❧ The Sociology of Theoretical Physics ❧

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Declaration

This work has not previously been accepted in substance for any degree and is not concurrently submitted in candidature for any degree.

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This thesis is being submitted in partial fulfilment of the requirements for the degree of PhD.

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This thesis is the result of my own independent work and investigation, except where otherwise stated. Other sources are acknowledged by explicit references.

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When I heard the learn'd astronomer;

When the proofs, the figures, were ranged in columns before me;

When I was shown the charts and the diagrams, to add, divide, and measure them;

When I, sitting, heard the astronomer, where he lectured with much applause in the lecture-room,

How soon, unaccountable, I became tired and sick;

Till rising and gliding out, I wander'd off by myself,

In the mystical moist night-air, and from time to time,

Look'd up in perfect silence at the stars.

—Walt Whitman

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Finalmente, dedico esta tesis a mi abuelo Rata, y a la memoria de la Abuela.

Abstract

This thesis is centred around the analysis of how the different groups of specialist experts that make up theoretical physics at large communicate and transmit knowledge between themselves. The analysis is carried out using two sociological frameworks: the Studies in Expertise and Experience (SEE) approach by Collins & Evans (2007), and mechanisms of sociological and institutional trust in the general sociology of science literature. I argue that the communication process is carried out in two ways: through *interactional expertise* that is based on deep comprehension when the interaction is between micro-cultures that are sociologically closely connected, and through lower forms of knowledge relying on trust for the micro-cultures that are sociologically far apart.

Because Collins & Evans' framework is strongly based on processes of transmission of tacit knowledge, an analysis of the importance of tacit knowledge in physics is carried out to support the thesis. Specific types of tacit knowledge are closely examined to understand how they shape theoretical physics practice. I argue that 'physical intuition', one of the guiding principles of all theoretical activity, is in fact a type of tacit knowledge — somatic tacit knowledge — that is familiar to both philosophers and sociologists within the academic literature. The end result is a description of physics that highlights the importance of sociological mechanisms to hold the discipline together, and that *permit* knowledge to flow from the empirical to the theoretical poles of physics practice, and vice versa. The thesis is supported by unstructured interview material and by the author's prolonged interaction within theoretical physics professional circles.

Contents

Introduction

Like my mother used to say, 'if my grandmother had wheels, she'd be a trolley car.' My mother didn't have wheels, she had varicose veins. Still, the woman gave birth to a brilliant mind. I was considered for the Nobel Prize in physics. I didn't get it, but you know, it's all politics just like every other phony honor.

— Boris, theoretical physicist; opening monologue from Woody Allen's 'Whatever Works'

A new picture of theoretical physics

This thesis will examine the way in which different types of theoretical physics groups establish communication amongst themselves, and with other theoreticians from fields of knowledge closely related to physical theory (viz. experimental physics and mathematics). A preliminary difficulty in carrying out this project is that while physics is usually categorised as either experimental or theoretical — and this classification is observed within the majority of philosophical and sociological studies of science — this classification does little justice to the many types of physics involved in non-experimental work. Thus before facing the problem of communication in full, I will start out by offering a richer vision of physics practice than is usually given in the science studies literature. Although I will examine the culture of 'traditional' pure theoreticians working in front of blackboards, trying to write down the laws that describe the fundamental interactions of the physical universe, I will also draw attention to other areas of theory that lie between this high-theory and purely experimental physics (that is, laboratory physics). The areas that mediate between theory and experiment constitute sociologically autonomous fields of knowledge and practice, even though their

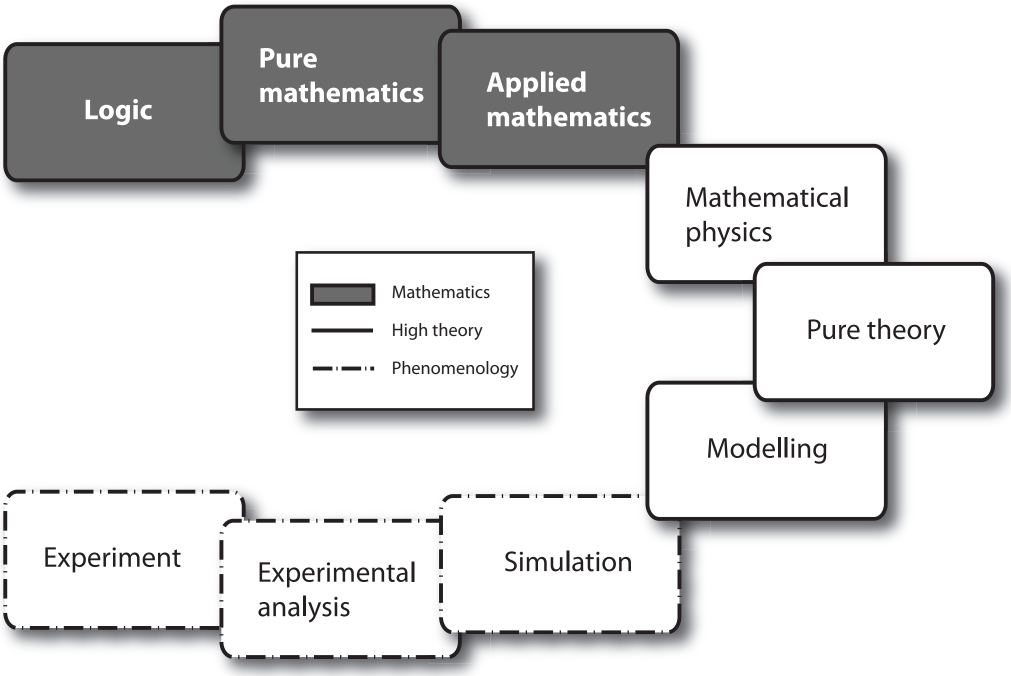

Figure 1: The horseshoe diagram.

social networks overlap with the networks of physicists in the other more theoretical or more empirical domains. These phenomenological physics domains include sets of skills that are distinct from those found either in pure theory or experiment. The structure of these overlaps and the exchanges of knowledge which they permit are the main topics of this thesis.

By probing the role of phenomenological physics I will offer a new picture of physics that portrays the discipline as a diverse mix of epistemic elements bonded together by social interactions and special languages. In parallel I will also describe mathematical physics as the connection between physics and theory's other epistemic pole, pure mathematics. Like phenomenology, mathematical physics has developed into an autonomous area of expertise that is independent from both pure theory and pure mathematics. This model of physics is represented in Figure 1, which I will refer to as the 'horseshoe diagram'.

The horseshoe diagram represents a 'chain of practices' that I will illustrate in the following chapters, by elucidating how theoretical knowledge is sociologically connected to the production of empirical and mathematical knowledge. The horseshoe shape depicts how physics is a science that is 'grounded' on two poles, like a magnet. While these poles are 'opposites' if viewed from a classical epistemological point of view, they are in fact tied together by scientists' practical work and discourse. In fact, there are further similarities to the physical properties of a magnet. When one breaks a magnet into smaller pieces, these still maintain their bi-polar properties. Likewise, every one of the lozenges in the diagram, seen as a piece of practical physics, is subject to the simultaneous pull of the empirical and the mathematical. This is the heart of all physics. All physics confronts this tension, but each epistemic pole affects the different 'micro-cultures' of theoretical physics in different ways. In order to explain how this occurs, I will focus on two themes: the way that each lozenge can be conceptualised as a sociologically and epistemically autonomous space, and the way in which knowledge is transmitted across and between these. The horseshoe diagram also distinguishes between three broad 'families' of practices (mathematics, high-theory, phenomenology), groups of micro-disciplines whose practitioners tend to interact closely or where there is ample possibility for individuals to move across the boundaries of the individual lozenges.

The unity and disunity of physics

A topic central to science studies is the discussion concerning whether science as a cultural phenomenon is unified by any sort of generalised property, method, object, etc., or what in the literature is loosely referred to as the question of 'the unity of science'. [1] As Galison & Stump (1996) point out, the debate in modern terms can be traced back to the Vienna Circle's desire to find a language whereby all scientific endeavour could be given firm universal roots, as glimpsed through the title of the logical positivist's most celebrated collective publication, the *Encyclopaedia of Unified Science*.

In Chapter 1 I explore how the positivist unification project has influenced science studies' descriptions of theoretical physics, and how the implicit positivist lead has given rise to minimal epistemic diversity in descriptions of physical theory. The chapter is dedicated to tracing how social studies of science developed alternative accounts of scientific knowledge by positing sociological demarcation criteria that do not fit traditional subject-centred epistemologies, and the effect of this transition to a col-

[1] See for example Oppenheim & Putnam (1958).

lective epistemology on the unification project. Although the present consensus in social studies of science is that science is a diverse and multi-faceted collection of practices and languages that defies unification (what may be termed the *disunity thesis*), this generalisation is nevertheless not reflected in the way theoretical physics is currently understood or described by social studies of science. This is argued to be due in part to the limited number of sociological reflections on theoretical physics. Appealing to a popular science studies metaphor, although sociologists, anthropologists and ethnographers extensively mapped out the practice of experimental physics, theoretical physics still remains very much a 'black-boxed' discipline in sociological terms. The horseshoe diagram is an illustration of how theoretical physics can be re-conceptualised as a set of smaller and disjoint micro-cultures. Hence, this work resonates with and embraces the disunity thesis.

