the others. But the model does not say how to do this. Laudan required the components to be both consistent and

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[438] Doppelt (1986); Losee (2004).

realizable, a requirement that was supposed to provide a fixed standard for evaluating possible adjustments of the components. But in many evaluative contexts there is more than one possible adjustment that can be made that is consistent and realizable. Furthermore, the realizability constraint can be challenged. For example, exact measurement may be impossible to realize experimentally, given the imprecision inherent in the use of measuring instruments. But it is not irrational to pursue exact measurement in spite of the unrealizability of the aim, because one can at least make progress towards it, say by making the instruments more precise.[439] But if the realizability constraint is discarded, then the only constraint left is consistency. But there may be numerous consistent alternative modifications of theories, methodological rules and cognitive aims.

The reason this underdetermination is a problem is that it seems to pave the way for relativism about scientific change, insofar as it implies that new theories are no more rational than their predecessors to the extent that the latter can be accommodated to meet the consistency and (perhaps) realizability requirements just as well as the former. Assuming this criticism is correct for Laudan's model, a question for *my* model is whether it also implies relativism. Though it is beyond the scope of this chapter to answer that question, I will point out certain features of my model that might be relevant for answering it. The elements I have added are very different sorts of things than Laudan's original three, so it is reasonable to suppose that the differences may be relevant to the question.

Hasok Chang has suggested that "[c]apabilities have much to do with scientific rationality in general, because rational decisions should be based on an accurate sense of the agent's own capacities and skills."[440] Abilities underlie Laudan realizability requirement, for example in his claim that certain goals are utopian:

> When I say that a goal state or value is utopian, I mean that we have no grounds for believing that it can be actualized or "operationalized"; that is, we do not have the foggiest notion how to take any actions or adopt any strategies which would be apt to bring about the realization of

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[439] Though Laudan's requirement that a rational scientific goal must be realizable may be too strong, one could fall back to a weaker requirement that it be *regulative*, in that it guides scientists' actions so that they are able to make progress towards it even though they can never reach it. This weaker requirement may be able to handle cases like the ideal of exact measurement. See Niiniluoto (2015).

[440] Chang (2011), p. 211.

> the goal state in question ... I deny that it is reasonable to hold those goals. Implicit in this assessment is the belief that the rational adoption of a goal or an aim requires the prior specification of grounds for belief that the goal state can possibly be achieved.[441]

Aims are rational, for Laudan, only insofar as one has the ability to make progress towards them. Yet Laudan's model does not explain how scientists have the ability to accomplish their aims, except for the contributions of the methodological rules and theories. And even the latter have to be viewed as part of the means of scientific work, as I have done in Figure 6.6, in order for their contribution to scientific ability to be clear. But this is not how they are viewed in Laudan's model, where they are treated as products to be evaluated for consistency with aims and rules. As Figure 6.6 shows, however, much more is involved in the constitution of a scientific ability than methodological rules and theories.

For example, the stock of knowledge includes knowledge from outside the field of the model (section 6.4). Practitioners within the field are not in a position to modify knowledge borrowed from other fields, except when the knowledge is close to their own (as in Woody's case of concept amalgamation).[442] The borrowed knowledge seems to be off-limits to modification, which suggests an additional constraint on the choices the scientists can make to establish equilibrium. This constraint might reduce the underdetermination.

According to Figure 6.6, the labor process makes possible and constrains the application of the methodological principles. Moreover, the labor process includes things like instrumentation, techniques, object domains (e.g., the pure substances of classical chemistry) and social organizations that are material and also partly subject to extra-scientific evaluative norms (like productiveness or convenience), and so cannot be changed merely by cognitive scientific fiat. The material environment, a laboratory for instance, needs to be produced, requires material support of various kinds (capital, utilities, etc.), and presupposes a certain level of technological development. So change in the labor process depends on factors beyond the purely cognitive ones in the original model. How do these factors constrain scientists' attempts to establish equilibrium? Is the

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[441] Laudan (1984), p. 51.

[442] Trout (1992) provides a discussion of this kind of reliance on external fields, which he calls "mercenary reliance."

underdetermination increased, decreased or the same? One can conceive of situations where scientists decide to adopt one kind of methodological rule rather than another because the instruments available to them make it easier to realize one than the other. To return to the electron example, the inference of the charge itself (prior to the generalization to all electrons) is a form of demonstrative induction, a form of inference in which theoretical quantities are deduced from observations rather than observations from theoretical quantities.[443] As this example illustrates, in certain kinds of research scientists are forced by the theories available to them to adopt this form of inference over the hypothetico-deductive method, in this case because theory does not allow the charge to be deduced *ab initio*.[444]

In short, there are reasons to think that once the material context of scientific work is fleshed out in the manner suggested by my augmented reticulational model, the relativism objection to the original model may have to be reconsidered. On the other hand, the augmented model introduces many more degrees of freedom, so it may actually increase the underdetermination. I leave the resolution of this problem as a question for future research.

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[443] Norton (2005).

[444] For an extensive study of J. J. Thomson's use of demonstrative induction-style inferences in his research resulting in the discovery of the electron, see Smith (2001).

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7.0 DISCOVERY AND INSTRUMENTATION: HOW SURPLUS KNOWLEDGE CONTRIBUTES TO PROGRESS IN SCIENCE

7.1 Introduction

The philosopher of science Thomas Kuhn, in his 1962 *The Structure of Scientific Revolutions*, asked:

> Why should the enterprise sketched above [modern science] move steadily ahead in ways that, say, art, political theory, or philosophy does not? Why is progress a perquisite reserved almost exclusively for the activities we call science? (Kuhn [1962] 1996, p. 160).

Kuhn was putting his finger on the peculiar nature of scientific progress, namely that it appears to be continuous and cumulative in some sense. In this paper, I will provide an explanation of this progress by relating science to the more general practice of laboring. An important fact about human labor is that it can result not just in reproduction of what it started with, but in something new, a surplus product. When the latter is a means of production, it makes possible a mechanism of change consisting of reproduction by means of the expanded means of production. "Means of production" must here be understood in a broad sense, to include not just tools in a narrow sense, but also material means of representation and communication (Lefèvre 2005). Each iteration of the labor process can differ from the preceding one insofar as it incorporates the surplus generated previously. Over the long-term, this cyclical process can lead to the self-transformation of labor and, through it, of human societies and cultures.

In this paper, I will provide a largely theoretical argument that this mechanism of change is also at work in the history of science. More specifically, the thesis I will defend in this paper is that surplus knowledge contributes to progress in science. The basic argument is this. Labor makes progress by producing surplus use-values (objects of utility). Science makes progress as does the labor process, except that the specific use-value that it produces is knowledge. Therefore, science makes progress by producing surplus knowledge.

The paper is structured as follows. In section 2, I argue that the form taken in science by the mechanism of reproduction by means of the expanded means of production is that of a feedback loop between discovery and instrument construction. This process requires the integration, and transformation into material form, of different kinds of knowledge. In section 3, I argue that this process suggests a concept of scientific progress complementary to those that have so far been advanced in the philosophical literature on scientific progress, and defend the concept of progress as transcendence of native human epistemic ability. In section 4, I criticize narrowly biologistic approaches to the history of science for ignoring the role of surplus generation in transforming the labor process, and discuss some problems associated with viewing science as labor. I offer concluding remarks in section 5.

7.2 The dialectic of discovery and embodiment

My view is that the specific product of scientific labor, scientific knowledge, contributes to scientific progress. There is a sense, already recognized by philosophers of science, in which scientific knowledge may be said to contribute to scientific progress: that is when knowledge accumulated in a scientific episode is said to *constitute* progress.[445] That is not the sense I intend. Rather, I mean that the knowledge accumulated provides a starting-point for future work. Again, there is an obvious sense in which this is true, for the acquisition of new knowledge inevitably suggests lacunae to be filled and new questions to be answered. What I would like to draw attention to, however, is that the knowledge accumulated provides a starting-point for future work in the sense of contributing to a *stock of knowledge* from which future scientists can draw.[446]

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[445] See Mizrahi (2010) and Niiniluoto (2015) for reviews of philosophical accounts of scientific progress.

[446] This function of scientific knowledge was described by the noted chemist Carl Djerassi in Sturchio & Thackray (1985):

As will be explained in more detail in the following sections, instruments represent an important way in which the stock of knowledge can be incorporated into the scientific labor process (or ordinary material labor processes, for that matter). I claim that an important way in which instruments contribute to scientific progress is by making possible a *dialectic of discovery and embodiment.* Scientists start from the stock of past results or knowledge. Using this knowledge, they produce instruments that they then use to discover new things about the world.[447] If successful, new items will be added to the stock of knowledge. The augmented stock can then be used to build new or improved instruments, thus renewing the cycle.

On this conception of scientific progress, the stock of knowledge is viewed as a means of production for on-going research. It is a means for the production of the material means of discovery, the instruments. The latter are used to acquire new knowledge. The overall product of the process is a transformed stock of knowledge. Transformed how?

According to Mizrahi (2013), scientists make judgments about progress according to the following pattern:

1. Survey the body of knowledge *B* in field *F* at time *t* prior to discovery *D*.
2. Estimate what was known (*B*) in *F* at *t*.
3. Identify a lacuna, imprecision or error in *B* at *t*.
4. Spell out how *D* improved on *B* by adding new knowledge, correcting imprecision or exposing errors and correcting them.[448]

> I have a very different opinion of what a publication is. It is really to pay back to the scientific pool of knowledge from which we borrowed so much, because that's all that science is really—stepping on someone else's shoulders. Put it back in there, and let other people select what they need or what they do not need. Some of the things that you yourself think are trivial may sometimes be exactly the trivial things that someone else needs to jump on very quickly.

Though Djerassi seems to have in mind the use of past results to solve immediate research problems, my focus is on another use, connected with tool use, that contributes to progress in the long-term.

[447] I do not claim that the production of new instruments is the only way to make new discoveries. New theoretical ideas, as well as new sorts of experiments using old instruments, can also contribute to discoveries. For reasons provided below, however, I think instruments have distinctive properties that contribute to discovery differently than ideas or new experiments.

[448] Mizrahi (2013), p. 379.

Here, the "body of knowledge *B* in field *F*" is similar to my 'stock of knowledge,' except that for reasons I will provide below, my 'stock of knowledge' is not field-specific but involves the totality of scientific and technological knowledge. Admittedly, the contours of this totality are vaguely defined. But I think the history of scientific innovation bears out that the latter often involves the creative integration of ideas and practices from multiple fields (Harman & Dietrich 2018, pp. 9-10). What combination of fields contributes to innovation in a particular episode depends on the specifics of the episode, one obvious constraint being what fields the scientists are familiar with. These specifics cannot be determined *a priori*—in chemist Carl Djerassi's words, it is up to the players to "select what they need or do not need." Vagueness is a virtue in this case.

Paraphrasing Mizrahi, we are interested in cases where a discovery *D* improves on the stock of knowledge *S* by adding new knowledge, correcting imprecision or exposing errors and correcting them. This improvement yields a transformed stock of knowledge *S'*, which is distinguished from *S* in virtue of containing more knowledge, being more precise or having fewer errors.

I have used the term 'surplus knowledge' to designate certain features of the relation of new knowledge to the stock of knowledge. Surplus knowledge is not simply new or recently acquired, but stands in a definite relation to pre-existing knowledge. It is knowledge that is acquired by means of pre-existing knowledge, and which transforms the latter in the sense specified above. Tentatively, for the sake of clarity I suggest the following analysis of surplus knowledge:

> (SK) Discovery *D* is an item of surplus knowledge if and only if (i) *D* was acquired by means of stock of knowledge *S* and (ii) its addition to the stock yields an improved stock *S'*, where the improvement consists in adding new knowledge, correcting imprecision or exposing errors and correcting them.

According to (SK), a discovery that does not yield an improved stock of knowledge does not count as surplus knowledge. A situation where this sort of non-progressive discovery occurs is one where a discovery *D* merely cancels out a prior claimed discovery *C*. For example, the invention of the telescope represented a form of knowledge,

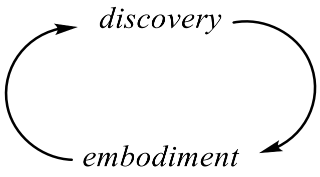
knowledge of how to observe distant objects. By means of this knowledge, Galileo discovered that Venus has phases, just like the moon. This situation was inconsistent with Ptolemaic theory, so it was eliminated. If matters had stood there, the phases of Venus could hardly have counted as "surplus," given the loss of what European astronomers had thought for centuries they knew about the solar system. Luckily, there was another theory competing with the Ptolemaic, the Copernican system, and since it was consistent with Venus's phases (among other reasons) it replaced the Ptolemaic.

It also follows from this analysis that if a discovery is not achieved by means of the stock of knowledge but nevertheless yields an improved stock *S'*, it would also not count as surplus knowledge. For example, were a UFO to visit my home and an alien species to come out, then perhaps I could plausibly claim to have discovered those aliens for humanity. Even in this case, one might question whether this discovery was made without relying on prior scientific knowledge, since the identification of a new species presupposes scientific concepts, like that of a 'species,' as well as empirical knowledge of already known species. Setting aside such considerations, however, cases like this one are best described as *windfalls*, acquisitions that cost no labor to the acquirer. Unfortunately for scientists, their importance is minimal.

A more realistic scenario is one where a discovery has consequences that go beyond the discovery itself. For example, the discovery of the double-helical structure of DNA was not in itself a momentous discovery; as far as molecular structures go, this one was fairly boring. But, *in addition*, the structure solved the puzzle of genetic inheritance, and led to important applications like gene editing. Are these additional discoveries windfalls or SK? It would seem the latter, since such consequences depend on the stock of knowledge as well, though different parts of it than the initial discovery. The double helix was discovered by means of X-ray crystallography, chemical information about the base composition of the molecule, and mechanical model building. Establishing that the structure was the genetic material, however, required evidence of its duplication mechanism as well as of its role in protein synthesis. Indeed, it was only when the outlines appeared of a mechanism for DNA's involvement in protein synthesis that the biochemical community began to take a serious interest in the structure (Olby 2003). This example illustrates a point I will make later for instruments, that some degree of integration with other knowledge is usually necessary to exploit new discoveries.[449]

In this chapter, I will focus on additive improvements to the stock of knowledge, which is much more common than the spectacular cases of theory overthrow that philosophers have tended to focus on. Nevertheless, in the more interesting cases, the relation between surplus knowledge and the stock of knowledge will not merely be additive. As will be discussed in greater detail in the following subsections, the expanding stock of knowledge does not remain external to scientific work, but releases possibilities for the development of that work. This release occurs because the new knowledge reveals new, useful employments of the old knowledge.