Chapter 2 describes the general properties of each lozenge and illustrates the multiplicity of practices in theoretical physics, that is, it shows that the disunity thesis holds true in theoretical practice. However, adhering to the disunity thesis is problematic because it comes coupled to another question: how is it that despite the multiplicity and fragmentation of practices, cultures, and languages in science it nevertheless remains possible for scientists to collaborate, cooperate and exchange knowledge? The answer given here is twofold, based on the work of Collins & Evans (2007) and their Studies in Expertise and Experience (SEE) framework. Within Collins & Evans' framework knowledge-transfer is carried out in two ways: through 'direct' transmission from esoteric-experts to other esoteric-experts who speak the same expert language, or through secondary accounts that are reconstructed for the 'layman'. Theoretical physics is argued to operate through these knowledge-exchange mechanism also. Lozenges in the horseshoe diagram that are near to each other develop *interactional expertise*, or the ability for individual experts in one lozenge to acquire proficiency in the language of another lozenge without becoming full-blown experts in a 'foreign' domain's practice. Interactional experts acquire the ability to become fully proficient in a foreign lozenge's language and culture, while still maintaining their own cultural identities. On the other hand, for the case of lozenges that are 'far' from each other in the horseshoe diagram, the low-level knowledge exchanged is based on setting up chains of *trust* in autonomous expert domains.

I end the chapter discussing the *interactional expertise/trust model* in relation to the highly influential *trading zones model* of Galison (1997), which has been broadly posited as a solution to the problem of cross-cultural communication in science. According to this model when two autonomous cultures meet, in order to communicate effectively they start developing hybrid languages inside mutually sanctioned linguistic spaces that are created for this purpose. A critical feature of trading zones is that while some of these may be ephemeral and parasitic on the interaction between the disjoint cultures, if the interaction is sustained for a long enough period a new and autonomous language may emerge from the hybrid trading zone language. I argue that this leads to conceptual complications which render trading zones as incomplete models of generalised scientific knowledge-exchange; in fact if taken at face value the linguistic metaphor used by Galison only makes cross-cultural communication *more* complicated. While both models are not incompatible and can be seen to work in parallel in some cases of theoretical collaboration, the trading zone model does not solve the problem of communication posed by the disunity thesis, while the IE+trust model is tailored made to *permit* communication across fragmented domains.

Chapter 3 analyses the problem of communication focusing on the high-theoretical section of the horseshoe diagram, by describing how high-theoreticians understand and use empirical data in their work. In order for it to be considered physics and not mathematics the object of a theoretician's study has to be somehow grounded on empirical work. But experimental work lies well outside of high-theoretician's realm of epistemic authority, and in this sense high-theory and experiment are completely autonomous domains, and the communication problem is most visible. I illustrate how trust is established to apprehend established experimental results, even when in practice high-theoreticians have no access to understanding the means of experimental work. In fact, no mutual understanding is necessary, and to further appreciate the structures behind this 'blind trust', I illustrate the existence of what I have termed *virtual empiricism*: the idea that although the epistemic mechanisms of experimental physics lie well outside theoreticians' direct access, other experts are always *potentially* if not actually available to explain empirical knowledge production (or that given enough time, the theoreticians themselves could *potentially* come to comprehend experiment in full). Lastly, I also show that when these epistemic gaps *are* bridged by a sheer practical necessity to collaborate, it is mainly through immersion in the other collaborator's language and culture — that is, through the development of interactional expertise.

Chapter 4 explores the relationship between high-theory and mathematics, explicating an epistemic division between them that parallels the one between high-theory and experiment, and that also also uses mechanisms of trust to bridge large epistemic gaps. I deepen the analysis of the concept of tacit knowledge further, examining the role of tacit knowledge in mathematical and theoretical practice directly. Using the tripartite typology of tacit knowledge found in Collins (2010), I analyse a famous discussion between two mathematical schools in order to emphasise the importance of a particular kind of tacit knowledge that is of importance in both mathematics and physics: somatic tacit knowledge. This type of tacit knowledge is traced to the concept of *intuition*, which both mathematicians and physicists uphold as one of the key guiding elements of their practice. The role of a second type of tacit knowledge, relational tacit knowledge (knowledge that is potentially explicit, but remains tacit), is also explored in connection with mathematical proofs and theoretical derivations. Tacit knowledge is used as a primary sociological marker to delimit the constitution of autonomous epistemic fields and expertises. Finally I argue that it is the difficulty of transmitting tacit knowledge that is the source of the theoretical physics' fragmentation into different micro-cultures, and that gives rise to the need for mechanisms such as virtual empiricism to face the difficulties in transmitting knowledge between micro-cultures that are distant from each other.

Chapter 5 elaborates on the last type of tacit knowledge, the most sociologically significant type: collective tacit knowledge. I present cases of people who practice theoretical physics outside of the culture of mainstream professional physics, and the reasons that lead them to be labelled 'cranks' by physicists. These cases show that although it is possible for cranks to do technically competent work in physics, the work is illegitimate from the collective point of view because cranks are not immersed in the social world of professional physics. While scientists tend to emphasise the individual's universal accessibility to scientific knowledge when speaking to a non-scientific public, when theoreticians confront cranks they tend to stress the collective dimensions of scientific knowledge. In the latter case, theoreticians argue that the strength of their discipline lies not in the individual but in the collective efforts of consensus opinion. Additionally, the strategies for 'isolating' the pool of communal knowledge from cranks is carried out at the collective, and not at the individual level. Finally, I also discuss the means and processes through which novice physicists acquire collective tacit knowledge and are thus set up on the path of becoming expert theoreticians by their mentors, that is, how novice theoreticians learn to practice 'legitimate' physics and become experts in their field.

CHAPTER 0

On methodology

Thoughts and intentions, even one's own— perhaps one's own most of all— remain shifting and elusive. There is not one single thought or intention of any sort that can ever be precisely established. What the uncertainty of thoughts does have in common with the uncertainty of particles is that the difficulty is not just a practical one, but a systematic limitation which cannot even in theory be circumvented.

— M. Frayn, 'Copenhagen' (postscript)

On methodology: Fleck, 'thought styles' and 'thought collectives'

In his analysis of medical biology, Fleck (1935, p.39) introduced the concept of *thought collective*, which he defined as "a community of persons mutually exchanging ideas or maintaining intellectual interaction." A though collective, he claimed "provides the special carrier for the historical development of any field of thought, as well as for the given stock of knowledge and level of culture." This stock he named a *thought style*.

Fleck illustrated thought styles by developing a careful study of the emergence of 'syphilis' as a scientific concept, from antiquity to modern medical science. Thought styles were the antecedents of T. S. Kuhn's *paradigms*, and in Fleck's opening chapter he condenses the ideas concerning paradigms that would later be set out and amplified in Kuhn (1962). Fleck (1935, p.29) also antedated Kuhn's differentiation between normal periods of science and revolutionary periods brought about by 'anomalies' stating that "many theories pass through two periods: a classical one in which everything is in striking agreement, followed by a second period during which the exceptions begin to come to the fore". [1] In Fleck's account thought styles are the collective matrices from which concepts spring, just as paradigms are the social matrices on which Kuhn's scientific practice is carried out. As Fleck stresses — and as Kuhn would later posit as a central tenet of his theory — it is not enough to investigate the relationship between subject and object to understand how cognition and new knowledge come about, and one must also include thought styles as "a third partner in this relation; it is a basic factor of all new knowledge".

For Fleck the principal object of study in the analysis of scientific knowledge is not the individual but the thought collective and its associated thought style. It is in the phenomena that transcend the individual that one is to find the characteristics of thought collectives and thought styles. Fleck (1935, p.45) in fact argues that the thought collective is more stable than the individual as an object of study and as a repository of scientific knowledge, since as the epigraph for this chapter suggests, individuals "consist of contradictory drives".

Taking all this into account, one could posit that to apprehend the thought style of a thought collective, one ought to probe the majority of individuals that are known to be immersed in a thought collective, and then find the common strand in their thought patterns. Following this pathway, if the aim of a researcher were to enunciate the thought style underlying a particular scientific group, then the proper way to carry on would be to probe as many individual minds of that community as possible and to highlight the commonalities.

Fleck himself offers an alternate pathway. After discussing thought styles and collectives, Fleck presents *himself* as an example of a member of a scientific thought col-

[1] Although Kuhn acknowledged Fleck's influence on his own work, there are some differences in both their theoretical and conceptual frameworks. Mößner (2011) for example suggests that though collectives differ significantly from paradigms by the scope of conceptual phenomena that each author aimed to encompass. Perhaps more importantly, Moßner argues that while the process of abrupt change and incommensurability is central to Kuhn's conception of scientific change and the role of paradigms, Fleck stresses that within a thought style changes occur gradually. Nevertheless, even in this critique the similarities between the two concept are significantly greater than the differences.

lective, and then provides an extended reflection upon the thought style that he has — by definition — adopted in becoming a proven expert in a scientific field. Fleck (1935, p.52) does this by attempting an explanation of a technique known as the Wasserman reaction. "For a long time I have wondered how I could describe the Wasserman reaction to a layman. No description can take the place of the idea that one acquires after many years of practical experience with the reaction", he confides. Fleck's strategy is to identify those elements that allow him, a medical scientist, to enunciate to the layman the 'facts' concerning the Wasserman reaction. Fleck is then able to reflect on what allows him to make sense of the scientific world in which the Wasserman reaction is meaningful, as opposed to a layman, who by not having these concepts would see the very same world devoid of any significance.