Schematically, the dialectic of discovery and embodiment may be presented as in Figure 7.1:



$$1)\ k_1, k_2, \ldots k_i \xrightarrow[\text{(a)}]{\text{production of}} \text{instruments} \xrightarrow[\text{(b)}]{\text{discovery}} k_{i+1}, k_{i+2}, \ldots k_n$$

$$2)\ k_1, k_2, \ldots k_n \xrightarrow{\text{repeat (a) and (b)}} k_1, k_2, \ldots k_n, k_{n+1} \ldots k_m, \text{etc.}$$

Figure 7.1 The dialectic of discovery and embodiment. Scientists start from the stock of past results or knowledge. Using this knowledge, they produce instruments that they then use to discover new things about the world. If successful, new items will be added to the stock of knowledge. The augmented stock can then be used to build new or improved instruments, thus renewing the cycle.

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[449] In the context of a critique of empiricism, Karl Popper ([1960] 1985) made the following remark on the role of prior knowledge in scientific progress:

> Knowledge cannot start from nothing—from a *tabula rasa*—nor yet from observation. The advance of knowledge consists, mainly, in the modification of earlier knowledge. Although we may sometimes, for example in archaeology, advance through a chance observation, the significance of the discovery will usually depend upon its power to modify our earlier theories. (55)

Here, each k_j is some instance of knowledge. The initial store of knowledge is represented by k_1, k_2, … k_i, the "surpluses of knowledge" by k_{i+1}, … k_n and k_{n+1}, … k_m.

Some qualifications are in order. The "knowledge" at issue in this iterative process has to be understood broadly, in a twofold sense to be described shortly. The first sense has to do with the form of knowledge, and the second with its source. Corresponding to the first sense is a process of integrating knowledge I call 'form-integration.' Corresponding to the second sense is another process of integrating knowledge I call 'source-integration.'

7.2.1 Form-integration

First, the knowledge at issue in the dialectic of discovery and embodiment involves not just theoretical knowledge, but also empirical knowledge and various kinds of know-how. The conception of knowledge I employ follows Mizrahi (2013). Basing his argument on evidence from scientists' reflections on progress, Mizrahi argues that scientists employ a broad conception of progress that includes different kinds of knowledge. The four kinds he identifies are:

> (EK) *Empirical Knowledge*: Empirical knowledge usually comes in the form of experimental and observational results.
>
> (TK) *Theoretical Knowledge*: Theoretical knowledge usually comes in the form of well-confirmed hypotheses.
>
> (PK) *Practical Knowledge*: Practical knowledge usually comes in the form of both immediate and long-term practical applications.
>
> (MK) *Methodological Knowledge*: Methodological knowledge usually comes in the form of methods and techniques of learning about nature. (Mizrahi 2013, p. 380)

The reason a broad conception of knowledge is necessary is that scientific instrument production and use require more than just theoretical knowledge but also empirical knowledge and know-how. Theoretical knowledge may provide basic principles of design, as for example spectrometers are designed based on principles of quantum mechanics and electromagnetism. But empirical knowledge may be required for calibration or data interpretation. The rationale for using the instruments is usually based on methodological knowledge, and in fact their use is often called a “technique” or “method.” Moreover, instrument construction involves a great deal of practical knowledge, for example knowledge of how to grind lenses in the case of the telescope (van Helden 1983) or of how to produce a vacuum in that of the cyclotron (Baird & Faust 1990).

I call the process of combining these four kinds of knowledge ‘form-integration,’ because it involves integrating different kinds of knowledge distinguished according to their form: theoretical, empirical, practical, or methodological.

In terms of the Figure 7.1 schema, form-integration may be represented as in Figure 7.2:

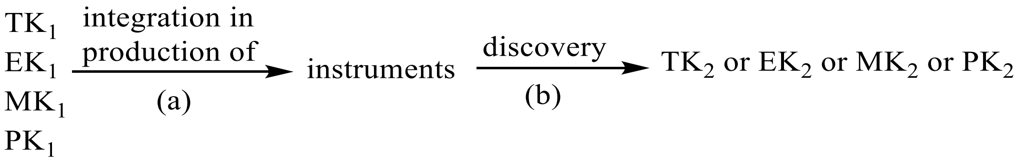

Figure 7.2 Integration of different forms of knowledge (theoretical, empirical, methodological, or practical) in the dialectic of discovery and embodiment.

The production of instruments typically requires the integration of different forms of knowledge. Their use can lead to the discovery of TK, EK, MK, or PK. The example of the clock will be discussed in section 2.4.1.

7.2.2 Source-integration

The second sense in which the knowledge involved in the process has to be understood broadly is that the process can only fully realize its potential for progress if it concerns collections of instruments and discoveries, not just individual instruments. The reason is that discoveries made by means of an individual instrument cannot necessarily be used to improve that very instrument. For example, the knowledge of the moon’s surface afforded by Galileo’s telescope could not be used to improve the telescope itself.

True, strictly recursive improvements of instruments may be conceivable for certain kinds of improvement, such as for increasing precision. In his well-known account of the development of temperature standards, for example, Hasok Chang describes a succession of increasingly precise instruments, starting with the hands and ending with the high-precision Beckmann thermometer, for estimating warmth.[450] Each instrument in the sequence provided a standard for assessing the reliability of its successor. In the terms of the broad conception of knowledge above, the methodological knowledge (MK) represented by each instrument in the sequence provided the starting-point for the design and validation of a more precise successor.

But this kind of strictly recursive progress only captures part of what is involved in instrument construction and use. In the early development of the telescope and microscope, for example, it was recognized that both theoretical and practical knowledge might be useful, the former in the form of optical theory and the latter in the form of lens-crafting knowledge.[451] Instrument development tends to be holistic, drawing on many sources and kinds of knowledge. Indeed, one of the things instruments allow us to do is to make use of knowledge on a far greater scale than it is possible for the individual human user to know him- or herself. This ability arises from the fact that we can use an instrument without knowing all the things necessary to make it.

I will call the process of combining knowledges from different sources ‘source-integration,’ because it involves integrating different kinds of knowledge distinguished according to their source, which in this paper will be a practice or field. The telescope example involved integrating knowledge from the science of optics with knowledge from the practice of lens-crafting. As this example also illustrates, the two kinds of integration can overlap. But they need not, as when theoretical knowledge from different sciences is combined.

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[450] Chang (2004), pp. 47-48 summarizes the process of developing numerical thermometers starting from the senses; Chang (2007), pp. 9-11 extends the analysis to Beckmann thermometers.

[451] Spelda (2017) documents this recognition by 17th century natural philosophers. According to Smith (2015), pp. 381-391, both theoretical and practical knowledge contributed to the development of the microscope and telescope, but practical knowledge led the way.

In terms of the schema of Figure 7.1, source-integration may be represented as in Figure 7.3 for the case of the telescope:

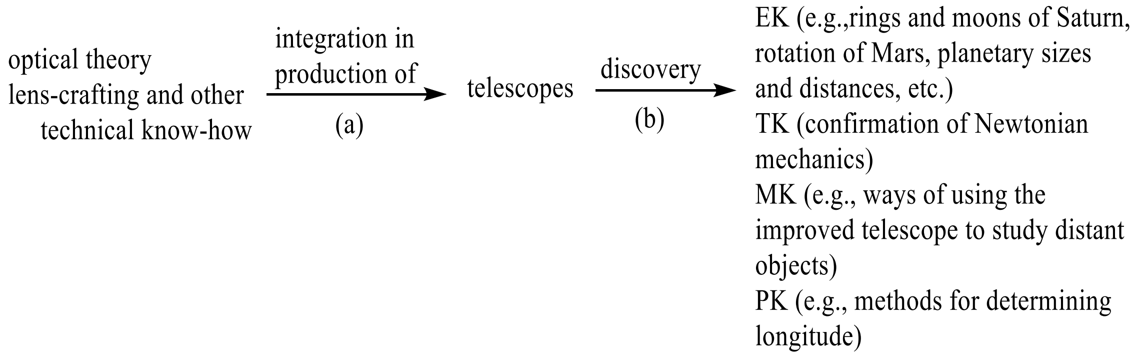

Figure 7.3 Integration of knowledge from different sources in the 17th century dialectic of discovery and embodiment involving the telescope.

In general, both surplus knowledge and integration are required for instrument development. True, sometimes integration by itself, combining long-established items of knowledge, can result in a new instrument. Conversely, surplus knowledge may also be sufficient, as in the recursive example just discussed. But in general, some degree of integration is necessary to exploit new discoveries, and a new discovery is necessary to make some combination of knowledges useful. Examples will be given below.

7.2.3 “Embodiment”

A final qualification is that “embodiment” is difficult to define precisely. Perhaps one way of putting what is meant by this term, for my purposes, is that it involves finding some functional substitute in material form for whatever items of TK, EK, MK and PK are needed to build the instrument. Though he does not categorize knowledge in exactly the same way, Davis Baird (2004) provides an example of this process in his account of the development of direct-reading spectrometers:

> we can see in it how various kinds of knowledge were integrated into a material medium to produce a measuring instrument. Model knowledge is built into the instrument in several ways, including the material representation of wavelengths of light emitted by important elements in the “exit slits” of the instrument ... Working knowledge is built into the instrument, again in several

> ways, including the use of a diffraction grating to disperse light into the constituent wavelengths ... Theoretical knowledge is also built into the instruments, of which the theory of condenser discharge is a particularly clear example ... Functional substitutes for human discriminatory skills are built into the instrument too. With a spectrograph, where photographic film is employed instead of photomultiplier tubes, humans have to determine how dark—or “dense”—a “spectral line” is; instruments called densitometers helped to refine this skill. With a direct-reading spectrometer, photomultiplier tubes and electronics are crafted to provide a functional substitute for this skill. The material medium of the instrument encapsulates and integrates all these different kinds of knowledge. All are necessary for the instrument to render information about a specimen. (Baird 2004, p. 70)

As the instrument is built, so the knowledge required to build and use it is “built into” the instrument as well.

Instruments are powerful vehicles for the dialectic of discovery and embodiment. Why? After all, the accumulation of theoretical knowledge may be sufficient to enable further discovery. For example, according to Hempel (1966, pp. 76-77), a good theory will allow us to discover phenomena that were not known at the time the theory was formulated. Nevertheless, the possibility of embodying knowledge in instruments paves the way for greater progress in knowledge than would be possible without it.

Why? The usual answer is that instrumentation provides access to objects of inquiry that are inaccessible by means of our native human abilities. However, four further features of instruments also contribute to the growth of knowledge. First, and as noted in chapter 2, a complementary, but less obvious, answer that more directly affects the temporal characteristics of scientific research is that the instrument’s contribution is not necessarily fixed once and for all but can be enhanced over time, more so than human abilities. The basic reason is that technology is improvable in a much deeper way than are our native abilities. The degree to which the latter are improvable is constrained fundamentally by human biology. In contrast, the improvement of instruments is, in principle, only constrained by the laws of nature, though in practice it must be adapted to human users.

Second, there is the feature, alluded to earlier, that we can use an instrument without knowing all the things necessary to make it. What might be called the “black-box-ability” of the instrument allows a much greater amount of knowledge to be brought to bear in research than would otherwise be possible.

Through these properties, instruments extend our observational and computational powers. Some of the new knowledge yielded can then be embodied in new instruments by way of their improvability. Something similar can happen with humans. Human computational powers can be improved by the discovery of algorithms like the rules of arithmetic. This would be another recursive case, where a discovery made by means of "instruments"—the mind and means of mathematical representation—could be used to improve the powers of the instruments themselves. So the native human abilities can engage in this dialectic as well, but not to the same extent as artifacts due to the constraints imposed by humans' natural endowment.

A third property, related to black-box-ability, is *durability*. Because they are things rather than activities, instruments can subsist beyond the subjective activities that engendered them and serve in new activities. Durability allows future users to take advantage of the producers' work and knowledge. It also allows the instruments to be perfected.

Moreover, because they are durable, instruments can provide *scaffolding* for the integration of new knowledge into the labor process. By 'scaffold,' I intend a structure that allows a new structure to be constructed from it. The old structure may be physical or conceptual, a design for example. Many instruments are not developed *de novo*, but rather from the modification of precursors or precursor designs. The precursor or precursor design provides a scaffold for the development of new instruments. The old knowledge embodied in the precursor or precursor design provides a structure within which new knowledge can be exploited.

7.2.4 Some examples

I will now provide a few historical examples of how source-integration, form-integration, and embodiment work together to produce change in science.

7.2.4.1 Clocks

The clock provides an example of an instrument with extremely important applications both inside and outside science. According to Landes (1987, 2000), the invention of the mechanical clock was a seminal event in the history of methods of measuring time, though its importance was only made possible by later developments. The European Middle Ages inherited two types of time-keepers from antiquity, the sun-dial and the water-clock. Both were based on the same principle: the continuous measurement of a continuous phenomenon. They both had major context-dependent defects. Sun-dials don't work at night nor when the sky is cloudy, the latter being a serious impediment in cloudy regions. Water-clocks are very sensitive to changes in temperature, which makes their proper functioning vulnerable to daily and seasonal temperature variations. The mechanical clock, invented around 1300 CE, was relatively free of these defects, yet that was not what made it a revolutionary time-keeper. What made it revolutionary was its principle: instead of tracking the passage of time by imitating its continuous flow, it made beats according to an (ideally) regular rhythm and counted them.

According to Landes, this "digital principle" made possible all subsequent improvements in time-keeping techniques. All clocks based on this principle, starting with the first mechanical clocks, comprised the same five basic design features (Landes 2000, pp. 6-10 and 413):

1. A source of energy (e.g., falling weights, spring or battery)
2. An oscillating controller (e.g., balance, quartz crystal)
3. A counting device (e.g., escapement, solid-state circuit)
4. Transmission (e.g., wheelwork, electric current)
5. Display (e.g., hands, liquid-crystal display)

Though the earliest mechanical clocks used a foliot crossbar to control the rhythm, subsequent controllers, such as the pendulum (invented by Huygens in 1657), the tuning fork, quartz, and atoms, were all based on the same principle. Though the inventor(s) of the original mechanical clock could not have anticipated these later versions, the recourse to an oscillator and the other design features provided a scaffold within which subsequent discoveries and inventions could be exploited. For example, in the early 20th century, quartz crystals were being used to emit radio signals, based on the piezoelectric effect discovered by Pierre and Jacques Curie at the end of the 19th century. Though the signals emitted by the first such crystals were unstable, improvements in the preparation of crystals and in their integration within resonating circuits resulted in stable high-frequency resonators. The physics of high-frequency resonators could then be exploited in the invention of quartz clocks. High-frequency resonators are both less prone to dampening, and keep a more stable rhythm, than low-frequency. Further modifications were required, however, to take advantage of these properties. Thermal effects on crystals are small relative to mechanical clocks, but for scientific measurements they are non-negligible. Laboratory quartz clocks were eventually equipped with a thermostatic enclosure with a variance of 1/10,000 ºC. To counter variations of frequency caused by accident or by changes in power supply, the quartz was inserted into a closed resonance system in which fluctuations were detected and corrected by a servomechanism.