To be then not to be

Just like Fleck's writings on the Wasserman reaction, the present work is also borne from personal experience, specifically on nearly a decade of professional work as a physicist during which I actively participated as a junior research assistant in one of Latin America's most important physics institutes. Although I never formally attended higher degree courses, I sat through countless seminars, colloquia, and conferences on physics throughout these years. I co-authored a handful of peer-reviewed articles and spoke at international conferences on a few occasions. Although I do not possess an upper-level physics degree, I nevertheless have the experience of having been an actively participating young physics researcher, immersed in the daily life and the social world of the physics professional.

But it is not on my credentials or my track record that this thesis on the sociological aspects of theory is based on. It was not in a classroom, but in the prolonged interaction, in the being part of everyday physics research for an extended period where I learned 'what being a physicist is like'. Even if I had not published anything (as happens with most undergraduates and many graduate students), direct exposure to activities and dialogues with and between other physicists would have put me in the same position of immersion within the social world of physics.

This brings me back to Fleck's reflexive exercise. I grew interested in both philosophy and sociology of science in my academic career at about the same time as I was exposed to the world of professional physics research. I was also deeply interested in how sociological descriptions of science seemed to stray away from the image of science proposed by the more 'romantic' physicist talk I knew, or the physics of popular science. This was probably the beginning of the end of my career as a physicist. After a physics degree, while still actively involved in physics research and publication, I began a Master's degree in philosophy and sociology of science, which initiated a gradual migration out of physics research. This, perhaps, reached a conclusion around the middle of the writing of this thesis. Nowadays, although I still keep contact with my scientific ex-collaborators, and try to attend conferences as much as possible and to keep up with *arXiv* publications in my field, I cannot say that I would be able to publish in the field again — at least, not without months of intensive work exclusively on physics.

This interactional dimension is, I believe, fundamentally important for the plausibility of the kind of social science research which is carried out here. The thesis can be said to draw on a generalised method known in the literature as 'participant observation', which contrasts with another fundamental sociological methodological position which can be referred to as 'unobtrusive observation.' I will not argue over the superiority of either method over the other, but will discuss and outlining the positive and negative aspects of participant observation.

The first thing to note is that participant observation is not a methodology in the sense of a determined and algorithmic procedure or set of actions, but a generalised approach which involves the production of knowledge through the interaction of the researcher with the subject, and not despite it. Thus Schwartz & Schwartz (1955, p. 344) define it as "a process in which the observer's presence in a social situation is maintained for the purpose of scientific observation. The observer is in a face-to-face relationship with the observed, and, by participating with them in their natural life setting, he gathers data." The generalised approach thus having been set out, the way that interaction is carried out can vary enormously.[2]

The subjective/objective dichotomy

Gold (1958) distinguishes four types of participant observation: *complete-participant*,

[2] See Jackson (1983).

participant-as-observer, *observer-as-participant*, and *complete-observer*, outlining the virtues and difficulties associated with each type. Gold states *complete observer* as the most desirable position for those that aim to become *unobtrusive observers*, for in fact all observation is in some ways participant as in real research contexts there is no such thing as a perfectly-unobtrusive observer. In the study carried out here, most of my experience in theoretical physics is divided according to Gold's categorisation between participant-as-observer (as in the earlier stages of my physics undergraduate degree, where I still acted as a novice physics apprentice) and complete-participant (as in the latter stages of my physics research where I was actively publishing and participating in the activities of a professional physicist). The interview material comes from a transition period from a sporadic complete-participant to a current observer-as-participant.

Nevertheless, Gold (1958, p. 219) considers it crucial that the complete-participant not fall into the trap of 'going native', that is, of losing sight that the observer is not in fact part of the community under observation. According to Gold, the complete-participant observer "knows that he is pretending to be a colleague". This would seem to introduce a fundamental problem for a complete-participant (like Fleck, or me) to produce a sociological account of one's own discipline.

Collins (1984) however has presented a different way to address the problem, drawing heavily from a philosophical perspective of the social sciences deeply influenced by the 'late' Wittgenstein, a position notably advocated by Winch (1958). According to this philosophical view, the role of the social scientists is not to provide 'objective' information that aims to describe cultures or societies as 'outsiders' with varying degrees of detachedness, but rather to provide accounts that rest upon an understanding of these cultures or societies. Absolute 'distance' between a social scientist and his object of study is not only undesirable but also fatal to this sociological project. Thus Collins refers to his own work not as 'participant observation' but 'participant comprehension.'

Winch and Collins perceive sociology as a means to understand the 'forms-of-life' (*Lebensform*, sometimes translated as 'life-form') that define social groups, a term coined by Wittgenstein to refer to those elements that permit individuals to make sense of the linguistic world around them, to move around in that world, and to understand and to give meaning to it. This is possible because according to Wittgenstein, to speak a language is not only to know the meaning of a group of words and to put them in the right order, but crucially includes the understanding of the form of life from which it sprung:

> Here the term "language-game" is meant to bring to prominence that the speaking is part of an activity, or of a life-form.[3]

In fact, forms of life and languages are intrinsically tied together:

> "So you are saying that human agreement decides what is true and what is false?"— It is what human beings say that is true and false; and they agree in the language they use. That is not agreement in opinions but in form of life.[4]

Wittgenstein carefully explored the grammar of language games, that is, the set of rules that make an action within a game either meaningful or meaningless as part of the game. Wittgenstein showed that grammars are not and cannot be sets of pre-written and completely unambiguous rules. One of Wittgenstein most important propositions is that it is in the application of a rule that one learns what it is to follow a rule correctly. In fact, Wittgenstein argues, this is the way in which we actually use rules in practice all the time, by being shown their correct usage. Wittgenstein dismisses the possibility of there being unambiguous rules by noting that there must always exist conventions to enable rule following, for "the word 'agreement' and the word 'rule' are related to one another, they are cousins. If I teach anyone the use of the one word, he learns the use of the other with it". [5]

Translating this into a concrete sociological program was a crucial contribution from Winch. [6] It is thus the 'social grammar' that one investigates in Wittgenstein/ Winch/ Collins-type sociology. When applied to sociological investigations, the central topics to be studied are the elements of the socio-cultural world that enable members of a social group to make their actions socially meaningful. To act in a socially

[3]Wittgenstein (1953, §23 p. 23e).

[4]Wittgenstein (1953, §241 p. 75e).

[5]Wittgenstein (1953, §224 p. 73e).

[6]Pettit (2000, p. 64) has identified three core 'individual-level' theses in Winch's work that are at the centre of his sociological approach: "1. The rules thesis: understanding human action involves seeing the rules or proprieties in accordance with which it is produced, not just detecting regularities in its production. 2. The practicality thesis: understanding human action does not mean just grasping the intellectual ideas that permeate it but, more deeply, cottoning on to the practical orientations of the actors. 3. The participation thesis: understanding human action involves participating in the society of the agents, at least in imagination, not just standing back and surveying that which they are doing."

meaningful way is to adopt the form-of-life that enables the enactment of social action, thus focusing Wittgenstein's ideas beyond the world of pure language and in action itself.

From this perspective sociology takes on a new role where there is no longer a problem with how 'close' one should approach the object of study in order to produce proper results. Instead, one must come up with the means to become completely immersed in the way that the social actor understands the social world. 'Going native' is no longer a problem, but instead an idealised virtue. Collins (1984, p. 60) explains, "In participant comprehension, the participant does not seek to minimise interaction with the group under investigation, but to maximise it. Native incompetence is not a technical problem to be overcome [...], but rather the development of native competence may be the end point of participant comprehension."

The Wittgensteinian sociologist *wants* to see the world through the native's eyes. It is those elements that permit this native vision to arise that make up the 'social grammar.' The sociological sense is given not by an observational but by a reflexive exercise: it requires not only that the sociologist understand a form-of-life (as a native), but additionally that he understand the process that allowed the acquisition of the 'thought style' in question, as in Fleck's case.

There remains the methodological problem of whether 'estrangement' can be achieved by an ex-scientist in order to do sociology, and not just auto-ethnography. We know that this is at least *possible* because otherwise sociologists would be incapable of making any pronouncements of the society they themselves live within, a scenario which would invalidate an enormous (if not the most part) of existing sociological work! The establishment of 'distance' between oneself and one's object of study when one has strong links to that same object is not a simple matter, and it requires a deep reflexive exercise, but is not essentially impossible. As in Fleck's case, this naturally leads to trying to explain not only how it is that what one is trying to describe happens, but also why this happens. The 'why' then becomes the key to introducing a second-level explanation and entering the estrangement process where one needs to momentarily 'suspend doubt' on experience in order to grasp the mechanisms that bring this experience about. One can, for example, choose a sociological explanation, just as well as one can choose a historical one, an anthropological path, a psychological path, etc. The researcher also has the access to other's experiences, other's reflections and other's explanations, which may or may not fit with one's own. It is the latter one that tends to be the more intellectually productive, because it forces one to defend or revise one's own account, which auto-ethnography has no need for since it is by definition only the researcher's own experience that is needed.