These improvements produced quartz clocks that kept time with a precision of a hundredth of a millisecond per day. Ultimately, the development of high-frequency clocks permitted measurements of phenomena occurring on tiny timescales, in some cases on the order of femtoseconds. According to Landes, the result of the high-frequency revolution in clocks was that the measurement of time and frequencies became much more widespread across scientific domains, especially in astronomy, telemetry, interferometry, physics, and, I might add, chemistry in the form of new areas of research like femtochemistry. Furthermore, the merits of high frequencies made possible a number of applications based on the use and control of short time intervals and of very transient phenomena, including multiplying the number of communications simultaneously transmittable through the same wire and improving computer processing speeds.

Viewing this episode as an example of form-integration, we might say that an instance of practical knowledge (PK), the clock based on the digital principle, provided a scaffold on which theoretical and empirical knowledge (TK and EK) could be exploited. The integration of these knowledges into the scaffold then allowed new methodological knowledge (MK), empirical knowledge, practical knowledge and theoretical knowledge to be acquired.[452] This example also illustrates the use of instruments to extend our observational reach as well as their capacity for improvement.

7.2.4.2 The mass spectrometer

The mass spectrometer provides an example of an instrument built expressly for scientific purposes.[453] In mass spectrometry, the components of a sample are ionized and then separated by various arrangements of electric and magnetic fields. TK is employed here in the form of the laws governing the motions of charged particles. The mass-to-charge ratio of each kind of ion is measured, and this information allows the components of the sample to be identified. Prior to the 1940s, the photographic plate was the most common method of detection, which required the skill of measuring spectral line density that Baird alludes to in the passage quote above. Starting in the 1940s, the photographic plate tended to be replaced by electronic detectors, which produce an amplifiable signal. This modification enabled automatic strip chart recording of the mass spectrum, which simplified and accelerated spectrum recording compared to the photographic method. Strip chart recorders yielded an analog recording, however, which had to be converted into tabular form through a labor-intensive process. The earliest use of computers (1958) in mass spectrometry was that of a digitizer that could tabulate the data as the spectrum was being generated. The Mascot digitizer was itself fairly crude, in that it was unable to do anything else but digitize the output of the spectrometer to which it was hard-wired. But digitization eventually enabled new applications of the computer to mass spectrometry in the 1960s. The DENDRAL algorithm was developed to interpret the spectra of unknown compounds, albeit with limited success. High-resolution mass spectrometry, which allows deduction of elemental composition, relied heavily on computers to digitize the data from the detector and process them into exact mass and intensity information. Library search algorithms were developed to match the spectra of unknowns with those of reference

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[452] For example, Ahmed H. Zewail's (1999) Nobel lecture on femtochemistry reviews the various observations, methods, theoretical concepts and models, and applications that emerged through the study of chemical bond dynamics on the femtosecond scale.

[453] The following relies on Grayson (2004) and Nier et al. (2016).

compounds. In the 1970s, techniques and instrumentation were developed that allowed the spectrometer to be coupled with a gas chromatograph and a data system. The GC-MS-DS was capable of generating several hundred spectra per half hour, which could eventually (1990s) be compared via library search algorithms to libraries containing hundreds of thousands of reference spectra. In contrast, only a few spectra per hour could be prepared by an operator using a strip chart recording machine of the 1940s and 1950s.

In this example, the spectrograph of the 1930s provided a scaffold for the exploitation of new technologies—electronic detectors, digitizers, computers, software, and instrument-instrument interfacing technologies. The process of integrating these technologies into the mass spectrometer resulted in instrumentation with capabilities that far exceeded what was possible with the old spectrograph.

The mass spectrometer was not an isolated case, but rather part of a far-reaching transformation in how chemistry was done known as the "Instrumental Revolution."[454] For our purposes, what is interesting about the latter is that it illustrates the dialectic of discovery and embodiment on *a social scale.*

7.2.4.3 The Instrumental Revolution in chemistry

This was an episode in which knowledge produced in diverse disciplines was combined in the form of new instruments for chemical analysis. Various scientific and social developments—the discovery of quantum phenomena, the emergence of computer science and physical organic chemistry, the needs of the petrochemical and rubber industries during the Second World War, the prioritizing of output by university administrations—converged to make the embodiment of scientific and technological knowledge possible and sought after as a means for solving chemical problems. This process resulted in the emergence of many high-tech methods of chemical analysis, of which some of the better known are nuclear magnetic resonance (NMR), mass spectrometry (MS), ultraviolet spectroscopy, infrared spectroscopy, Raman spectroscopy, and X-ray crystallography.

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[454] Overviews of this episode may be found in Borg (2019), Steinhauser (2014), Reinhardt (2006), and Morris (2002).

By analogy with Figure 1, the epistemic component of the convergence process might be illustrated, in a highly simplifying way, as shown in Figure 7.4:

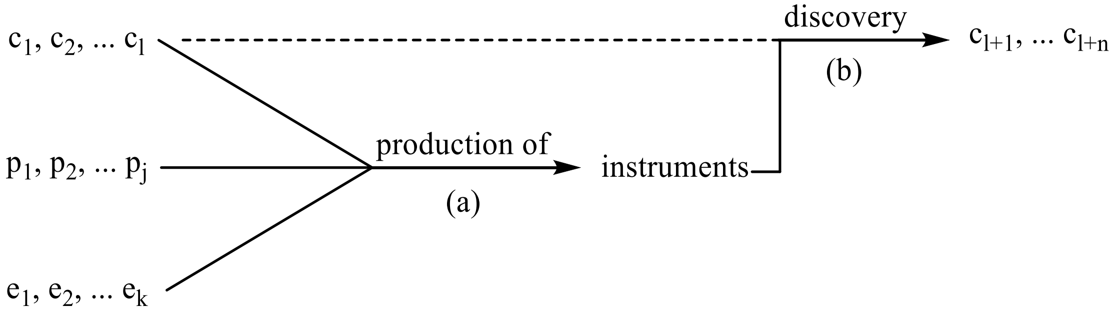

Figure 7.4 The Instrumental Revolution expanded the set of instruments available to chemists for analytical purposes by combining knowledge from different sources (chemistry, physics, engineering sciences, etc.) in the production of new instruments.

The series on the left represent instances of kinds of knowledge: the c_i's chemical knowledge, the p_i's physical knowledge, and the e_i's "engineering knowledge," a broad notion intended to cover computer science, electrical engineering, and various forms of know-how.[455] There is expansion, as chemistry acquires more instruments than it had at the beginning. There is also reproduction, as the new instruments allow chemists to continue doing chemical analysis. In addition, most of the old knowledge was retained, since the new methods were compatible with the old. There was thus accumulation not just of the stock of instruments, but of chemical knowledge. Indeed, the surplus knowledge afforded by the new methods was vast, for the transformation not only accelerated traditional chemical analysis but made possible many new lines of inquiry. Moreover, some of the knowledge obtained was used to develop new generations of instruments.[456]

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[455] Baird & Faust (1990) argue that such know-how is essential for the construction of scientific instruments. Kletzl (2014), pp. 122-123 argues that there exists "engineering theory," consisting of systematic propositional language of how to manufacture an artifact, in contrast to the "explanatory theories" of natural science.

[456] See Becker et al. (1996) for a survey of the new lines of inquiry and instruments made possible by the development of NMR spectroscopy.

The diagram also illustrates a mechanism of expansion: the combination of knowledge from different sources in the production of the new instruments, which would not have been possible with a more insular mode of development. This is a case of what I called ‘source-integration’ above. In this respect, the episode illustrates, on a social scale, a pattern of innovation observed by Harman & Dietrich (2018) at the level of individual scientists: the creative integration of ideas and practices from multiple fields. In the case of the Instrumental Revolution, however, the integration was driven not just by the nature of human creativity, but also by the technical requirements of applying knowledge of physical phenomena to chemistry. Each of the new techniques was based on a physical phenomenon. For example, NMR is based on the detection of transitions between energy levels of nuclear spins in bulk materials in the presence of an external magnetic field. The initial phenomenon, however, was generally useless for other than physicists interested in measuring nuclear magnetic moments, and instrument specialists like Herbert Gutowsky or Richard Ernst,[457] until a host of supporting knowledges and technologies were brought to bear. Mechanization was required to develop instruments that had the speed and control needed to produce data informative enough to compete with traditional chemical data. For example, carbon is the key structural element in organic chemistry. The ^{13}C NMR effect was discovered in 1957 by means of early NMR spectrometers. But the combination of only a 1.1% natural abundance of ^{13}C, the only carbon isotope with a nuclear spin, and its relatively low intrinsic sensitivity initially prevented the routine exploitation of this effect. The latter was achieved in large part through technical improvements including the introduction of computers (which allowed the signal-to-noise ratio to be improved), the incorporation of techniques for stabilizing the magnetic field, and the employment of more powerful magnets. In 1972, the first routine ^{13}C NMR spectrometer for organic chemists was brought to market.[458] Further improvements, notably the development of Fourier

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[457] See Reinhardt (2006) for detailed accounts of the contributions of these and other instrument specialists to the development of the new techniques.

[458] The instrument was the Varian CFT-20. I rely here on Becker et al. (1996), sections 3, 6 and 10 for the history of ^{13}C NMR. This history illustrates both steps shown in Figure 1, with the discovery of the effect being the outcome of step 1, the development and application of which led to a host of structural, mechanistic and methodological discoveries in organic chemistry and biology.

transform technology, allowed the ^{13}C NMR effect to be applied to the study of biochemical systems. In general, black-boxing, an empirical approach to data interpretation, a new division of labor and various technical improvements were other elements required to make the methods attractive to chemists outside of chemical physics.[459]

This pattern of integration is typical of the dialectic of discovery and embodiment. As noted above, the process illustrated in Figure 1 can only fully realize its potential for progress if it involves collections of instruments and discoveries, not just individual instruments. These collections can stretch across fields. The process therefore requires the convergence of a totality of labor processes. As in ordinary material production, so in science: innovation in one process needs the support of many other processes.

7.3 Progress as transcendence of the limitations of native human epistemic abilities

Historical materialists hold that the transformation of the labor process over the long-term moves humans farther and farther away from the constraints of their biological origins.[460] In this section, I will argue that the surplus-knowledge theory described in the previous section explains one of the more striking features of 20th century and contemporary science, the increasingly important role of automated or semi-automated instrumentation in scientific research. The philosopher of science Paul Humphreys has suggested that "one of the principal achievements of science has been to transcend the limitations of humans' natural epistemic abilities" (Humphreys 2004, p. 6). The reasons he gives are that the evidence of the human senses, as well as human computational abilities,

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[459] Reinhardt (2006) describes the efforts of instrument developers to adapt the methods to chemical needs. Feeney (1999) describes the role of technical improvements in making NMR applicable to chemistry.

[460] Marx & Engels (1978 [1845-6]), p. 150; Engels (1987 [1895-6]); Novack (1980), ch. 1; Damerow (1996), ch. 11; Sève (2014), pp. 285-291. I will say more about this claim in section 4.

are more error prone, and severely limited in scope, compared to what can be achieved with instruments.

I submit that these limitations suggest a backward-looking goal relative to which progress can be made. According to Niiniluoto (2015), a goal may be backward-looking or forward-looking, depending on whether it refers to the starting-point or destination point of an activity. Humphreys' suggestion suggests a kind of progress away from our natural endowment: we might say that an episode of science constitutes scientific progress if it shows the transcendence of limitations of native human epistemic abilities. For comparison, consider three other accounts of the concept of scientific progress:

> (E) An episode constitutes scientific progress precisely when it shows the accumulation of scientific knowledge. (Bird 2008, p. 279)
>
> (S) An episode constitutes scientific progress precisely when it either (a) shows the accumulation of true scientific belief, or (b) shows increasing approximation to true scientific belief. (Bird 2008, p. 279)
>
> (I) An episode constitutes scientific progress when it shows the adoption of a practice in which an instrument (technique) with more capabilities replaces one with fewer.[461]

(S), (E) and (I) are called the *semantic*, *epistemic*, and *instrumental* accounts of progress.[462] For notational congruence, I will use (H) to denote the concept of progress as transcendence of limitations of native human epistemic abilities.

> (H) An episode of science constitutes scientific progress if it shows the transcendence of limitations of native human epistemic abilities.

By 'transcendence' I merely mean (following Humphreys) that the instrument is less error-prone, or of broader scope, than a human ability that it *enhances*. I use the latter verb in the three-pronged sense of Humphreys (2004, ch. 1). Humphreys uses 'enhance' to

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[461] Adapted from Kitcher (1993), p. 117. The original reads: "Instruments and experimental techniques are valued because they enable us to answer significant questions. One instrument (or technique) may do everything another does and more besides. If so, then we make *instrumental* (or *experimental*) progress by adopting a practice in which the former instrument (technique) replaces the latter."

[462] See Mizrahi (2010) for a discussion of these and other accounts of progress.

denote three ways in which limitations of native human abilities may be overcome: by extrapolation, by conversion, and by augmentation. *Extrapolation* takes place by extending an existing modality of human ability, like vision, along a given dimension. Paradigmatic examples are the optical telescope and microscope, which bring very distant and very small objects within the range of visual detection. *Conversion* occurs when phenomena that are accessible to one sense are converted into a form accessible to another. Sonar devices that have visual displays are one example. *Augmentation* gives us access to features of the world that humans are not naturally equipped to detect in their original form, such as alpha particles, positrons and spin.

In addition to enhancement, transcendence can also be brought about through *replacement*, which occurs when an instrumental ability replaces a human ability.[463] Replacement may involve the other operations. For example, the replacement of ocular detection telescopes with photographic plate detection in the late 19th century involved extrapolation, since it greatly increased the quantity of data obtainable in the visible portion of the electromagnetic spectrum (Bigg 2000). On the other hand, the replacement of photographic detection with electronic detectors somewhat later involved augmentation because it gave astronomers access to celestial phenomena outside the visible portion of the spectrum, such as the Cosmic Microwave Background dating from the very early universe.

So a more precise formulation of (H) is:

> (H') An episode constitutes scientific progress if it shows the adoption of a scientific practice in which an instrumental ability that is either (a) less error-prone or (b) of broader scope enhances or replaces a native human epistemic ability.

For brevity, however, I will use 'transcend' in what follows, it being understood to have the meaning just given.

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[463] I thank an anonymous referee for pointing out that transcendence can involve enhancement as well as replacement.

I have worded (H) as a sufficient condition, not a necessary condition, since we want to allow for other kinds of scientific progress. My purpose here is neither to endorse nor refute these other accounts, but merely to propose a complementary account that fits certain trends in modern science.

Of the three other accounts listed, (H) is most similar to (I). (H) may even seem to be a special case of (I), in which the instrumental ability that is replaced is a native human ability. But viewing (H) thus presupposes that native human abilities are instrumental abilities. This presupposition itself, however, requires that we have already *conceptually* transcended native human abilities, in the sense of viewing them as merely one kind of instrumental ability among other possible ones by which it could be replaced (I will say more about the nature of this conceptual transcendence shortly). So native human abilities can only be subjected to the process described in (I) if they have already been subjected to a conceptual analogue of the process referred to in (H). I conclude that (H) is independent of (I).

On the other hand, (H) may be subsumable under the epistemic account of scientific progress. Since (H) concerns abilities, then the kind of knowledge involved would have to be know-how, presumably the methodological knowledge (MK) discussed in section 2.1. As noted by Mizrahi (2013), however, this is not usually the kind of knowledge proponents of the epistemic account have in mind, for they tend to be focused on TK and EK in particular.

The term ‘natural’ or ‘native’ ‘human epistemic ability’ is somewhat of a misnomer, since very few of our epistemic abilities are completely natural, in the sense of resulting solely from our biological endowment. Most human abilities require socialization and education, as well as material means. So in order to clarify its meaning, I will venture the following tentative definition:

> X is a native human epistemic ability if and only if humans[464] Y can use ability X to acquire knowledge, *and* biological facts about humans are required for the success of the exercise of X.

This definition is intended to retain the biological foundation of human abilities, while not supposing that the former is sufficient for the success of the latter, since facts about socialization, education, material means etc. may also be required for success.

7.3.1 Examples: mathematical and observational abilities

I will now provide two examples of "native" human epistemic abilities. Recall the computation example of section 2.3. We are not born with mathematical ability. It has to be acquired through socialization and education. For most people, any but the simplest calculations require material means of mathematical representation, like pencil, paper and a symbol system. Nevertheless, mathematical ability is a native human epistemic ability, because a human can use it to acquire knowledge, and biological facts about humans—that they have brains with certain features, motor skills that permit symbol manipulations—are required for the success of the human's exercise of the ability.

In contrast, consider the situation where a human Y, rather than carrying out a pencil-and-paper calculation, types instructions into a computer which tell the computer to carry out the calculation. Clearly, Y uses mathematical ability to acquire the same knowledge as in the previous case. But biological facts about humans are no longer required for the success of the exercise of the ability, for it is not Y's ability but the computer's. The success of the calculation is determined by facts about the computer software and hardware. So the mathematical ability Y uses is no longer a native human ability. Where biological facts about humans are, of course, still central is the operation of

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[464] I use the plural "humans" to allow for the possibility of collective abilities, e.g., the ability of groups of workers to carry out a task that would be impossible for each worker individually, for example lifting very heavy objects or evaluating extremely complicated mathematical proofs.

the computer, which has to be adapted to human operators, and the use of its output, which has to be useable by humans (e.g., the answer should not be in binary code).

Observation provides another example. Hacking (1983) argued that observation is a skill. Knowledge of how to observe is acquired through scientific practice. These skills can be transmitted from master scientist to apprentice, and when this happens the untrained sense perception of the apprentice is augmented by the new skills. The apprentice can use her ability to observe in order to acquire knowledge, and biological facts about human senses and cognition are required for the success of the exercise of her ability. An example alluded to in section 2.3 is the determination of the density of spectral lines on a photographic plate, mentioned in Baird's discussion of direct-reading spectrometers.

In contrast, consider the situation where the apprentice, now a mature scientist herself, replaces her old spectrograph with a spectrometer equipped with an electronic detector. Clearly, he can use the observational ability of the instrument to acquire the same knowledge as with the spectrograph (and more). But biological facts about humans are no longer required for the success of the observation. The latter is determined by facts about how the machine detects the ions generated from the sample and how the detected signals are amplified and processed into mass-to-charge and intensity information. Where biological facts about humans are still central is, as in the calculation example, the operation of the machine and the use of its output.

In both the calculation and the observation cases, we started with a situation in which a native human ability was used to acquire knowledge, and ended with one in which an analogous machine-based ability was used to acquire the same knowledge. On the assumption that in the given case the machine is either less error-prone, or has broader scope, than the human ability, then the latter has been transcended. This makes it an episode of (H).

7.3.2 The mechanism responsible for progress (H)

Assuming that I have provided grounds for thinking that (H) is a reasonable concept of a variety of progress [to borrow a phrase from Kitcher (1993)],[465] further questions are how well it fits the history of science and what mechanisms are responsible for it. I will start with the latter question. I submit that the surplus-knowledge theory described in section 2 explains this kind of progress. The extension of knowledge shows that native human abilities involved in scientific work are subsumable under more general abilities associated with general types of instruments. Ocular observation may again provide an example. In his classic 1982 discussion of the concept of observation in science and philosophy, Dudley Shapere argued for an extension of the philosophical concept of observation beyond its previous associations with perception, such that there can be observation by or with scientific instruments. Though he does not use the term, his argument was based on the impact of what I am calling surplus knowledge on scientists' understanding of their own practice of observation. Physical science claims to discover the existence of entities and processes that are not accessible to the human senses. It further claims to discover that those senses are receptive to only a limited range of types of events that form part of an ordered series of types of events, the electromagnetic spectrum in the case of the eye. This spectrum encompasses a range of wavelengths on the order of 10^{22}, of which only about 10^{-19} is accessible to human vision. As a result of the extension of knowledge about vision, then, it is realized that the eye is just a particular sort of electromagnetic receptor, capable of detecting electromagnetic radiation in a certain range, there being other sorts of receptors capable of detecting other ranges of the spectrum. The extension of knowledge thereby leads to the generalization of the notion of a receptor or detector, which subsumes the eye as one type.

A further generalization occurs when it is recognized that there are other fundamental interactions besides electromagnetic ones: strong, weak and gravitational interactions. As a result, ocular detection abilities become subsumable under the even more

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[465] I thank an anonymous referee for suggesting this phrasing from Kitcher.

general ability to detect one of the fundamental physical interactions. This general ability is associated with a type of instrument, the ‘receptor’ or ‘detector,’ which is capable of detecting one of these interactions and the entities engaging in it.

For Shapere, this process of generalization was a theoretical matter, resulting from the accumulation of theoretical knowledge. For it to have practical significance, however, that knowledge needs to be embodied in instruments. From what was said in section 2, it follows that two conditions are necessary. First, the auxiliary knowledge must be available for form- and source-integration to be possible. Second, the appropriate scaffolding must be available for embodiment. If these and other conditions (fit with scientists’ goals being an obvious one) are met then the surplus knowledge can be applied in research.

Shapere’s account of observation in science illustrates the reflexive character of scientific knowledge: as the latter accumulates, it sheds light on scientific practice itself, in particular by showing how native human epistemic abilities are subsumable under more general abilities that can also be exercised by instruments.

7.3.3 Fit with the historical record

How well does (H) fit the historical record? Space does not permit a survey, so I will take mass spectrometry as a fairly typical example of technologically driven transformations in bench-top science in the 20th century.[466] The reader will recall the brief account of the development of mass spectrometry in section 2.4. Table 7.1 shows which native human epistemic abilities were transcended in that episode:

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[466] Humphreys (2004) has more examples, though his focus is not historical.

Table 7.1 The transcendence of native human epistemic abilities in mass spectrometry.

Abilities	*How Exercised*	*How Transcended*
Ocular detection	Reading of photographic plates	Electronic detectors, amplifiers
Data processing	Tabulation of analog recording data	Digitizers, minicomputers
Problem-solving	Interpretation of spectra	Interpretation algorithms
Memorial	Data storage, instrument control, multi-tasking	Hard disks, RAM, CPUs
Pattern recognition	Comparison of spectra of unknowns to references	Pattern recognition algorithms
Searching abilities	Searching of spectral libraries	Search algorithms
Manipulative	Instrument control; sample handling and transferring between instruments; densitometering	Computer control; automated sample handling and direct instrument coupling; automatic recording

The left-hand column lists various abilities that were involved in the production and use of mass spectra by means of the spectrograph and early spectrometers. The center column lists the ways in which those abilities were exercised in this field. The right-hand column lists the means by which those abilities were transcended. Computerization obviously played a large role, but advances in electronics, separation techniques and instrument interfacing technology were also important. As noted in section 2.4, the replacement of photographic plates by electronic detectors made computerization possible, and electronic detectors are themselves based on the photoelectric effect. Besides massively increasing the scope of mass spectrometry, the replacement of "manual" methods appears to have reduced the likelihood of errors (Serum 2016). The development of mass spectrometry therefore seems like a good candidate for (H).

7.4 How to be scientific about scientific change without being reductionist

A famous proponent of another backward-looking notion of scientific progress was Thomas Kuhn. As is well-known, he suggested that we may have to abandon the notion of progress as approaching closer and closer to the truth. To replace it, he proposed a Darwinian move, in which the idea of an evolutionary process with a distinct goal was to be replaced by the idea of an evolutionary process that has moved steadily away from primitive beginnings. Though he acknowledged that “the analogy that relates the evolution of organisms to the evolution of scientific ideas can easily be pushed too far,” he immediately added that “it is very nearly perfect” with respect to the question of whether there is progress through scientific revolutions:

> The process described in Section XII as the resolution of revolutions is the selection by conflict within the scientific community of the fittest way to practice future science. The net result of a sequence of such revolutionary selections, separated by periods of normal research, is the wonderfully adapted set of instruments we call modern scientific knowledge. (Kuhn ([1962] 1996, pp. 171-2)

I note in passing that he does not say much about what scientific knowledge is adapted to, except that the process leads to “an increase in articulation and specialization.” van Fraassen (1980) makes an even stronger claim than analogy, holding that “science is a biological phenomenon, an activity by one kind of organism which facilitates its interaction with the environment.” He uses this claim to appropriate the no-miracles argument for scientific realism on behalf of constructive empiricism:

> I claim that the success of current scientific theories is no miracle. It is not even surprising to the scientific (Darwinist) mind. For any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive—the ones which *in fact* latched on to actual regularities in nature. (van Fraassen 1980, pp. 39-40).

David Hull’s (1988) theory, that the history of science is the result of selective pressures operating on scientific theories, works this idea out in detail.[467]

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[467] See Renzi & Napolitano (2011) for a longer analysis of the various evolutionary analogies that have been used to describe or explain scientific change.

By emphasizing the commonalities between science and ordinary labor I have implicitly taken a naturalist approach to the study of scientific progress. Darwinian theories of scientific progress are a major alternative naturalist approach in philosophy of science. From the perspective adopted in this paper, however, the analogy or identification of science with natural selection amounts to a misrecognition of the peculiar character of the evolution engendered by the labor process, insofar as it attributes a feature of a particular kind of human labor—scientific progress—to a mechanism that has nothing specifically to do with human labor.[468]

The reproduction of animals is characterized by the development of the individual from birth to death, by its physical reproduction in interaction with nature, and by the reproduction of the characteristics of the species by means of procreation and genetic inheritance. The only mechanism of change is random variation of individuals followed by natural selection of mutants with a selective advantage in a given environment.

The labor process is special because it can bring about a material result over and above the means of subsistence required for individual survival, and it can do so as a systematic and planned outcome. This result consists in the produced means of production, paradigmatically represented by tools of material production but also including cognitive tools like material representations and symbol systems (Damerow (1996), ch. 11; Sève (2014), pp. 285-291). Under appropriate social conditions, such as the existence of a social division of labor, these material means can be accumulated, creating an environment of implements that forms the starting-point for renewed cycles of reproduction with expansion. The expanding environment of implements does not remain external to the labor process to which it owes its existence, but in turn releases the inherent possibilities of the process. The feedback between the accumulated means of production and the labor process

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[468] The following is indebted to the discussion of cultural evolution in Damerow (1996), ch. 11. In a somewhat similar vein, Gerson (2014) discusses problems with understanding cultural evolution by analogy with biological evolution, though without Damerow's emphasis on the development of material culture as a cause of divergence between them.

In the context of a reconsideration of Kuhn's image of science, both Marcum (2018) and Renzi & Napolitano (2018) discuss problems with understanding scientific change by analogy with Darwinian natural selection. I thank an anonymous reviewer for sharing these references with me.

means that the process of accumulation is not linear but rather expands and accelerates exponentially. This acceleration is not merely quantitative, but includes essential qualitative changes based on the reflexive character of the tools: because the environment of implements is constantly changing, the techniques and organization of the labor process are also constantly changing. As a result, the development of the individual human takes place under constantly changing initial conditions. The reproduction of the characteristics of the species in the individual can no longer be satisfied by reproduction through procreation and genetic inheritance, but requires socialization and education. It follows that to the extent that reproduction of the individual involves the transmission of the characteristics of the species—in particular the ability to use and produce tools—to the individual, this reproduction is from the outset an essential cause of the development of society and simultaneously an effect of it.

The fact that the labor process can result in a surplus is a very important one for understanding human history. This fact makes possible *a new mechanism of change, over and above random variation and selection*: reproduction by means of the expanded means of production. Each iteration of the labor process differs from the preceding one insofar as it incorporates the surplus generated previously.

So far we have only been considering one aspect of the labor process, that it is capable of generating a surplus product. As pointed out by the philosopher and historian Wolfgang Lefèvre, in addition to a surplus product, surplus *knowledge* can also be obtained (Lefèvre 2005, section 3.1). Lefèvre claims that, in the utilization of definite means for tackling specific problems, more knowledge can be acquired than was necessary to invent the means, because "by applying a material means in the labor process, its material nature can reveal new ways of application and employment, which were not given along with the original ends" (Lefèvre 2005, p. 215). This fact explains the growth of knowledge in general. It also, Lefèvre argues, explains the growth of *scientific* knowledge. Like ordinary material labor, science also makes use of material means. The material means of science include not only things that resemble, or in fact are, production apparatuses, like certain observational instruments or, say, distillation apparatuses. They also include "material means of scientific thinking" like diagrammatic representations or numerical notations. The material means of thinking "delineate a horizon of what results scientists can achieve and even what results are conceivable or probable.” The application of the material means of science generates a surplus of scientific knowledge, as more knowledge is gained in applying them than was needed to invent them.

Lefèvre’s account raises two questions. First, given that all forms of labor use material means, then an explanation may be needed of how progress in science differs from progress in any other kind of labor. Such an explanation is especially welcome in light of the widespread belief (among philosophers of science, at least) that science is exceptionally progressive relative to other kinds of *intellectual* work.

Lefèvre provides a partial answer. It is that science transforms the realization of potential knowledge inherent in tools into a systematically performed social enterprise. The free exploration of the possibilities tools present is ruled out in ordinary labor, due to the utilitarian aims and economic considerations that impose narrow limits on how the means are employed. In contrast, “free exploration constitutes the core of science.” Thus, the fundamental difference between science and other forms of labor would be that free exploration of the uses of tools is systematically performed in the former but not in the latter (Lefèvre 2005, p. 218).