Many kinds of physics

The Physics Institute where I worked had the advantage of being theoretically centred; during the first year I spent there at the Experimental Physics Department I proved to be completely and absolutely incompetent at any work of that type. During that time I did however learn a good deal of what experimental work looks like, by playing a rather insignificant role in developing the computer simulation that would be used to calibrate a neutron detector that would later be used at CERN. Again, more than my 'achievements', it was the exposure to the weekly seminar and colloquia, the possibility of seeing the inside of a rusting but still working van der Graaf accelerator, the extended conversation with my experimental student colleagues and supervisor, that enriched my vision of what physics looks like from the inside. After a year I moved to the theoretical half of the institute, fortunate enough to land in a group where a diverse mixture of people interacted and where work involved exposure to many kinds of theoretical physics, from computer programming of simulations of nanotechnology devises, to theoretical scrutiny of very mathematical theory.[7] The Physics Institute also offered me the same great advantages that being at the National University of Mexico offers all its students and staff: a constant bombardment of physics-related seminars and colloquia. Just within the Physics Institute, there were three regular weekly seminars to attend: solid state, theory and experiment. Since all the research institutes are located within minutes of each other, I also had the opportunity to attend seminars from other institutes.

I started this thesis with one thing in mind, which brings me again back to Fleck: attempting to explain to the 'sociological layman' what theoretical physics looks like

[7] My main topic of research was the Casimir effect, which has been tackled from the theoretical perspective at the Institute by several research groups through the study of Green function methods, and which was our main mathematical tool. I also grew interested in using Nonstandard Analysis to understand the infinite quantities that appear in Casimir force calculations.

'from the inside', and to connect it to sociological analyses of scientific practice. However, a problem arises which has been amply discussed in the sociological literature. How can I be sure that my experience and my conceptions of theoretical physics are the reflection of the 'thought collective', to use Fleck's terminology? There are ways for the researcher to keep 'checks and balances' and show that an argument is reasonable. The first is also based on one of Fleck's observation that in order to develop ideas, and in order to communicate them to one's professional peers, there is a lot of common knowledge that has to be presupposed. When trying to explain the Wasserman reaction, Fleck explains how he finds that many of the terms that as a professional he would find understandable almost as second-nature, when explaining to a layman he has to stop and reflect on how to put into easier terms. The important point here is that as 'specialists', the thought collective's thought style plays an important role not only for the individual to 'tap into' the thought collective, but for also for the individuals within a thought collective to dialogue between themselves, to understand each other. During the interview sessions with theoreticians to gather material for this thesis, I acted not only as a passive recipient of information but sometimes introduced questions that were specifically intended as a check on either my or previous interviewees' affirmations. There were times where despite my insistence on particular viewpoints, the interviewees were very resistant to my answers, and I was forced to concede that these were indeed views that 'did not fit the mould'. I have included this in the text where appropriate by noting that even if I hold a certain point to be 'widely held', there may be significant number of physicists or theoreticians that do not adhere to my viewpoint.

In fact being a sociologist, or passing as a sociologist, is not always the best way to get at the spirit of theoretical physics. A curious phenomenon which I can only recount in anecdotal terms is the attitudes that my interviewees exhibited depending on whether they saw me as a sociologist or as a physicist. I noticed very clearly that with most of my interview subjects, when I established contact first as a sociologist the initial interview answers were very much aligned with the public, 'politically-correct', non-sociologically informed portrayal that science disseminates to the public. On the other hand, when I presented myself as an ex-physicist, the answers were more frank and closer to the traditional findings of STS.[8] This phenomenon is not unknown in sociology. Jackson

[8] A brief example is given in Chapter 1, when Professor M. Berry admits having started out our interviews with such an attitude, when by the second interview he had 'let down his guard.' In order for this

(1983) for example quotes Whyte (1979), explaining that, "informants generally find it a rewarding experience to be interviewed by a skilled and sympathetic person informants...often find the experience of being interviewed is not only enjoyable but also useful in helping them to gain perspective on and understanding of their ideas and experience," suggesting that "this sounds all too convenient for the social scientist." I claim only this subjective observation: I found that when there were signs of resistance, there was a clear difference in respondents' dispositions when they believed or were offered evidence that I was familiar with the world of theoretical physics.

A final methodological point related to this discussion is to differentiate the present work from an autoethnographical exercise. Certainly, on the surface there were elements of autoethnography in the beginning stages of my work, such as the inclusion of autobiographical data, and the reflexion upon personal experiences to link these to a wider social context. But unlike the autoethnographical exercise, described by Ellis (2004) as "research, writing, story, and method that connect the autobiographical and personal to the cultural, social, and political", my personal experience is not the central point of this thesis but only the *means* to access to the real object of study: the collective dimension of a particular scientific culture. Science as culture is of course a topic that has been previously dealt with by sociologists, and this brings about another difference with autoethnography. It is not 'in my own voice' that the present thesis is written, but is directly aimed at being comprehensible to a scholarly audience, and firmly placed within the tradition of science studies and its conceptual frameworks. It is of course impossible or at least otiose to draw attempt to draw a firm boundary between sociology and autoethnography, but the present work certainly is different to that of, for example, J. Ziman in that it is my full intention to directly engage with current debates in the sociological literature and to ultimately hope that it feeds back into sociology.

to happen I had to actively argue against Berry's views and challenge them directly not as a sociologist but drawing on my experience in physics. Eventually Berry conceded, and even recommended me to read J. Ziman's works as an example of an important theoretical physicist that through reflexive efforts had worked out a description of scientific practice that coincided with 'my' point of view (in reality, it was mostly the 'standard' sociological view of scientific publications). Ziman himself often described in his work how his entry into sociological aspects of science eventually stopped him from continuing as a physicist. Although Ziman attributed this to lack of time and the impossibility of keeping up with the field, I think the deeper answer is the immersion into a 'reflexive stance'. Nevertheless, Ziman's work is a good example of an auto-ethnographical exercise that is not quite sociological, in that it rarely goes beyond a statement of Ziman's experiences as a researcher, and contains only minimal references to canonical sociological work.

The interviews

The interviews started at the Physics Institute in Mexico, more because I already knew it and its structure than any other reason. By the time I began the interviews I already had a working list of topics I wanted to explore (e.g. the role of 'physical intuition' in theoretical work, the methodology for training students to become independent researchers, how and if theoreticians used experimental results in their work, etc.) and a general scheme of different 'types' of 'theoretical styles' I wanted to describe. I therefore chose my interviewees according to the field they specialised in, trying to cover all the 'types' I wanted to focus on. Because I had sat at conferences or seminars with most of them being speakers or part of the audience, I had a good idea of the kind of theory they did. In some other cases the recommendation came from physics acquaintances who thought interviewing particular people would give me greater insight.

Although some of the theoreticians I interviewed have superlative professional track records and tremendous experience, I tried to avoid the trap of interviewing only very senior scientists. Physics is a rapidly changing discipline, and what was true of the physics practiced forty years ago may not be true of today's physics. Consider for example the role that computers play in physics nowadays; forty years ago computer simulations, although possible to implement, were certainly not a desktop activity. Today computers and simulations are crucial elements for much— though not all— theoretical work. Thus I interviewed active theoreticians, from researchers in the earlier stages of their careers, to a couple of emeritus professors.

I must also mention the question of geographical bias. Certainly if one wanted to explore experimental physics, there would likely be a tremendous difference whether one interviewed scientists from the Third World or scientists from the First World, something that is easy to understand in terms of the budgets typically available for either case. Fortunately, theoretical physics is more even-handed since financing plays a lesser part in what a theoretician can and can't do (something that was nonetheless probed in the interviews in Mexico). In general, those theoreticians in Mexico interested in the phenomenological dimensions of theory did explain that they faced a relative drawback in the lack of top rate experimental work near them, but although it may make their work slightly harder, I did not get the feeling it impacts the general outline of their work. Moreover, except for one, all the Mexican theoreticians interviewed have

Table 1: Interviewees divided by areas of expertise

Micro-expertise	**Interviewees**
Mathematics/ Mathematical physics	M. Berry, G. Mitchison, D. Tong, M. Mondragón, L. de la Peña.
Pure theory	M. Berry, D. Tong, M. Mondragón, L. de la Peña, V. Romero, M. Horgan, K. Volke, B. S. Sathyaprakash, S. Fairhurst.
Phenomenology/ Simulation	R. Esquivel, V. Loke, K. Volke, M. Mondragón, M. Horgan, V. Romero, C. Noguez, B. S. Sathyaprakash.
Experiment/ Experimental analysis	V. Romero, K. Volke, S. Fairhurst

established international connections or form part of international collaborations.