A second question is what role the specific products, resulting from the application of scientific means, play in the acquisition of new knowledge. Lefèvre focuses on how the discovery of new uses or ways of using the material means leads to new knowledge. He provides the example of Greek geometry:

> It is not the nature of the means themselves but their *use* for purposes of cognition that renders them scientific means. To give an example: The inventors of Greek geometry did not invent the compass and ruler on which this geometry essentially rests. Living in a society that used these instruments in several practical domains, they rendered them scientific instruments by making a specific use of them. They employed them not to design the ground plan of a temple or for another practical goal, but to gain insight in the regularities of constructions that can be accomplished by compass and ruler. (Lefèvre 2005, p. 218)

The geometer, like the architect, may use a compass to draw a circle, but instead of using the circle to design a temple, the former thinks abstractly about the properties of circles. On this view, the product of tool use is a secondary matter; what counts is the use that is made of it. In the end, it seems that what differentiates scientists from other workers is that the former think abstractly about the results of tool use, whereas the latter think with regard to practical purposes.

This difference helps explain how science began. In modern science, however, instruments of science are constructed and employed specifically because of the particular form and content of the knowledge they yield about the world, not only to explore what happens when they are used in new ways. My discussion of discovery and integration aimed to address this additional function by shifting the focus onto the products of scientific tool use and how scientists use these to transform their own practice.

To conclude this section, I think the evolutionary views of the history of science discussed at the beginning of the section can yield valuable insights into processes of innovation and the transmission of ideas in science. But they run the risk of reductionism, insofar as they evacuate features of science that are specific to human evolution. In particular, an evolutionary approach must take into account how humans' relationship to nature is mediated. Historical materialists have aimed to correct reductionist distortion by studying the ways in which labor, as a specifically human activity, mediates that relationship. Labor gives human evolution the character of expanded reproduction. The latter puts humans on a different developmental trajectory than other species, for the changing environment of use-values alters the nature of the labor process, and hence the skills and abilities that can be marshalled therein. My suggestion in this paper has been that this process is also at work in the history of science.

7.5 Conclusion

According to Chang (2007), "[s]cientific progress remains one of the most significant issues in the philosophy of science today. This is not only because of the intrinsic importance of the topic, but also because of its immense difficulty. In what sense exactly does science make progress, and how is it that scientists are apparently able to achieve it better than people in other realms of human intellectual endeavour? Neither philosophers nor scientists themselves have been able to answer these questions to general satisfaction." The approach taken in this paper has been to compare science to ordinary labor rather than to other intellectual endeavors like art, religion, philosophy, morality or politics, as is usually done.[469] Though I by no means purport to have provided a general answer to the questions Chang raises, I claim to have offered grounds for thinking that one of the mechanisms by which science makes progress is similar to how ordinary labor makes progress. In addition to the question of fit with the history of science, future development of the view outlined here should answer the question of why and by means of what social mechanisms scientists engage in the processes described in sections 2 and 3. The approach taken here also suggests a difference with the other realms of intellectual endeavor to which science is usually compared. None of these has a comparable cycle of discovery and embodiment to power its growth. None of these has a similar ability to transcend the limits of native human abilities.

7.6 References

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[469] The list in the text is from Niiniluoto (2015). Sarton (1927), pp. 3-4 contrasts science to religion, art, and social justice; Kuhn (1996 [1962]), p. 160 to art, political theory, and philosophy; Resnik (2000), p. 253 to literature, philosophy, law, religion, and music; Okasha (2002), p. 1 to art, music, theology, history, astrology, and fortune-telling; and Smith (2010), p. 574 to "other areas of inquiry."

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8.0 CONCLUSION

8.1 Know-how, progress and rationalization

Appreciating the role of abilities in science sheds light on the dynamics of knowledge accumulation. The conditions of action determine the abilities an agent can exercise. Thus my physical make-up enables me to jump two feet vertically, but not four. Galileo's telescope enabled him to observe the moon and other objects in the solar system, but not objects outside it. As these examples illustrate, abilities depend crucially on the means available for carrying out actions. The means available allow agents to expand their abilities, and hence their horizon of possible actions. By the same token, the available means also limit the abilities an agent can have, by constraining the range of actions that are possible for the agent in a given context. It follows that dependence on the means available can impede the acquisition of abilities, if the means don't permit the performance of certain actions.

But abilities do not depend solely on the means available. They also depend on the specific features of the activity in which they are exercised. For this reason, this dissertation has focused not just on scientific instruments, but on the nature of the scientific labor process. The labor process involves not just means of labor and the worker's individual abilities, but other features like division of labor and workers' attitudes and beliefs about their work. Ultimately, what the workers are able to do depends on all of these aspects of work.

It follows from these considerations that the acquisition of epistemic abilities can be impeded by these features of scientific work, if they block the performance of certain actions. In other words, and as suggested in the conclusion to chapter 5, there can be ideological and material impediments to the acquisition of abilities in science. If we want to be able to answer the question posed in chapter 1, why is it possible for scientists at a given time to have more epistemic abilities than scientists at an earlier time?, then identifying these impediments, and the ways in which they were or were not overcome, is a necessary step.[470]

In chapter 3, it was argued that the emergence of the modern scientific method involved certain breaks with previous ways of conceiving the relation between science, labor and instruments. In the discussion of the Scholastics, it was shown that the incorporation of instrumentation and experimentation as fundamental components of the scientific method was not trivial because it was discouraged by dominant attitudes towards manual labor, the distinction between theoretical and productive sciences, and by beliefs about the proper object of scientific inquiry. These attitudes prevented the emergence of a full-blown experimental science. In the study of Kepler's optics, it was argued that the pre-Keplerian conceptualization of sense experience tended to see a big difference between how the senses (or at least vision) work and how mechanical instruments work. This conceptualization entailed criteria of certainty and error that are inappropriate for an instrumental, measurement-based science. In chapters 4 and 5, it was argued that the progress of early 20th century chemistry was impeded by two historically specific obstacles. One was the insular nature of chemical methods and know-how, the dependence on chemical reactions to solve analytical problems. The other was the cognitive correlate of this dependence, the use of structure-to-structure reasoning to make analytical claims.

The overcoming of these obstacles led to progress in the form of methodological knowledge. The progress discussed in chapter 3 was of two kinds. First was the emergence of a full-blown experimental approach, unfettered by previous strictures on what constitutes the proper methods and objects of science. Second were improvements in methods of measurement and, in the long run, the improvement of scientists' ability to study nature through technological change. In chapters 4 and 5, the progress resulted from an increase in the rate of methodological innovation in chemistry. The progress consisted

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[470] Recall that by 'impediment' or 'obstacle,' I intend a feature of the circumstances in which scientists are working whose elimination or overcoming is a necessary precondition for specific further changes. The idea is that earlier developments produce the necessary preconditions for later ones, in this case by overcoming such features of the circumstances. Thus the notion of an impediment highlights a form of dependency of later developments on earlier ones.

in part in improvements in the efficiency of compound identification. Moreover, the power of analytical methods soared, thereby creating opportunities for new kinds of research.

Each of the cases has been concerned with the ability, or inability, of a scientific field to acquire certain kinds of knowledge. In each case, it was found that there were certain obstacles that impeded the acquisition of this ability. Thus, it was found that the Scholastics were unable to engage in full-blown experimental work, and hence to acquire experimental EK, due to their allegiance to the social prejudice against manual labor and the distinction between theoretical and productive science. The Perspectivists granted an epistemic privilege to ocular observation that discouraged the exploitation of artificial means of observation, and hence to acquire instrumental EK. The classical chemists were unable to overcome the indirect character of chemical analysis, the fact that structure was destroyed in order to determine structure, due to their dependence on chemical reactions. Though they were able to acquire some knowledge of structures and their chemical relations, and hence chemical TK and EK, the methods for doing so were inefficient and precluded certain other kinds of structural knowledge.

I submit that focusing on the negative role of the means of scientific work reveals a mechanism of progress at work in modern science by which such impediments can be overcome. I will call this mechanism ‘rationalization.’ Rationalization, as I use the term here, is the criticism and transformation of the means of scientific work in light of the available scientific and technological knowledge. As noted in the conclusion to chapter 5, scientists do not always, and often cannot, develop techniques and instruments out of whole cloth to fit novel research situations. Instead, they use the means available to them until better means are developed. The old means have a positive effect, insofar as they allow research to proceed in the novel situation, but they can also have a negative effect, insofar as they are not optimal for the changed circumstances. But they can be *rationalized* by criticizing, reconstructing and transforming them into more suitable means in light of the available scientific and technological knowledge. Descartes’ response to the telescope provides a simple and well-known example of rationalization:

> The telescope originated outside the theoretical framework of optics as a more or less chance discovery in the framework of artisan production of *mirabilia*. Therefore, right at the beginning of *La dioptrique* Descartes too writes that, to the shame of our sciences (*à la honte de nos sciences*), this invention was made on the basis of experience and good luck. According to Philippe

> Hamou, the fact that the telescope was discovered by accident was scandalous for Descartes; he therefore considered it necessary to incorporate the telescope into the order of reason, that is, to deduce it from mathematical optics. Coincidence had to be denied and it was therefore necessary to discover the telescope again – this time according to reason with the help of the proper method and on the basis of understanding the laws of optics. Descartes even believed that based on the methodical application of the experience of craftsmen it would be possible to perfect telescopes and microscopes. In connection with this, in *La Dioptrique* he introduced a new method of grinding lenses.[471]

The telescope was initially borrowed by scientists from the artisans, who produced it for non-scientific purposes using non-scientific methods. Descartes took a step towards rationalizing its use and production by showing how its properties could be deduced from optical theory. He then took a second by showing how better telescopes could be produced by the joint application of theory and lens-making technology.[472]

Thus, to rationalize a science is to criticize and reconstruct its material practices as suitable means to its ends in light of the available scientific and technological knowledge.

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[471] Spelda (2017), p. 11. The passage in the *Dioptrique* to which Spelda alludes reads as follows: *Mais, a la honte de nos ſciences, cete inuention, ſi vtile & ſi admirable, n'a premierement eſté trouuée que par l'experience & la fortune ... Et c'eſt ſeulement sur ce patron* [the model provided by the original telescope] *que toutes les autres qu'on a veües depuis on eſté faites, ſans que perſonne encore, que ie ſçache, ait ſuffiſanment determiné les figures que ces verres doiuent auoir. Car, bien qu'il y ait eu depuis quantité de bons eſprits, qui ont fort cultiué cete matiere, & ont trouué a ſon occaſion plusieurs choses en l'Optique, qui valent mieux que ce que nous en auoient laiſſé les anciens, toutefois, a cauſe que les inuentions vn peu malayſées n'arriuent pas a leur dernier degré de perfection du premier coup, il est encore demeuré aſſés de difficultés en celle cy, pour me donner suiet d'en eſcrire. Et d'autant que l'execution des choſes que ie diray, doit dependre de l'induſtrie des artiſans, qui pour l'ordinaire n'ont point eſtudié, ie taſcheray de me rendre intelligible a tout le monde, & de ne rien omettre, ny ſuppoſer, qu'on doiue auoir appris des autres ſciences. C'est pourquoy ie commenceray par l'explication de la lumiere & de ſes rayons ; puis, ayant fait vne brieue description des parties de l'œil, ie diray particulierement en quelle ſorte ſe fait la viſion ; & en ſuite, ayant remarqué toutes les choſes qui ſont capables de la rendre plus parfaite , i'enſeigneray comment elles y peuuent eſtre adiouſtées par les inuentions que ie deſcriray.* (Descartes 1996 [1637], pp. 81-83.)

[472] Kepler was actually the first scientist to offer an explicit theoretical analysis of lenses and, on that foundation, of the telescope in his *Dioptrice* of 1611. According to A. Mark Smith, both theoretical and practical knowledge contributed to the development of the microscope and telescope, but practical knowledge led the way (Smith 2015, pp. 381-391).

In general, rationalizing involves two components. There is a theoretical component, which is the explanation of a practice or instrument in scientific terms. But there is also a practical component, which involves activities like constructing, modifying, producing, optimizing and transforming. This practical component was the focus of chapter 7, where I called it 'embodiment.'

The telescope case is misleading to the extent that it suggests rationalizing merely involves explaining a practice scientifically.[473] The more interesting, and common, case involves a transformation of the old practice. A fairly radical example of this was given in chapter 4. There, the emphasis was on the role of machines in rationalizing chemical analysis. The basic idea was that machines allow for greater rationalization than human-centered methods because the latter's reliance on human intervention present an obstacle to the application of science and technology to the improvement of those methods. Human-centered methods present an obstacle to the application of science and technology in three ways already discussed in chapters 2 and 7. First, although individual expertise is necessarily limited, we can use an instrument without knowing all the things necessary to make it. The "black-box-ability" of the instrument allows a much greater amount of knowledge to be brought to bear in research than would otherwise be possible. Second, the extent to which science and technology can be applied is limited by the plasticity of the object to which they are to be applied. Human plasticity is constrained fundamentally by human biology. In contrast, the plasticity of instruments is, in principle, only constrained by the laws of nature, though in practice it must be adapted to human users. Third, the durability of instruments allows the knowledge they represent to be used well beyond the activities of any particular knowing subjects.

We identified a kind of episode of scientific change in which the introduction of machines removes this obstacle. The machine case differs from the telescope case, however, in that the former made possible an entirely different way of producing the knowledge sought. One might also interpret Kepler's optical theory as a way of rationalizing the role of the eye in scientific observation. Scientific ocular observation

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[473] Van Helden (1983) discusses the efforts Galileo went to in order to construct a research grade telescope.

requires skill and training. But ocular observation itself is not a specifically scientific activity, no more than the eye is a specifically scientific instrument. The eye's use for scientific observation arose simply because it was the default means of observing. But there is no essential reason why it must continue to be so used, once scientific and technological progress show that other means can either do the same job (perhaps better) or permit observation of things beyond the eye's reach. I argued in chapter 3 that by showing the eye to be no different from a mechanical instrument in its operation, Kepler contributed to the criticism, reconstruction and transformation of scientific observation.

In terms of the categories introduced in chapter 2, rationalization leads to methodological and instrumental progress, where progress is constituted by the accumulation of either methodological or instrumental knowledge. The reader may recall how these forms of knowledge were characterized:

> (MK) *Methodological Knowledge*: Methodological knowledge usually comes in the form of methods and techniques of learning about nature.
>
> (IK) *Instrumental Knowledge*. Instrumental knowledge usually comes in the form of instruments or techniques for carrying out operations or actions.