The second major round of interviews was performed in the United Kingdom, and I sensed no significant deviations from the answers I received in Mexico. This is consistent with my experience in the world of physics, where it is usually seen at conferences that while First World universities dominate the experimental world, theoretical physics is much more dispersed throughout the globe. A final round of interviews was arranged in Cardiff University with theoreticians working as data analysts in large scale experimental collaborations.

A breakdown of the interviews is given in Table 1 according to the categorisation of theory in the horseshoe diagram. Some of the names are included in more than one category because these individuals have worked in more than one area at different points in their careers, or their work directly involves expertise from different categories.

Other primary sources

Along with the interviews and personal experience, I have also tried to use material that I know to be important to physicist's work. Some resources gathered from my own work have also been used, mainly some of the referee reports from my publications that I've used to highlight some of the hidden dimensions of physics publications. Although I try to limit the use of technical papers as a sociological source (for an extended discussion of the reasons behind this, see Chapter 1), sometimes these can serve to illustrate specific points and I have not shied away from using these when I believe it to be the case. One important source is personal e-mail communications, both with the interviewees and with other physicists who for practical reasons could not schedule face-to-face meetings.

The Internet also offers a wealth of material if one knows where to look. Many theoreticians nowadays spend more time in front of their computers than anywhere else, and many devote ample time to their personal pages and in producing documents available online that illustrate a lot of physics' pop culture. Likewise, physics on-line forums and discussion groups such as `www.physicsforum.com` can provide insight into discussion among junior physicists, particularly students in the case of *physicsforums*. Nevertheless, as with all internet resources, these should be used with a degree of scepticism both because of the kind of physicists that populate them and because of the anonymity behind the posts often found them.

An important issue in any scientific work, sociological or not, is the crucial question: how does one know when the 'evidence' gathered is sufficient? There is no formula that can tell the researcher how to strike a balance between the time spent interviewing or gathering data and the time spent making sense of the material and building a coherent narrative. It is, however, possible to get a sense of when new 'data runs' are simply increasing the sample size while providing no additional insights nor new information. Although, when the interviews are considered in isolation, the sample size is not large when compared to many social science projects (notably any that have a quantitative face), the data goes well beyond the interviews. Indeed, the depth of the interviews was made possible only because of the other sources of understanding on which the analysis rests. To explain this in a 'methodology' chapter I would need to list every conference, class, seminar and coffee talk I was ever part of during the ten years I was active in the world of physics.

One of the questions that I posed in one form or another to all the interviewees was whether there is in fact such a thing as a common practical stance or characteristic that one can find in all theoreticians. The general answer is that there is not, and some of the interviewees even stressed the diversity of 'personal styles' in 'doing' theoretical physics. It certainly seems the case that in terms of personalities and professional habits theoreticians are a very diverse lot. Yet one must remember that it is not, as Fleck noted, in an individual's head that one will find 'thought styles' but in their relationship to the thought collectives. And in this, there are definite patterns, which I try to highlight throughout the analysis. These are the same patterns that have been amply discussed in both the sociological and anthropological literature concerning experts and expertise: the necessity of direct interaction between expert and novice as the typical training pathway, the role of tacit knowledge in marking out experts, the necessity of socialisation in becoming a recognised expert, the mechanisms of closure and legitimisation of expert communities, amongst other topics.[9]

[9] See Summerson Carr (2010) and references therein.

Part I

The problem of communication in physics

CHAPTER 1

Theoretical and historical background

Now please give heed to my brief description about the field, what it constitutes, its changes, its origins, as well as its knower and its influence.

— 'The Bhagavad Gita', Chapter 14, Section 4

1.1 Sociology and philosophy as tools of analysis

This chapter will introduce the two main approaches for studying science that will be drawn upon in this work: the philosophical and the sociological study of science, with particular stress on why the sociological approach to knowledge is in this case the preferable one. I am in no way opposed to a philosophical analysis of science, but as I will argue, I consider the dominant philosophical conception of scientific knowledge to be incompatible with descriptions of scientific practice.

I also rely on historical studies of theoretical practice, of which there is a small but excellent tradition as set out in Galison & Warwick (1998) and references therein. The work of the historians participating therein sheds interesting light into the genesis and development of theoretical practices, but its sociological relevance must always be pondered in relation to the present state of theoretical physics, as extrapolating in order to avoid anachronisms . Despite this caveat, many of the results in works such as those by Kaiser (2005a,b), Wüthrich (2010) and Warwick (2003) are absolutely compatible with the present thesis, and where relevant have been referenced in the text as support to my sociological project. I see no fundamental break between sociology or this particular brand of history of science other than the directness and type of material that each chooses to focus on. Indeed, it is the case that in science studies today's sociology is tomorrow's history. Still, the emphasis in this thesis will be on my own empirical material, the historical resources being used only to support particular points.

I begin by analysing the origins of modern science studies in positivist philosophy, which crucially relies on the partition of scientific knowledge into two basic kinds: empirical and theoretical. Positivism was hugely influential on science studies, and for the better part of the 19th and 20th centuries positivist ideas were the foundation of all reflections on science. It was only in the mid 20th century that positivism in science studies was challenged, in what is often called the 'social turn'. As a result, many of the positivist's idealisations of science were shown to be either incorrect or inaccurate portrayals of it. Due to the 'natural' objects of study of each discipline, sociology of science concerned itself mostly with experimental work (based on analyses of practice and action), leaving non-experimental work to the philosophy of science (which was very comfortable working with highly intellectual theory). While a lot of sociology of science was aimed at 'unpacking' experimental physics from the philosophical and popular 'black-box' it had once been, philosophy treated non-experimental physics quite differently, in most cases idealising it or focusing on very narrow products of theoreticians. I postulate that further sociological studies of theoretical physics are necessary to complement the work carried out for experimental physics by sociologists *despite* the numerous works of philosophical nature relating to theory, with this thesis being an attempt to fill in this gap.

Along with the differences in their objects of study, social studies of science developed two important theses that deviate from traditional philosophy in the explanation of what constitutes knowledge: that scientific knowledge is a collective phenomenon and therefore socially constituted, and that a socialised epistemology allows for a greater diversity than the positivist outlook permits. Social studies of experimental physics have highlighted this numerous times, but again, this has had only minor impact in the way we understand physical theory. Thus, non-experimental physics is still referred to as 'theory', that is, as one all encompassing category in which a diversity of practices is not reflected. This is also a dimension of theoretical physics that must be unpacked, and which I have already touched upon in the discussion of the horseshoe diagram.

1.2 The influence of positivism on science studies and physics

Philosophical and historical analyses of science were carried out in the 19th century that deeply influenced scholarly reflections upon science well into the mid-20th century, and of these positivism has had a particularly long lasting influence on all branches of science studies. Both early and late positivists were fascinated by the physical sciences, which they saw as a pinnacle of positivist ideal knowledge far above the development of other fields of knowledge. Although positivism and philosophy were the preferred perspectives from which to study science for more than a century, positivism — at least in its original form — is hardly encountered in science studies circles anymore, except as a historical curiosity.

Nevertheless, in this chapter I will try to show that positivism has left its mark in one significant way when science studies examines physics, a legacy that has shaped the way both philosophers and social scientists have approached the discipline. In short, science studies have inherited from positivism its classification of physical practice. We 'naturally' tend to label physics as either experimental or theoretical, and this division is at the heart of the positivist conception of science, one that does not reflect the multiplicity of practices of modern theoretical physics. Comte's perspective presents a good illustration of the most prominent features of positivism and its classification of knowledge. Although he is by no means the earliest positivist thinker, his work is arguably the first extended meta-theory of scientific knowledge that is not meant purely as a work of philosophy, but also aims to describe the state of contemporary science. Comte's depiction of the structure and organisation of scientific knowledge has come to influence the way we still conceptualise physical theory, and the positivist language used to describe the structure of the physical sciences remains practically intact.

Comte (1830, ch. 1) drew up a framework within which he wished to describe the historical development of all areas of human knowledge, from the most primitive disciplines to the highest in form. According to this Law of the Three Stages, knowledge-making disciplines develop in three fixed types of increasingly growing stature. The first and most primitive stage is the Theological or Fictitious; the second stage is the Metaphysical or Abstract; the third and superior is the Scientific or Positive stage. According to Comte, all fields of knowledge must necessarily follow these stages of development until they reach the positive stage, this being the highest possible form of human intellectual achievement.

While the first and second stages are dominated by explanations that rely first on providential entities (Theological stage) and then on supra-physical entities (Metaphysical stage), the Positive seeks to establish "the explication of facts, reduced to their real terms". The Positive stage's ultimate goal — the ultimate goal of human minds in general according to Comte— is to "represent all diverse observable phenomena as particular cases of a general fact", for as Comte reminded the reader, "all fine minds after Bacon have recognised that there is no real knowledge other than that based on facts".[1] Like Bacon, Comte posited two and only two categories of knowledge in the mature positivist sciences: empirical (relating to 'particular observations') and theoretical (relating to 'general facts'). Like any good empiricist, Comte argued that real knowledge could only be achieved if based on the senses, relegating theory (the seeking of general facts) to an instrumental, classificatory role.