Thus, knowing how to produce an instrument for carrying out some action constitutes a form of know-how. If a new kind of instrument has more capabilities than one that it replaces, then there is instrumental progress. If the action the instrument helps to carry out is part of a method for learning about nature, then the instrumental knowledge also contributes to methodological knowledge. If the MK to which the instrument contributes is an advance over prior knowledge of how to study nature, than there is methodological progress as well.

For example, the production of the first telescopes constituted an item of instrumental knowledge, knowledge of how to observe distant objects or even of how to correct defective vision.[474] Since the eye-telescope system had more capabilities than the

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[474] In what historian Albert van Helden (1977) identified as the earliest "undeniable mention of a telescope," the Dutch spectacle-maker Hans Lipperhey, in a letter to Count Maurice of Nassau, stated that he wanted to present the Count with "a certain device by means of which all things at a very great distance can be seen as if they were nearby." On the use of the spyglass to correct defective vision, see Van Helden (1977), p. 24.

naked eye, the use of this system rather than the naked eye for long-distance observation also constituted instrumental progress. When Galileo adopted and adapted the telescope for studying the heavens, the instrument also became a component of MK. When Galileo, Descartes, Kepler and others explained and modified the workings of the telescope in order to improve its capabilities, there was further instrumental progress. But there was also methodological progress, as this effort increased our knowledge of how to study nature.[475]

As explained in more detail in chapter 7, rationalization both leads to more knowledge but also draws on prior knowledge, of all five forms introduced in chapter 2: TK, EK, MK, PK, and IK. Rationalization also ties together the two guiding questions posed in chapter 1. Question (2) asked, how is it possible for knowledge acquired in the past to be used in on-going or future research? The answer suggested here is that this is possible because scientists rationalize their practices by incorporating prior knowledge into them. Question (1) asked, why is it possible for scientists at a given time to have more epistemic abilities than scientists at an earlier time? Answer: Because they have thus rationalized their practices, and in doing so they have enhanced their ability to engage in a mental or physical action (or set of actions) that is intended to contribute to the production or improvement of knowledge.

Why does any of this matter for philosophy of science? Traditionally, philosophers have focused on the nature or acquisition of TK, and to a lesser extent EK. I contend that if we want to understand how and why science changes, the traditional focus on TK and EK is insufficient. On the picture of science that has emerged in this dissertation, the motor of scientific change—over the long-term—is not the accumulation of TK or EK but rather the use of the five forms of knowledge to make progress in know-how, MK and IK. Major theoretical advances often come in the wake of much change in know-how. In addition to the arguments already advanced in this dissertation, I provide empirical support for this observation in the appendix to this section.

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[475] Van Helden considers this improvement of the spyglass into a telescope, for the purposes of studying nature, to be what transformed it into a scientific instrument (van Helden 1977, p. 26).

The process of rationalization is a cause of certain phenomena of scientific change that have attracted the interest of philosophers. Here I will briefly discuss observability, demarcation and scientific revolutions.

8.1.1 Observability

Debates over what we can observe tend to revolve around the question whether or not the human senses play an essential role in observation. This question acquires its relevance largely as a result of the modification of the means of observation used by scientists. Starting from the human senses, over time scientists have developed an immense array of means for observing, thickly mediated by the knowledge acquired over the centuries. This process has in turn allowed the occurrence of highly mediated relationships between humans and the objects of observation, raising the question whether 'observation' is still an appropriate term for this relationship, and if so, how it should be understood.[476]

This mediation raises further issues, such as realism, the theory-ladenness of observation, and the validity of empiricism as a theory of knowledge. The theory-ladenness of observation is, of course, related to issues of theory choice and scientific revolutions. Though these issues all pose problems for the status of the TK afforded by science, it is worth noting that they are all based on considerable achievements in observational know-how, namely observational MK and IK. This suggests a two-track picture of science, briefly discussed in section 2.2.2., according to which the dynamics of theoretical progress differ from those of know-how progress, with revolutions in one dimension not necessarily entailing revolutions in the other.

8.1.2 Scientific revolutions

Put in the terms of this chapter, the overall conclusion of chapter 4 was that rationalization is a direct cause of scientific revolutions. This conclusion is to be contrasted

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[476] For classic discussions of this issue, see van Fraassen (1980) and Shapere (1982).

with the usual scenario of revolutions resulting from the competition of rival theories. Rationalization changes the elements constituting the material practices of a science. I will call this effect ‘reconstitution,’ meaning that the change can affect fundamentally the basic methods, problems, goals, values, theories and even the ontology of the field (though not necessarily all of these at once). Three cases of reconstitution were discussed in the dissertation.

First, in the case of the Scholastics, the natural philosophers’ relationship to labor determined whether the science was experimental or not. When they abandoned certain ideas about manual labor and the proper object of science, they were able to adopt the experimental method into the core of their activities.[477]

Second, Kepler contributed to the rationalizing of scientific observation by showing the eye to be no different from a mechanical instrument in its operation. In doing so, he changed the problem the theory of the eye was a solution to, from the problem of what guarantees veridical perception to the problem of how to achieve accurate measurements by means of vision. Kepler showed that these conceptual changes had ramifications for how astronomical measurements should be performed.

Third, during the Instrumental Revolution, analytical chemists had to decide whether “analysis [is] primarily a chemical process of separation and identification, or a

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[477] One might question whether this episode was really an example of rationalization in my sense of “the criticism and transformation of the means of scientific work in light of the available scientific and technological knowledge,” since ideas about labor and the proper object of science are not means, and moreover are the province of ideology and philosophy rather than scientific and technological knowledge. On the other hand, one might interpret the critique of the methods of natural philosophy in the works of Bacon, Descartes, Paracelsus and other scientific revolutionaries, together with their admiration for the technological successes of the crafts, as rationalization in a broader sense of the term, since it was in part a critique of a *way* of acquiring knowledge of nature. Moreover, some of the knowledge produced by the crafts entered directly into the processes leading to a rupture with Aristotelian natural science. This knowledge included not only the experimental know-how discussed in section 3.2.5, but also empirical knowledge. For example, the identification of projectile trajectories in gunnery provided *explananda* that challenged Aristotelian physics and thereby contributed to the advent of classical mechanics (Schemmel 2008).

physical process of direct identification."[478] Organic chemists had to decide whether the goal of structure determination was to determine the network of chemical relations of a substance, or the structure of a molecular species. Was the object of analysis a substance, or a molecule? Were chemical problems to be solved by chemical methods, or by a "science of instrumentation," and hence in part by the methods of other fields? What skills and knowledge should chemists possess? How should they be trained?

Other, more general, constitutional questions were also raised. Is science a craft, and therefore fit only for specialists to do? What organization of labor is best for scientific progress? What should be the relative importance of exploratory versus confirmatory experimentation in the field? What is the value of automation versus direct human involvement in scientific work? What is the value of mechanical objectivity relative to the contributions of the subject?[479] What values are supported by changes in scientific agency? For example, is automation merely more convenient than manual methods, or does it promote scientific values of accuracy, sensitivity, objectivity, etc.?

The examples in this dissertation show that the rationalization of material practices can create discontinuity in the history of a science. This recognition suggests a research agenda for historical epistemology: 1) look for significant change in the material practices of a science, 2) determine whether and how there were fundamental changes in the basic methods, problems, goals, values, theories or ontology of the field, and 3) determine to what extent the changes in (1) and (2) were brought about by prior developments in science and technology.

If the overcoming of these ideological and material impediments leads to the reconstitution of the field, this raises the question of whether the change can be progressive after all, since in some cases what counts as progress will surely change too. For example, if Kepler changed the problem the theory of the eye was a solution to, then that is reason to think that progress towards solving the new problem might not be progress towards

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[478] Baird (2002), p. 40.

[479] On mechanical objectivity see Daston & Galison (2007). Young (2016) argues that understanding observational instruments in terms of mechanical objectivity has obscured the contribution of the skilled practice of the subject to scientific observation.

solving the old problem. We saw a similar situation in the chemical case: progress in structure determination after the Instrumental Revolution might not count as progress in classical chemistry, since the goals differ. On another interpretation, however, the chemical example suggests that there may also be progress through discontinuity. First, because there were multiple goals at issue, and one was retained. Second, because there was progress in the sense of an improved understanding of the meaning of structural representations, from representing mere links between atoms to representing chemical bonds and spatial properties.[480]

As with scientific revolutions involving theory choice, the rationality of science is also at stake in episodes of rationalization. Indeed, the very term suggests that science becomes more rational in such episodes. Roughly, ‘rational’ here means that the elements constituting scientific practice are not simply taken for granted, but are constructed as suitable means to its ends in light of the available scientific and technological knowledge. Rationality in this sense is not a necessary condition for the acquisition of scientific knowledge. Suitable means may arise by chance, or be borrowed from prior uses for reasons of convenience, say. But they would not be rational in the sense intended here

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[480] In his 1936 Presidential Address to the Chemical Society of London, Nevil V. Sidgwick claimed that

> our knowledge of the meaning of these structures has developed, especially in the last 20 years, to an enormous extent. We have applied to their investigation a whole series of physical methods, based on the examination of the absorption spectra in the infra-red, the visible, and the ultra-violet, and of the Raman spectra : on the measurement of specific heats and heats of combustion, of the dielectric properties, and of the scattering of X-rays and electron waves, as well as on the study of chemical dynamics : to mention only the most important. To Kekulé the links had no properties beyond that of linking; but now we know their lengths, their heats of formation, their resistance to deformation, and the electrostatic disturbance which they involve. Throughout all this work the starting point has always been the structural formula in the ordinary organic sense. There is no better example of the effect of new discoveries in giving new meaning to a theory while they leave the truth of the theory unaffected, and of the way in which modern research, instead of being content with evidence of one kind, as were the older organic chemists with that of chemical reaction, draws its material from every side, and from every branch of chemistry and physics. (Sidgwick 1936, pp. 533-534).

unless their suitability were explained and optimized in light of the available scientific and technological knowledge.

Why is rationalization rational? Because prior knowledge provides good reasons for the criticism and reconstruction of scientific practice, given the ends of the scientists. It might seem as if the rationality of the rationalization depends on the rationality of the ends. Since this is not the place to get into the nature of instrumental rationality, I will here limit myself to making two points. First, the good reasons for the criticism and reconstruction do not stem solely from the ends, but also from the prior knowledge; for example, the laws of optics provided good reasons for beliefs about the telescope's functioning and for modifications of the instrument, quite apart from whether astronomical research (say) was a good reason to invest in it. Second, the end does not determine whether the process is rationally coherent. The rational coherence of the process is determined, rather, by whether it is believed to be a means to the end. Again, the rationalization of the telescope cohered with the end of astronomical research, quite apart from whether the latter was a rational end.[481]

8.1.3 Demarcation

There are different problems associated with demarcating science from other intellectual endeavors. The classic problem of demarcating science from pseudoscience has to do with finding criteria that would distinguish legitimate science from nonscientific fields with scientific pretensions.[482] The demarcation problem I have in mind, however, involves demarcating science from other legitimate intellectual endeavors. Scientists (or at least natural scientists, who are the focus of this dissertation) seem to be able to make progress better than people in other realms of human intellectual endeavor. How does science differ from these other endeavors in such a way as to make better progress? There

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[481] On the nature of instrumental rationality, see Kolodny & Brunero (2016).

[482] Mahner (2007).

is therefore a problem of demarcating science from other intellectual fields in a way that is explanatory of the superior progress of the former.

For example, George Sarton, one of the founders of the academic discipline of history of science, wrote in his 1927 work *Introduction to the History of Science* that the purpose of the book was to explain the development of science, which he characterizes as "systematized positive knowledge." He included the qualification that "I am not prepared to say that this development is more important than any other aspect of intellectual progress, for example, than the development of religion, of art, or of social justice. But it is equally important." He goes on to contrast the progress of science to the lack of progress in religion and art, before concluding that "[*t*]*he acquisition and systematization of positive knowledge is the only human activity which is truly cumulative and progressive.*"[483] Thomas Kuhn, in his 1962 *The Structure of Scientific Revolutions*, asked

> Why should the enterprise sketched above move steadily ahead in ways that, say, art, political theory, or philosophy does not? Why is progress a perquisite reserved almost exclusively for the activities we call science? The most usual answers to that question have been denied in the body of this essay.[484]

Samir Okasha, in his 2002 introductory text *Philosophy of Science*, asks "what is science?" and then contrasts it with art, music, theology, history, astrology, and fortune-telling.[485] Smith (2010) contrasts science with "other areas of inquiry."[486] Once these contrasts have been made, the game is then to identify the specific differences that distinguish science from these other endeavors and that explain its distinctive progress.

As is evident from these examples, some historians and philosophers of science not only believe that scientific progress is superior to fields that, many would agree, are pseudo-scientific (astrology, fortune-telling), but also that it is superior to that of legitimate intellectual fields like art, philosophy or political theory. This belief raises some questions.

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[483] Sarton (1927), pp. 3-4.

[484] Kuhn (1996 [1962]), p. 160.

[485] Okasha (2002), p. 1.

[486] Smith (2010), p. 574. In addition, Niiniluoto (2015) contrasts science with art, religion, philosophy, morality, and politics, and Resnik (2000) with literature, philosophy, law, religion, and music.

First, is it true? Just as we know much more about the natural world than we did at the beginning of the modern era, so we know much more about human history than we did then too. Our repertoire of artistic techniques has grown also. Some of these other fields seem to make good progress, so in what sense is their progress inferior to that of the natural sciences? Second, does the belief even make sense? It is not obvious that, say, physics and art have enough in common to make the progress of one comparable to the progress of the other.

For my purposes, it is sufficient that scientific progress be distinctive in some way(s) from that of these other fields, not that it be superior. That said, I will now note that the assumption shared by these authors is that the appropriate genus for the selection of a contrast class is that of *intellectual and creative activities*: science is explained *qua* intellectual and creative activity. Setting the genus this way, however, abstracts from a striking feature of natural science: unlike these other endeavors, it is based on a specific relation to nature—it is epistemically driven by instrumentally mediated causal interaction with nature. In this dissertation, I have been trying to relate science to a different kind of endeavor, one that is also based on instrumentally mediated causal interaction with nature: ordinary material labor.

Thus, my answer to the question of how science differs from other intellectual fields is that it is a special form of material labor. As such, it makes progress differently because of its instrumentally-mediated relationship to nature. This relationship allows the incorporation of what science learns into its methods and tools, since these are a part of nature as well. How does this fact about how science makes progress differentiate science from other intellectual activities, like art, history, philosophy, law, etc.? Certainly, all these fields have a cumulative aspect, in that current workers build—in some sense—on prior work. Philosophical theorizing is largely driven by criticism of earlier theories. Case-law builds on the precedents of prior cases. Historians base their research on the facts and interpretations provided by earlier historians. Based on what has been said above, an obvious answer is that none of these other activities is based on causal interaction with nature.[487] Scientific instruments and techniques embody causal knowledge in order to interact causally with nature, which interaction yields more causal knowledge.[488] This surplus of causal knowledge can then be used to develop new instruments and techniques, which may again yield new causal knowledge (see chapter 7).