1.3 Logical positivism and the early developments in philosophy of science

Comte's century was one of tremendous change for science as a professional activity. At the turn of the 18th century people who would nowadays be regarded as scientists would not have called themselves by that name, since the word 'scientist' did not appear until 1833 when another major figure in early science studies, W. Whewell, coined the word (we owe Whewell the word 'physicist' too). However, by the mid 1800s science was well on its way to establishing itself as a respected professional activity in most European universities. All along the the 19th century, science, and particularly physics, was rapidly becoming an institutionalised academic field.[2]

[1]"There are and can be *only two ways* of searching into and discovering truth. The one flies from the senses and particulars to the most general axioms, and from these principles, the truth of which it takes for settled and immovable, proceeds to judgment and to the discovery of middle axioms. And this way is now in fashion. The other derives axioms from the senses and particulars, rising by a gradual and unbroken ascent, so that it arrives at the most general axioms last of all. This is the true way, but as yet untried [emphasis mine]", Bacon (1620, Book I, Aphorism XIX).

[2]See for example Otto Sibum (2003).

The transition into the 20th century carried with it two of the great revolutions of modern physics, the birth of quantum theory in 1901 and of special relativity in 1905. In 1910, Russell and Whitehead published their *Principia Mathematica* on the foundations of logical analysis and mathematics. These events deeply influenced academic positivist philosophy of science, particularly that of the Vienna Circle in the 1920s and 30s. Though the members of the Vienna Circle held views that were more varied than is often acknowledged, there was at least one clearly continuous assumption underlying all their work, the unchallenged division between empirical and theoretical knowledge.[3] The unifying purpose of the Vienna Circle's members can be broadly described as a project to give philosophical support and surety to the science of their days under the auspice of empiricism as a foundation for knowledge, while abolishing the role of metaphysical arguments in scientific explanations. Contemporary developments in the foundations of logic and mathematics suggested to the positivists that theoretical statements could have no epistemological foothold except where they could be tied down to observation statements. The central positivist idea concerning theory is that it is the tool with which science correlates observation statements, and has little value apart from that.[4] Logical positivism was an attempt to give epistemological certainty to scientific knowledge by demonstrating how theory based on empirical facts leads to truth. Although there are significant differences between Comte's positivism and the work of logical empiricists, the one unifying strand is the division of knowledge between empirical and theoretical domains.

1.4 The birth of sociology of science

Ironically, Comte also recognised a phenomenon that would eventually end the dominance of positivism in science studies, identifying "the need — in every stage — of some theory or another to bind the facts".[5] That is, although like Bacon he postulated that

[3]A classic anthology taken as representative of the Vienna Circle's work is Ayer (1959), although it has been criticised for presenting a picture of positivism that is too homogeneous and ignores the work of important members of the Circle like Otto Neurath. Hacking (1983, ch. 3) presents a brief analysis of the development of positivism from Comte to the Vienna Circle, to its rebirth in the work of van Fraassen.

[4]As positivism matured, the role of theory also changed for some of the Circle's members. Hempel (1973) for example eventually recognised that theory could also have 'heuristic' roles of explanation.

[5]Comte, *idem.*

observations are the only grounds for true knowledge, Comte admitted an awkward problem for the empiricist project: no observation is possible that does not to some degree require a non-empirical background to 'bind' it's meaning for the observer. Without attachment of observations to some basic theoretical principles, says Comte, "one would be completely incapable of retaining [these observations]; and at most times, the facts would remain unperceived before our eyes". While Comte evidently saw the importance of 'theoretical' assumptions in experimental activity, this was ignored in his work beyond this brief statement; in Comte's implicit theory of knowledge there remained the marked and unambiguous separation between the classes of pure empirical facts (observations) and theoretical facts (general statements) later taken up by the Vienna Circle. For Comte, and for positivism in general, the idea that theory is necessary for observation does not detract from the hypothesis that they can still be regarded as separate forms of knowledge and is *the* central tenet of positivist epistemology.

The dominating influence of positivism's classification of knowledge can be seen in that even strong critics of positivism like Popper (1934), while engaged in direct controversy with the group, took the experiment/theory distinction for granted. Popper's falsificationist program challenged the classical induction arguments of positivist epistemology, but falsificationism still relied on a rational reconstruction of scientific knowledge based on the observation vs. theory dichotomy. [6]

Comte's afterthought regarding the role of theory is now known as the 'theory-ladenness' thesis. It entered science studies mainly through the work of Hanson (1958) and Kuhn (1962) in the mid-twentieth century, and marked the beginning of the so-called 'social turn' in science studies. Theory-ladenness affirms that pure, 'unpolluted' observation without some sort of theoretical substratum is impossible, and so that the positivist foundation of knowledge — pure observation — is a chimera. Kuhn went

[6]Curiously, falsificationism is to this day the dominant philosophical view *within* theoretical physics, as it has been since the 1950s. As one theoretician recalled:

> I remember, in the fifties, I was at school — late fifties — I read Karl Popper, and the emphasis then was...how wonderful refreshing this is because at last here's somebody who's writing about what scientists actually do! He was concerned with the logic — that's the title of his book, *The Logic of Scientific Discovery* — pointing out that there were aspects of this logic which were very different from the Baconian way of looking at things. And this was valuable actually; it was very helpful. *But then he didn't discuss the sociological aspects*. (emphasis added)

well beyond that, affirming that it is precisely through the gestalt-like changes in the non-empirical substrata that true scientific change comes about, and even more, that it is within this *paradigm* that all science is carried out during its normal, non-revolutionary phase.[7] This intertwining of 'theory' and 'experiment' meant a radical departure from the Vienna Circle's foundational assumptions, and its lasting influence on science studies signified the end of logical positivism as the dominant view of science. Sismondo (2009, p. 12) declares Kuhn (1962) to have "challenged the dominant popular and philosophical pictures of science" as his version of science "violated almost everyone's ideas of the rationality and progress of science".

Following Kuhn's socio-historical lead, social scientists then asked themselves if this could be taken a step further. Perhaps there could be other non-empirical elements that affected the creation of scientific knowledge aside from the 'theoretical background'. In particular, they posed the question of whether social and cultural conditions could also influence scientific activity, scientific observation, and scientific results. The shift away from a purely philosophical perspective is referred to as 'the sociological turn'. Before the sociological turn, science studies had mostly been limited to reflections about scientific knowledge (mostly by philosophers and scientists themselves) or by a history of science that concentrated exclusively on the heroes of scientific pantheon, or as Butterfield (1931) called it the 'Whig' interpretation of history, and the sociological work stemming from Merton's functionalist school. The social turn comprised a rapidly growing interest and involvement of social scientists and cultural academics into the field, which eventually resulted in a major change of the science studies landscape. The Strong Program is considered to be the foundational work of the sociology of scientific knowledge through the works of Barnes (1974, 1977) and Bloor (1976).

Barnes and Bloor, with the Strong Programme of the Sociology of Scientific Knowledge, pioneered a sociology of scientific knowledge that took up a new 'socialised' version of epistemology. Barnes' work was particularly important for introducing the importance of 'purely' sociological factors into explanation of how scientific knowledge develops, such as the influence of ideology and interests on scientific activity and its out-

[7]In the same tradition, Maxwell (1962) claimed that there was no manner to cleanly demarcate an empirical ontology from a theoretical one in real life, and thus that what is 'theoretical' and what is 'empirical' is intrinsically intertwined. Lakatos (1978) and Feyerabend (1958), despite being radically opposed in many topics, both held that any difference made between theoretical and empirical terms was actually a psychological construction, and without a real epistemological foundation.

comes in a tradition that relates to Marx, Lukács and Habermas and which also finds much common ground with K. Mannheim's work.[8]

The Strong Programme not only added more possible 'influences' than could be seen in Kuhn's paradigms and dogmas, but also gave more power to these influences than had been admitted before. Kuhn argued that the education of a scientist is purposefully guided to incorporate the young scientist into the reigning paradigm. Kuhn's paradigmatic education involves a deep commitment to a particular way of viewing the world and of practicing science in it, but commitment implies a high degree of consciousness, even if that consciousness is not manifest at all times.[9] On the other hand, the Strong Programme's proposed influences seem much subtler and more dramatic: the scientist might at times seem like a peon of socio-historical contingency to which he is subject without his knowledge, like the individual immersed in Fleck's thought collectives.

Through its incorporation of sociological causality as a method for explanation of knowledge, the Strong Programme was treading on new ground in ways which many philosophers of science, and many scientists, found generally unacceptable. Of particular concern to opponents of the Strong Programme , and even for Kuhn, was the idea that purely social factors could not only tinge or tint scientific knowledge, but could actually mould its contents.[10]

1.5 'Theory' in sociological accounts of physics

It is often argued (at least by sociologists) that the sociology of science surpassed the philosophical image of positivist 'accounts' of science through its immersion in the study of practice.[11] After the 'practice turn', a number of accounts of laboratory life in many scientific fields provided a richer picture of what empirical science 'really' looked like when it was being carried out by scientists. Social scientists are trained to go into the 'field', to observe and attempt an explanation of what social actors do. Most of the best known works in the field are full of writings about 'scientific practice', 'science in

[8]See Olivé (1985, Introduction) for an analysis of the Strong Programme within the framework of these wider sociological theories.

[9]See Kuhn (1963).

[10]See Duarte (2007, ch. 4)

[11]See Collins & Evans (2002) and their comments on the Second Wave of Science Studies.

the making' and 'science in action'; scientists manufacturing apparatuses and discussing things and doing things.[12] But because the most 'active' part of physics is certainly not the theoretician's office but the experimenter's lab, social studies of science concerned with physics is mostly full of records of the activities of experimental physics, with few attempts to do the same with theoreticians. Compared to the tremendous number of sociological and ethnographic research of experimental practice that was carried out, the extension of the 'practice turn' language found little opportunities to be applied to physical theory.

In a way, this division of labour between philosophers studying theory and sociologists studying experimenters is 'natural': argument and concepts as the dominant elements in theoretical physics 'practice' have always been fodder for philosophical examination, while the human physical action intrinsic in experimental physics serves the same role in sociology.[13] Likewise, philosophers have pointed out that the philosophy of experimental physics is in a rather desolate state, with only a few studies to remedy the lack of work in the field, with emphasis on drawing from social studies of science as sources on which to reflect upon. Radder (2003, p. 2) sums up the situation by stating that "the philosophy of experiment is still underdeveloped, especially as compared to historical and social scientific approaches". In another important work that was partly a call for the development of philosophical reflections on the experimental and material dimensions of science, Hacking (1983, Introduction) mentions that "rationality and realism are the two main topics of today's philosophers of science", with much of the rest of the work emphasising the need to introduce the impact of material dimensions into philosophical accounts of science. Radder (2003, p. 1) considers Hacking's call to arms as having started an initially promising tradition, but which "seems to have lost much of this momentum."

The general situation is then that there is limited sociological insight into the nature of theoretical physics, which given the structuring of physics translates into a rather poor sociological picture of physics as a whole. While the sociology of experimental physics is a rich repository of fieldwork material and analysis that has radically changed the way we understand experiments and even science in general, the sociology of theo-

[12]See for example Latour & Woolgar (1979), Pickering (1992, 1995), Schatzki et al. (2001).

[13]Campbell (1998) contains a critical but quite thorough review of the role of 'action' in contemporary sociology.

retical physics does not exist as a strong, autonomous or continuous tradition. In one of the few papers that aims to "use the laboratory studies approach" to study physical theory, Merz & Knorr-Cetina (1997, p. 73) adopt the same segregation of the theoretical from the empirical seen in positivist philosophy. Early in their paper the authors straightforwardly separate the practice of physics into 'experimental' and 'theoretical', subsequently dividing theory into two classes: 'theoretical, or formal, or mathematical theory' and 'phenomenology'. 'Phenomenological theory' is simply theory that can be "considered rather directly related to experiment", while the 'theoretical theory' simply "is not [directly related to experiment]." How and why this 'relation' is carried out is neither discussed nor explained. Using the concept of 'epistemic culture', Knorr–Cetina (1999, Introduction) has highlighted the "fragmentation of contemporary science " and "the different architectures of empirical approaches", bringing out the "diversity of epistemic cultures", mentioning how this new vision of a disunified science runs counter to traditional assumptions and forces us to rethink our picture of science. Yet in this same work, Knorr-Cetina devotes a single paragraph to the theoretical physics —about its relationship to experiment —and there is no sign that theoretical physics is considered to be a diverse field in the same way that experiment is.

As another example, Pickering (1981) mentions the influence of the phenomenal, but only as far as it touches on experiment. He posits two categories of influence of 'theory' on experiment:

1. The instrumental, in which "debate centres on the apparatus, techniques, procedures and so on, from which scientists distil a set of data in a given experiment."
2. The phenomenal, in which "debate centres on the interpretation of those data".

Pickering thus states that "theoretical conceptions serve both to constitute and transmit prior agreements and disagreements between individual experimental contexts", again delimiting the role of theory as an influence on experimental practice.

Just as Radder has emphasised the need for a philosophy of experiment to complement the existing reflections on theoretical aspects of science and particularly physics, there is a need to develop a sociological account of theoretical physics to balance the existing sociological work on experimental physics.[14] The rest of this chapter is devoted

[14] Knorr–Cetina (1999) acknowledges that "no one has looked, to my knowledge, at contemporary

to establishing a firm theoretical ground upon which this situation may be resolved, in order to develop a sociology of theoretical physics in the same vein as has been done with the experimental side of the discipline. There is to my knowledge no strong tradition or wide literature we could firmly call a 'sociology of theoretical physics', since most of the reference material available is either purely philosophical, or touching on theoretical physics only as far as it is relevant to experimental physics. Although there are isolated works of sociologists exploring pieces of theoretical practice, they remain extraordinary as they are usually departures from the author's usual orientation towards experimental topics.[15]

1.6 Two different epistemologies

Hacking's assessment of the picture of science contained within philosophical studies is correct, but incomplete. Philosophy of science centred almost exclusively on the theoretical aspects of physics, but also on a very limited sort of theory, 'finished' theory. Philosophy has taken up a view of physics mainly through the finalised products of scientific activity, that is, mature and accepted mainstream theories. Textbook, journal or handbook reports are 'the science' with which a lot of philosophy of science informs itself about its object of study. Through the study of finished theories and through its concern with rationality philosophy of science developed extremely close links to a particular field of classic philosophy: epistemology, or the theory of knowledge. Epistemology is the discipline that tries to find the rational roots, the methodology and the criteria for calling a set of beliefs 'knowledge', or more precisely 'good knowledge'. For a long time, philosophers thought that studying scientific knowledge was synonymous with proposing a good theory or a good definition of what good scientific knowledge ought to be like, and in the process to be able to distinguish it from 'other things' that

physics theorising as practical work or at physics' theoretical culture either (the [...] books listed above are on the content of theoretical models)." Ziman (1968, 1978) offered a glimpse into 'the social dimensions of science' from an accomplished theoretical physicist's perspective, but lacking a connection to any sociological tradition, and without a direct relationship to sociological debates. The resonance between his general conclusions and most sociological accounts of physics can be considered supportive evidence for sociology's claims about scientific knowledge, but are more akin to an autoethnographic discussion of physics than a full-fledged sociological work.

[15] See for example Pinch (1980), Pickering (1984), Collins (2007), Merz & Knorr-Cetina (1997) and Kennefick (2000, 2007).

are not science. Philosophy of science is often tacitly assumed to be a branch of epistemology applied to science. If one conceives of philosophy of science solely as the philosophical study of scientific knowledge, then it seems sensible to admit this reduction as plausible. This view, however, clashes severely with the social sciences, which held that the study of science ought not to be centred on knowledge but rather on practice.

A strong criticism that sociologists have held against philosophy of science is that philosophers have generally reduced scientific knowledge to very limited set of things: the professional peer-reviewed publications of scientists, the knowledge found in scientific textbooks, and occasionally the things that scientists wrote about their own fields for outsiders to read. The sociological critique is that this offers a distorted image of what science actually encompasses, and that in most cases these sources do not represent the 'real' nature of scientific knowledge.

Kaiser (2005a, p. 7) offers a parallel critique aimed at historical inquiries into theory, declaring that "most studies have followed in the spirit of a joke that the wisecracking theorist George Gamow was fond of making. Gamow used to explain to his students what he liked most about being a theoretical physicist: he could lie down on a couch, close his eyes, and no one would be able to tell whether or not he was working. For too long, historians and philosophers have adopted Gamow's central metaphor: research in theory, we have been told, concerns abstract thought, wholly separated from anything like labor, activity, or skill. Theories, world-views, or paradigms seemed to be the appropriate units of analysis, and the challenge became charting the birth and conceptual development of particular ideas. In these traditional accounts, the skilled manipulation of tools played little role: theorists were assumed to write papers whose content other theorists could understand, at least in principle, anywhere in the world."

To illustrate, it is a well known phenomenon for sociologists of science that published scientific claims in peer-reviewed journals are rational reconstructions of scientific practice that seldom reflect with any sort of accuracy what actually happened during the production of the knowledge claims found therein. Both experimental and theoretical reconstructions are prone to this sort of rationalisation. This is not to be taken as an indicator that scientists lie, cheat or leave out unnecessary details of their experiments for unethical or dubious reasons. As we will see later on, matters such as limited publishing space, the fruitlessness of recounting every single negative result, etc. can justify the practicality of such approaches. Nevertheless the fact is that the image of an experimental science based solely on the published scientific papers of a scientific field is a distorted image of what goes on in the making of a theory or an experiment. The sociological critique is that a study of science that uses this idealised image to describe scientific knowledge only produces distorted narratives, for it is not a description of science but of scientific results, and these are only a small part of the iceberg of science.