Importantly, causal knowledge includes both knowledge-that and know-how. This is important because in practice, mere knowledge-that is insufficient to make instruments and apply techniques. The combination of the two is needed, a requirement illustrated in the account of Descartes' response to the telescope quoted above. Though the author at first claims that Descartes sought to deduce the whole instrument from mathematical optics, he finishes on a very Zilselian note, "the methodical application of the experience of craftsmen:" Descartes believed that the joint application of theory, systematic research and craft know-how were necessary to improve the telescope and microscope.

This way of demarcating science from other intellectual fields, as a synthesis of manual and intellectual labor may be contrasted with more obvious ways of trying to demarcate it. For example, science may be viewed as the attempt to understand, explain and predict the world we live in; or as making use of distinctive methods of inquiry; or of constructing theories with special properties (e.g., falsifiability). My view bears some resemblance to the distinctive-methods criterion. The former, however, is focused not so much on the precise methods as on the kind of knowledge used and produced in the application of the methods and how the knowledge produced gets cycled back into research.

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[487] Art may be a counter-example, since it centrally involves causal interaction with natural materials, e.g. a sculptor shaping a quantity of marble. On the other hand, given that Aristotle categorized the arts under the productive sciences (see section 3.2), perhaps the apparent counter-example just means that art, like science, should also be related to ordinary material labor.

[488] Zilsel emphasizes causal "research," "spirit" or "thinking" as one of the contributions of the superior craftsmen to scientific method, though he does not mention causal knowledge or know-how explicitly. His emphasis on causal research might be a point on which he does not do justice to the scholars' contributions; as discussed in section 3.2 of the dissertation, some of the late 16th and early 17th century Scholastics were quite interested in causal analysis.

Moreover, the view of natural science as a synthesis of manual and intellectual labor suggests a historical approach to the demarcation problem. The usual approach is what might be called a “criterial” approach, which aims to identify common features that all sciences possess and that distinguish them from non-sciences. This criterial approach is very difficult, given the heterogeneous nature of the activities that go by the name of science. The approach suggested by the view of natural science as a synthesis of manual and intellectual labor is that the natural sciences are *self-demarcating*. Precisely because natural science can materially incorporate what it learns into its work, it creates a context of tools and techniques that are themselves “scientific” in the sense of embodying scientific knowledge (both knowledge-that and know-how). Descartes’ response to the telescope is paradigmatic: he aimed to transform a non-scientific instrument into a scientific one, by explaining its functioning in terms of scientific laws and modifying its construction in light of those laws and by applying a systematic approach. The approach suggested here is historical because it views the distinction between science and other intellectual endeavors as the outcome of a process in which science gradually distinguishes itself through its reflexive character (the incorporation of scientific knowledge into its tools and techniques). It should be noted that this process is driven by advances in MK and IK, and hence know-how.

The historical approach transforms the demarcation problem. Rather than ask, how are the natural sciences different from other intellectual endeavors? the historical approach asks, how did the natural sciences *become* different from other intellectual approaches? Such a process might start with the practice of a pre-scientific activity aimed at systematized knowledge of nature, e.g. Scholastic and Renaissance natural philosophy. With the discovery of the potential of instruments both for dramatically extending the range of experience and also for continual improvement in light of prior knowledge, and with the erosion of social barriers described by Zilsel, the practitioners of the pre-scientific activity start systematically using and developing instruments and techniques to acquire knowledge of nature. They also start systematically using the knowledge acquired to develop instruments and techniques. In doing so, the practice has acquired a mechanism of progress that distinguishes it, and increasingly differentiates it, from the old practice and from those practitioners who continue to do the old natural philosophy.

This mechanism also differentiates natural science from other scholarly pursuits like philosophy, literature, or theology that do not make progress in the same way. It also differentiates natural science from manual labor, for the latter, once combined with scholarly capacities à la Zilsel, received a new goal: knowledge rather than the production of useful objects.

8.1.4 Concluding remarks

Focusing on the evolution of scientific abilities forced us to broaden our conceptions of knowledge and progress. It also forced us to think about the conditions of scientific action, and hence about science as a form of labor. Thinking about science as labor suggests that the, or at least, *a* motor of long-term scientific change is the use of prior knowledge to make progress in scientific know-how. This process is at the root of phenomena that give rise to philosophical issues concerning what can be observed, the nature and causes of scientific revolutions, and the specific difference of science from other forms of intellectual work. On traditional proposition-based analyses of science, each of these issues is construed in terms of a problematic relation involving theory and something else: for observability, whether instrumental data allow us to observe the entities referred to by theoretical terms; for scientific revolutions, whether the choice between rival theories can be rational; and for demarcation, whether adequate criteria can be found for distinguishing between scientific theories and pseudo-scientific ones. These questions essentially derive from the fundamental question of how to determine which beliefs are warranted. This question has much to do with scientific progress, but it is progress restricted to propositional knowledge, TK to be exact.

If we take seriously the idea that know-how is a genuine form of scientific knowledge, however, then these issues need to be deconstructed and reconstructed to see what they look like when we describe them in terms of know-how, abilities, action and labor. As noted in the previous section, the demarcation problem looks different, when we focus on how MK and IK are accumulated, from when we focus on static demarcation criteria. Though I am not in a position to do this here, in the future it might be interesting to investigate whether so-called pseudo-sciences can claim a similar track-record of accumulation.

For another example, of the cases studied in this dissertation, Kepler's is the closest to the standard cases discussed in philosophical debates over scientific revolutions. From one point of view, it was indeed a situation of theory choice between rival theories of vision, where one can ask the usual questions about rationality, incommensurability, relativism etc. concerning scientific revolutions. For example, one way to get incommensurability into the picture is to focus on the standards of theory acceptance. For the Perspectivists, it was important that a theory of vision account for how the picture formed in the eye is perceived and judged by the cognitive faculties. Kepler, famously, provides no such account. In chapter 3, I argued that this is not a problem on Kepler's own terms because he viewed the eye as a component in a measurement process and was thus not trying to provide a theory of ordinary observation at all. So perhaps this was a case of methodological incommensurability, since the rival theorists were assuming different standards of theory acceptance.[489] This is a problem for assessing theoretical progress.

On the other hand, once vision was viewed as a component in a measurement process, then this recognition suggested new ways of eliminating experimental error and otherwise rationalizing the measurement process. Kepler was thereby able to rectify many errors that had bedeviled astronomical observation in the past. This constituted progress in astronomical MK, despite the potential incommensurability in the TK dimension.

The sketches in this and the preceding three subsections are intended to show that science looks quite different from the proposition-based view when described from the point of view adopted in this dissertation, though clearly much more work would need to be done to determine what difference this makes to traditional philosophical debates.

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[489] 'Methodological incommensurability' is a form of incommensurability Howard Sankey has identified in Kuhn's writings on the topic. According to Sankey (2013), p. 34, methodological incommensurability "rests on the assumption that there are no fixed or independent standards to which appeal may be made in the comparison of alternative theories. Instead, standards of theory appraisal depend upon and vary with theory or paradigm. Competing theories may therefore be incommensurable in the methodological sense because there are no shared or neutral standards on the basis of which choice between such theories may be made."

8.2 Directions for future research

This dissertation has been concerned with relating traditional philosophical concerns about the nature of scientific change and progress with detailed studies of changes at the level of scientific agency and the scientific labor process. The emphasis on agency and labor suggests avenues of future research.

8.2.1 The demarcation problem and scientific progress

One of the main claims of the dissertation is that knowledge in the form of abilities is both a major cause, and constituent, of scientific progress. A major future project that I would like to undertake is to examine how this picture of scientific progress affects traditional issues in the philosophy of science. A promising target in this regard is the problem of demarcating science from pseudo- or non-science. Traditionally, the problem has been posed as one of finding a criterion that would demarcate science from pseudo-science. Another interesting version of the problem, however, is what makes scientific progress different from progress in other non-scientific but genuine intellectual endeavors (like philosophy, law, music, political theory, etc.). The usual approach is what might be called a "criterial" approach, which aims to identify common features that all sciences possess and that distinguish them from non-sciences. This criterial approach is very difficult, given the heterogeneous nature of the activities that go by the name of science. Since the model of scientific progress I have proposed is quite different from traditional philosophical models of science insofar as it emphasizes common features of scientific work and ordinary material labor, it will be interesting to determine what answers it can provide for demarcating science from pseudo- and non-science. Other intellectual endeavors do not appear to have a comparable cycle of discovery and instrument construction to power their growth, nor do they have a similar ability to transcend native human abilities.

My ultimate purpose in focusing on this problem is to develop our understanding of the nature of scientific method. Because neither abilities, nor technology, have the same roles in other intellectual endeavors as they do in science, there is good reason to think that the perspective of my dissertation will refresh this classic issue.

8.2.2 Is empiricism anthropocentric?

The traditional answer is "obviously yes," insofar as empiricism amounts to the view that "all knowledge rests on experience," where "experience" is understood as human sense-experience. Nevertheless, this view has come under attack in recent decades, as philosophers of science grapple with the spectacular success of science in studying phenomena inaccessible to the human senses. One focus of the debate has been on what aspects of empiricism are still valid, given the fact that our access to such phenomena is heavily mediated by instruments. Naturalistically inclined philosophers have tried to reinterpret the relevance of sense-experience by reducing it to what it has in common with instruments: causal interaction with nature.[490] Humphreys (2004) went so far as to claim that "scientific epistemology is no longer human epistemology."[491]

In assessing such claims, a question one can ask is, how do humans participate in science? Humans and instruments have 'epistemic roles,' by which I intend the activities carried out by the agent insofar as they contribute to the acquisition of knowledge. Thus, explaining, detecting, manipulating, causing, understanding, and interpreting are scientific activities (among many others, of course), and it is the role of the agents and instruments in scientific work to carry them out. Naturalist views argue that one after the other of these roles is being taken over by scientific technology, and that humans are not essential to production of scientific knowledge. The perspective of this dissertation, however, suggests

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[490] For example, see Brown (1987), Delehanty (2005), and Shapere (1982).

[491] Humphreys (2004), p. 9.

that these roles are in constant evolution. This raises the question of whether the apparent elimination of the human element should not rather be interpreted as a transformation of humans' epistemic roles, and what difference the transformation makes to the nature of the knowledge produced. I have made preliminary arguments towards answering these latter questions in a manuscript, "Scientific agency and the conceptual dynamics of science."

An opposing tendency is to claim that humans continue to play an essential role in science, and that scientific technology merely provides the causal background conditions for human actions. For example, van Fraassen (1980) famously construes observation as an essentially human act, as does Goldberg (2012) with testimony (including testimony relying on instruments).[492] From the perspective of my dissertation research, one avenue for research is whether the more dynamic view of scientific agency adopted there might provide a more accurate understanding, than these essentializing views, of how knowledge is produced, and of the nature of that knowledge, in contemporary science. I have written preliminary arguments concerning the role of humans in science in two manuscripts, "Measurement, representation and the scientific concept of observation" and "Belief-forming processes and instrument-based testimony."

8.2.3 Social epistemology

In recent decades, there has been increasing interest among philosophers of science in the social practices of scientists and the epistemic effects of these practices. One manifestation of this is a growing literature on what Philip Kitcher (1990) called 'the division of cognitive labor.'[493] This term reflects the fact that scientific research as a whole is organized according to a division of labor, and that this division seems to have something very important to do with scientific progress. Questions philosophers have been interested in is how this division is effected, how it contributes to scientific progress, and how the

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[492] van Fraassen (1980); Goldberg (2012), pp. 181-197.

[493] Kitcher (1990), pp. 5-22.

former ought to be effected to maximize the latter.[494] The focus of this literature has been on mechanisms of coordination between scientists, which involve things like reward systems, and how these mechanisms affect the allocation of research projects and the selection of research strategies. This dissertation, in contrast, has been focused on human-instrument relations, especially what epistemic roles are played by humans and instruments in scientific work and what effect these roles have on scientific change and progress. Another focus has been on changes in the organization of the work, and the locus of expertise, resulting from changes in human-instrument relations. Though the dissertation has focused on specific case studies, it might be worth exploring more general questions concerning these issues. For example, how does the distribution of instrumentation, and the capital needed to acquire it, across projects affect scientific progress, both its pace and its nature? Some philosophers have claimed that an entirely mechanized science is possible and desirable (e.g., Humphreys 2004). What exactly are the benefits of direct human involvement in scientific work?

The changing forms of epistemic agency, and the phenomenon of mechanization in science, together raise questions about the grounds for assurance in scientific technology. As the latter has become more sophisticated and complex, the form of agency needed to use, construct and understand it has also changed from an individual agent to a collective agent. This was one of the morals of the analysis of the Instrumental Revolution, and is besides widely recognized in history and philosophy of science. This trend has consequences for assurance in science, especially for whether laypeople can be justifiedly assured in the claims of scientists. First, the individual scientist may be ill-suited to be the guarantor of the reliability her instruments. A better guarantor may be the scientific community whose combined expertise has enabled the production, testing and understanding of the instrument. Second, the assurance that the community can offer at any given time is the result of a spatially and temporally extended process in which the instrument has been studied and checked by members of the community. So one question for further investigation is whether collective scrutiny is indeed necessary to provide

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[494] See Thicke (2016), ch. 3 for a recent critical review of these efforts.

assurance, or whether other methods may be capable of doing so.[495] This question is interesting at least in part because if the first disjunct were true, this would amount to the demand that the technology be made as transparent as possible. Transparency, however, may conflict with other social practices, like trade secrecy and intellectual property rights. So one avenue for further research would be to investigate to what extent the collective character of the assurance required for contemporary technology, if it is indeed required, can be reconciled with social practices that are resistant to “collectivization.” Moreover, if assurance indeed requires a temporally extended and indefinite process of checking, at what point can assurance be given by the experts that the technology is reliable? I have written preliminary arguments concerning the role of assurance in contemporary science in a manuscript, “Belief-forming processes and instrument-based testimony.”

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[495] See, for example, Ince, Hatton, & Graham-Cumming (2012), pp. 485-488; Mnookin (2008), pp. 343-358.

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APPENDIX A EMPIRICAL EXAMPLES OF CHANGES IN SCIENTIFIC KNOW-HOW

Earlier, I claimed that, on the picture of science that has emerged in this dissertation, the motor of scientific change—over the long-term—is not the accumulation of TK or EK but rather the use of the five forms of knowledge to make progress in know-how, MK and IK. This picture may seem controversial, and indeed it is is. Why, one may ask, isn't the motor epochal theoretical advances, e.g., the advent of general relativity, quantum mechanics, the theory of evolution by natural selection, the law of universal gravitation, and so on and so forth? I observed that major theoretical advances often come in the wake of much change in know-how. Below, I provide empirical support for this observation, focusing especially on the history of developments in MK and IK.

Scientists tend to be more conscious than many laypeople of the role of instruments in furthering scientific progress. For example, the physicist Anthony Zeleny devoted his retiring address as vice-president of the American Association for the Advancement of Science and chairman of Section B in 1916 to the thesis that scientific progress depends on the availability of instrumentation for observation, measurement and control. In his address, titled "The Dependence of Progress in Science on the Development of Instruments," Zeleny offers many examples of technology-dependent progress, among which:

- The first thermometer was devised by Galileo and consisted of a glass bulb with an attached tube, the end of which dipped into water. This instrument was affected by changes of atmospheric pressure and had an arbitrary scale. Over several generations, these defects were overcome in a developmental process issuing in the mercury thermometer, a general and reliable means for measuring temperature. Despite this advance, the range of temperatures that could be accessed by means of the mercury thermometer remained quite limited. This limitation was overcome by developing yet other means, such as resistance thermometers, thermocouples, bolometers, radiation pyrometers, the nitrogen thermometer, the liquid air machine and the electric arc. The overall result has been to extend the range of temperatures accessible for the study of natural phenomena from about two degrees Centigrade above absolute zero to 4,000 degrees. Access to the lower limit of temperature has made possible the discovery of superconductivity, and this together with research on conductivity at high temperatures has contributed "most significant data toward the development of the theory of electric conduction."[496]

- During the period of Aristotelianism's dominance, various instruments and phenomena involving vacuums—like the bellows, the siphon, the water pump, and the fact that water is supported in a filled inverted bottle when its mouth is in water—were explained by the principle that "nature abhors a vacuum." The invention of the barometer by Torricelli in 1643 enabled Pascal to prove the falsity of this principle in 1647 and to establish the correct foundation for the theory of hydrostatics.
- Improvements in the diffraction grating and photography made possible the study of astronomical phenomena, like the Doppler shift of astronomical objects, as well as that of atomic structure.

Rescher (1978), Chapter VIII cites several reports and reflections by scientists on the importance of technological advances for the discovery of new phenomena and the study of hitherto inaccessible domains. Many of the documents cited concern physics and chemistry in the era of Big Science, where it might be expected that instrumentation plays an important role, but not all of them do. For example, he notes Planck's emphasis on the dependence of theoretical advance on the "goading" of experimental results:

> … it was the facts learned from experiments that shook and finally overthrew the classical theory. Each new idea and each new step were suggested to investigators, where it was not actually thrust upon them, as the result of measurements.

Rescher leaves out that Planck goes on to list a number of classic physics experiments from the turn of the 20th century:

> The Theory of Relativity was led up to by Michelson's experiments on optical interference, and the Quantum Theory by Lummer's, Pringsheim's, Ruben's and Kurlbaum's measurements of

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[496] Zeleny (1916), p. 188.

> the spectral distribution of energy, by Lenard's experiments on the photoelectric effect, and by Franck and Hertz's experiments on the impact of electrons.[497]

These experiments are worth examining in somewhat more detail here, since they were performed before Big Science, presumably during the heyday of low-tech benchtop experimentation. Nevertheless, instrumentation was an important facet of most of these experiments. For example, Michelson's famous experiments to detect the ether in the 1880s were not designed *de novo* by him, but rather drew on a French tradition of using refractometers to measure refractive index. Moreover, the importance of the instrument Michelson eventually used in the experiment was not limited to preparing the way for the special theory of relativity, but had many important applications beyond the original experiment.[498] That is, the instrument was significant in itself as a contribution to scientific technology, and not just for its role in a particular experiment.

Lenard's experiments demonstrating the photoelectric effect at the turn of the 20th century were carried out by means of Crookes tubes—partially evacuated electrical discharges tubes— invented by the English physicist William Crookes and others around 1869-1878.[499] Crookes tubes were developed from the earlier Geissler tubes, invented in the 1850s, with part of Crookes' contribution consisting of improvements to the pump used to establish the vacuum. The development of applications of Geissler tubes in the second half of the 19th century depended on increasingly specialized glassblowing skills and ancillary technologies, especially electricity supply and vacuum techniques. The success of Lenard's experiments depended on the higher vacuums that could be achieved starting in the 1880s, for only at those vacuums do the discharges turn into the cathode rays studied by Lenard, which follow a linear path.[500] The Franck-Hertz experiments of 1914 demonstrating the quantum nature of the atom used apparatus based on that of Lenard, though with improved pumping techniques. This improvement allowed Franck and Hertz to correct the results of previous experiments on the ionization of gases.[501]

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[497] Planck (1937), p. 56.

[498] Warwick (1998), pp. 339-340; Staley (2008), ch. 2-3.

[499] Crookes (1878); Gilman, Peck & Colby (1902), p. 470.

[500] Hessenbruch (1998), pp. 279-281; Wheaton (1978).

[501] Franck (1965 [1925]), p. 99 and 102.

Lummer, Pringsheim, Ruben and Kurlbaum were a group of experimentalists at the *Physikalisch-Technische Reichsanstalt* in Berlin at the end of the 19th century whose work provided crucial data for Planck's derivation of his eponymous radiation law, which assumed that energy is quantized. According to Mehra (2001), these researchers had to devote a considerable amount of effort to improving the techniques and instruments at their disposal in order to acquire these data:

> In the first paper on cavity radiation, Wien and Lummer had already suggested the testing of Stefan-Boltzmann's law and Wien's displacement law by the new method [of using cavities to represent blackbodies] (Wien and Lummer, 1895) … After Wien's departure for Aachen, Lummer looked for collaborators, and he received the help of Ferdinand Kurlbaum and Ernst Pringsheim for his work on blackbody radiation. In less than three years these three experimentalists improved the techniques of observation to such an extent that the problem of the measurement of Kirchhoff's function [the energy distribution of blackbody radiation] could be attacked in earnest. In performing their investigation, however, Lummer and Pringsheim made use of another experimental development which concerned the analysis of very long wavelengths. The principal contribution in that field had been made by Heinrich Rubens … in 1889 he had already begun to measure the wavelengths of invisible infrared radiation with the help of a Rowland grating and the bolometer method. During the 1890s he penetrated further into the infrared spectral region by using various techniques. Finally, in 1896 he published … a new method for measuring long wavelengths … This new method, which was later called the method of 'residual rays' … made use of the fact that that all substances reflect radiation especially strongly in the region of strong absorption, hence it was possible to isolate certain wavelengths by multiple reflection; thus, by 1898, wavelengths of 61.1 μ were reached by using a sylvine crystal as the reflecting substance. The tools were thus made ready for a fresh attack on the empirical determination of the radiation law.[502]

A further experimental contribution that "goaded" the development of quantum theory was the set of measurements by Walther Nernst and collaborators of the specific heats of materials at low temperature. These measurements showed that the specific heats of all metals deviate from the classical Dulong-Petit law at low temperatures. Einstein was able to account for the deviation theoretically by assuming that the metal atoms had quantized energies. These experiments required the construction of several new

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[502] Mehra (2001), pp. 34-35.

instruments, notably the vacuum calorimeter, an invention which has been credited with starting the field of low-temperature physics.[503]

Astronomy and astrophysics provide further examples of new instrumentation bringing in new data that force changes in theory. Hubble discovered that the universe was expanding, rather than static as had previously been thought, by measuring redshifts, which he was able to measure using spectroscopic techniques developed earlier by Huggins and Slipher, among others.[504] Adaptive optics, an imaging technique that removes the effects of atmospheric distortion, has greatly facilitated the search for exoplanets. The development of the ruby maser, which has to operate at very low temperatures, allowed Arno Penzias and Robert Wilson in 1965 to discover the cosmic microwave background radiation (CMB), which supported the big bang cosmology against the rival theory of steady-state creation, championed by Fred Hoyle.[505] Space-based observatories have also informed theory development. For example, the Cosmic Background Explorer satellite or COBE, launched in 1989, carried out measurements of the CMB. Since the latter has a temperature of 2.7 K, the instruments themselves had to be at least that cold, since otherwise their own radiation would swamp out the CMB. To keep the instrumentation cold, the COBE satellite carried a 650 liter superfluid helium cryostat. The measurements revealed that the CMB was isotropic, a result that disconfirmed non-inflationary big-bang cosmological theory. Later very precise measurements by the satellites WMAP (Wilkinson Microwave Anistropy Probe, 2001-2010) and Planck (2009-2013) revealed very slight deviations from isotropy. These deviations support inflationary theories. At the same time, the data from Planck rule out the simpler models of inflationary theory.[506]

I will now mention a few examples from outside of physics. Chaos theory is an approach to studying complex systems that makes use of nonlinear mathematical equations. It first emerged from the increasing use of computers in meteorology in the post-World War II era, when computer scientists viewed the complex, nonlinear problems of

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[503] Ruhemann & Ruhemann (1937), pp. 136-7. See Barkan (1999), ch. 9 for an account of Nernst's low-temperature experiments.

[504] Hetherington (2003a), p. 62 and (2003b), p. 381; Brand (1995), pp. 135-139.

[505] Bertolotti (1998), p. 346; Hetherington (2003c), p. 184.

[506] Planck Collaboration (2015).

meteorology as a testing ground and meteorologists used computers as a way to handle large quantities of data and to model weather systems. In 1961, the meteorologist Edward Lorenz noticed that the outcome of computations on simple nonlinear models was extremely sensitive to differences in the initial conditions. This observation was noticed by mathematicians, physicists and economists, leading to the emergence of chaos theory, which has applications in various fields dealing with apparent disorder, like biology, ecology, economics, metereology, astronomy, and physics.[507] Electronic computers were important in part because the mathematics of chaos theory involves extremely numerous iterations of the same mathematical formulae, which makes it impractical for humans to do the computations themselves, and in part because computers aid in the visualization of fractals.[508]

I will not say much about chemistry here, since a chapter of this dissertation is devoted to changes in chemical instrumentation in the 20th century. It is worth mentioning, however, the importance of the balance in the 18th century Chemical Revolution. Multhauf (1962) points out that "[a]ll forms of quantitative chemistry depend ultimately on gravimetric analysis, that is, the weight measurement of the components of a chemical reaction in the light of the laws of the conservation of matter and of the constancy of chemical composition." Accordingly, one might try to explain the importance of the balance in the Chemical Revolution as following from a decision to apply these laws. Multhauf claims, however, that the laws of the conservation of matter and of the constancy of chemical composition had been the prevailing assumptions of chemists and natural philosophers since antiquity, yet the balance did not become a mainstay of theoretical chemistry until the late 18th century.[509] He argues that "it was the use of the balance in that century which brought a clearer recognition of the significance of the concepts of the conservation of matter and of the constancy of chemical composition, rather than the

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[507] Westwick (2003), pp. 138-139.

[508] Gleick (2008), "A Geometry of Nature" and "Images of Chaos."

[509] In a similar vein, Poirier (2005), p. 69 claims that the idea that mass is conserved during a reaction pre-dates Lavoisier, and that the latter's originality lies rather in taking the principle as an incontestable axiom, as a secure foundation on which, in Lavoisier's words, "the whole art of performing chemical experiments depends."

reverse."[510]

Here it is worth noting a contrast between Multhauf's claims and the views of Alexandre Koyré (mentioned in the introduction) on the priority of theory over experiment. Koyré held that theory had priority over experiment because the latter "presupposes and implies a *language* in which to formulate the questions [asked of nature], and a dictionary which enables us to read and to interpret the answers." Theory provides that language, and "obviously the choice of the language, the decision to employ it, could not be determined by the experience which its use was to make possible."[511] On a Koyréan view, then, the decision to employ these laws could not be determined by experience, since they are what allow the formulation of questions that it makes sense to answer by means of the balance, as well as providing the vocabulary for interpreting the answers. If true, however, Multhauf's claims inverts the Koyréan schema: rather than that the decision to employ the laws determined the nature of experience, it was experience that determined the nature of the decision, to employ the laws consciously and systematically.

Finally, I will mention a few examples from biology. Dierig (2003) has argued that the development of experimental physiology in the late 19th century was intertwined with growth of mechanized industry. His key example is an engineering innovation from that period, the gas motor, whose initial market consisted of urban craft workers seeking a means to compete with large-scale industry. This motor was eventually integrated into the material practices of physiology, along with industrial methods of organizing work, like large-scale divisions of labor and managerial hierarchies. These innovations changed the nature of the experiments that could be performed, allowing experimentation to take place over longer periods of time than before, producing massive quantities of data, and permitting greater precision, control and standardization than was possible when physiology was performed by solitary researchers (or small groups) using manual methods.

A more recent example is the use of computers and automation in molecular biology. Hagen (2000) traces the role of computers as important tools in molecular biology to the early 1960s. Several factors conspired to encourage molecular biologists to employ

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[510] Multhauf (1962), p. 210.

[511] Koyré (1968), p. 19.

computers in their research during this period. High-speed digital computers were becoming widely available to academic biologists. The idea that macromolecules carry information provided a conceptual link with computer science. An expanding collection of amino-acid sequences, partly a result of the automation of protein sequencing, provided a source of large quantities of data for which computers were well-suited. These factors encouraged molecular biologists to employ computers to solve problems that were otherwise infeasible to solve without their computational power, such as the determination of the amino-acid sequences of proteins, phylogenetic analysis, and the construction of three-dimensional models of macromolecules. According to Hagen, this early use of computers paved the way for the emergence of bioinformatics which, when combined with the automation of DNA sequencing, made contemporary genomics possible. The combination of sequencing technology and bioinformatics is vividly described in Garcìa-Sancho (2012):

> These two institutions, the Sanger Institute and the EBI [European Bioinformatics Institute], then [early 1990s] embarked on an audacious enterprise: to contribute to the Human Genome Project (HGP), an ongoing international effort aimed at the determination and computer interpretation of the 3,000 million chemical units which constitute our genetic material. The chemical units are called nucleotides …
>
> The HGP was virtually concluded by 2001, with the publication of a draft covering more than 90 percent of the human genome—the complete set of nucleotides forming our genetic material. The nucleotides are linearly aligned within the DNA molecule, the double-helical structure which had been elucidate some 50 years earlier in nearby Cambridge [on the basis of data from X-ray crystallography, itself a recently developed instrumental technique at the time], thereby providing evidence that DNA is the material of which genes are made. The Sanger Institute and the EBI are currently studying the genome of other species … This work is partly conducted at the Institute's Sequencing Centre, where dozens of aligned apparatuses determine one-by-one the sequence of adenines, cytosines, guanines or thymines in the genome of the corresponding species. Techniciancs walk around the machines—called sequencers—ensuring the cycle never stops. At the EBI, computer suites contain dozens of IT 'geeks' visualizing the sequential information on screens and building interconnected databases.[512]

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[512] Garcìa-Sancho (2012), p. 2.