Although there are calls for more 'realistic' philosophers of science that have strongly emphasised the need to take into account "the historical-social context of the experimental natural sciences" this view is far from being the dominant one.[16] Franklin (1994, p. 465) for example wrote in comparing his own philosophically centred work to that of Collins (2004) and its interview-based, micro-sociological account on the same episode of gravitational wave physics:

> I would like to address an important methodological difference between Collins's account and my own. Collins bases his account of the episode almost entirely on interviews with some of the scientists involved. They are not named and are identified only by letter. My own account is based on the published literature. A supporter of Collins might argue that the published record gives a sanitised version of the actual history, and that what scientists actually believed is contained in the interviews. I suggest that the interviews do not, in fact, show the scientists' consideration of the issues raised by the discordant results, and that these considerations are contained in the published record. In this particular episode, we have a published discussion among the participants, in which they explicitly addressed the issues as well as each other's arguments. I see no reason to give priority to off-the-cuff comments made to an interviewer, and to reject the accounts that scientists wished to have made as part of the permanent record. There is no reason to assume that because arguments are presented publicly that they are not valid, or that the scientists did not actually believe them. There are, in fact, good reasons to believe that these are the arguments believed by the scientists. After all, a scientist's reputation for good work is based primarily on the published record, and it seems reasonable that they would present their strongest arguments there.

[16]See for example Radder (1988). Giere (1985) has also called for a similar 'naturalistic' approach to philosophy of science in the same spirit as Quine's naturalized epistemology.

Franklin argues that no favourable argument was presented that the interview mediated, sociological, practice-centred account gives anything of value to the discussion. Moreover, this is hinted to apply not only to this case, but follows the rationale that scientists always present in the published records their strongest arguments. This philosophical fixation on rational reconstructions and, as Franklin called them 'sanitised versions' has impoverished our image of theoretical physics, which makes the development of a sociology of theoretical physics all the more important. It will be one of the principal aims of this work to overcome this image, and to argue in the strongest terms that the heart of theoretical physics lies not in the post-sanitisation results that are Franklin's object of analysis, but rather in the process that has the publication of the sanitised accounts as its very last stage, a stage that is not representative in any way of what 'doing theory' is like. All professional bodies have an internal working structure, and an external, public façade that is very different to how the professional work is actually carried out. Science is no different to any other profession in this respect. In an interview with distinguished theoretical physicist Michael Berry, the relationship between theoretical physics' 'polished' form and its inner working was considered in some detail. In our first interview round, Berry offered the semi-automatic, expected, Franklin-like answer to my question of how to understand theoretical physicists' work. He literally recommended me to "go and read the published literature". However, after a bit of friendly confrontation, the initial off-hand answer changed radically:

> Reyes: Yesterday at the beginning of our interview I asked you 'what is it that theoretical physicists do?' and you said that if one wants to know, it's very easy, you just have to go look at the papers.
>
> Berry: Yes, then you told me— and I agreed with you— that this doesn't give a clear picture of how you do things. It gives a picture of what you've done, beyond the results. It doesn't always give a clear picture, as with any creative activity. It's the phrase, 'Art that conceals art'. Which is fine. People don't want to know when they go to a music concert; they don't want to hear all the practicing of the scales, up and down, that the musician has spent hours and days doing.
>
> Reyes: What would be your equivalent of the practicing of the scales, and what doesn't go into your final work?

Berry: Lots of little details of calculations that don't go in the paper. I always make sure, because I have a bad memory, that I write enough that when I come back some years later I could reconstruct the argument. I can't always do it but mostly I can. Anybody who is technically competent will have enough information to pick up unusual steps, but all the rest are routine things I don't bother to write down. So my equivalent of these scales is ...you know, I often like to just by myself reproduce old arguments. I forget how to do things and get out of practice and so I do something to reproduce some result and remind myself. I don't publish that, I just do it for myself. Often, as part of something big. If I'm doing some problem and I need a result which I remember, I'll sometimes stop and derive that result myself without going back, for the exercise of doing it. Like we discussed yesterday, practicing. It's very much like musicians with their scales.

Reyes: So if I read your papers and I tried to say how it is that you do your work, is that the only part that's missing?

Berry: Well, I never thought about it. Well, of course there's the whole culture of science and theoretical physics in particular that lies behind things. It depends who I'm writing it for. So of course if I'm writing a technical paper I don't explain every technical term if it's standard. If I say 'Schrödinger's equation' I don't go and write it down, or reference it in the paper and describe it. People know it. If I'm giving a public lecture or writing something non-technical then I wouldn't just assume that those things are known. It depends on the audience how much is left out. Because it's tiresome to go back and you can't do it, to go right back to the principles. Science is communal. There's a body of commonly understood work, and I don't bother to repeat that. [...] When I read a paper that contains huge amounts of review and if it's not a review paper, I often say, 'You don't need all this. You've got to cut it out because, first of all it wastes space, but not only that, you're obscuring the original thing that you've done by going back and repeating all these references which are well known. Anybody who is going to understand your paper will know this. So leave it out; only a few carefully chosen sentences to set the context.

But you don't need to repeat.'

Unlike a concert-going member of the public, a 'sociologist of music' would be interested in seeing a musician learn his trade and practice his scales. If one were only interested in the rational reconstruction of theories (or the 'performance reconstruction' of music) then it would make no sense to see what goes on behind the scenes of a concert. For a sociologist, such limitations are contrary to the professional trade. The question posed by the Franklin-Collins alternatives can be put in these simple musical terms: who 'knows' the practice of music better, the musical aesthete that listens solely to studio-sanitised CD versions of a band's performance, or the groupie that sometimes sneaks backstage into a band's practice jam, and clicks pictures of the musician's both on- and off-stage? Of course, if 'music' is purely the enjoyment of a perfectly recorded performance, it is the philosopher's whose case is stronger; otherwise, sociology is the better alternative.

Putting published papers aside, a similar thing happens with accounts that rely on textbooks, but in a more extreme way. Peer-reviewed publications are full of results that will most likely never have great impact in the larger community. Of those that do have impact, even fewer will transcend time, and all but the most minuscule percentage will then make it to the textbooks. When and if they do, entire research programs and traditions are reduced to snippets of the type, "The fundamental observation, that magnetic fields exist in the neighbourhood of currents, and hence of moving charges, was made by Oersted in 1819". The simplifications inherent to these characterisations, whatever pedagogic purpose they may serve for physicists, renders them vacuous for the purpose of gaining any understanding of scientific practice and its creative dimensions. Kaiser's work must be mentioned again because although it relies heavily on analysis of textbook, it does so very differently by analysing the manner in which textbooks have presented particular theoretical tools diachronically. Kaiser (2005a, ch. 7) argues that although the changes in theoretical tools lag behind their real-time use at the research level, a historical analysis of textbooks can give a good picture of the evolving trends and usages of theoretical tools.

The moral that sociology has drawn from these observations, that practice cannot be reduced to rational reconstructions in the forms of scientific publications, or is isomorphic to them in any reasonable manner, is that in order to study science properly a different approach than the philosophical one has to be taken. Hence social studies of science has become preoccupied with case studies of actual scientific activity, and concentrated mainly on the labours of laboratory science. Sociologists in particular are interested in how scientific knowledge claims can come about despite the fact that science is a messy affair far removed from the clear-cut rationality of either positivism or their early commentators.

1.7 Two different definitions of knowledge

The differences between philosophical and sociological accounts of science are not limited to the aspects of science they concentrate upon, rational reconstructions for the former, and social action and practice for the latter. Philosophy and sociology hold completely different definitions on the 'nature' of scientific knowledge. In most cases, when philosophers talk about scientific 'knowledge', they mean something very different to what a sociologist would understand. Since both disciplines are after all concerned with understanding how scientific knowledge is created, one must understand what the epistemological underpinnings of each one are.

The classic philosophical definition of knowledge is that 'knowledge is true and justified belief' (the TJB definition). Although this is by no means an unchallenged and untroubled definition within classic epistemology, as shown most clearly by Gettier's celebrated counterexamples, it does capture the essence of what is commonly held as knowledge by philosophers.[17] Philosophical discussions upon the subject typically take

[17] In his landmark Gettier (1963), the author offers examples of situations where subjects could be said to hold beliefs that are both true and justified, but that intuitively cannot be said to correspond to actual knowledge . Basically, Gettier's counterexamples involve situations where the epistemic subject believes that something is true and the person can exhibit good enough reasons to hold that these beliefs are true (i.e. the beliefs can be openly and sufficiently justified by the believer on the outset). Alas, the reason why the beliefs are true are not in tune with the epistemic subject's explanation, but because of a completely different causal connection than that given by the believer. Gettier's counterexamples show that there may be cases when an epistemic subject can believe something which happens to be true, and give sufficiently good reasons for that belief, and yet for an outsider the belief could hardly say to constitute knowledge because those are not the actual reasons why the belief should be held. Gettier's counterexamples are a bit contrived, but they have put epistemology in hot water. The first class of counterexamples goes along these lines: before the next World Cup starts, I claim to my friends that the statement "the best football team in the world will win the next World Cup" is a plausible knowledge claim, because I believe that England is the best team in the world (say, by FIFA rankings) and that they will definitely win the World Cup having gone unbeaten for one hundred straight matches. Eventually, Mexico wins

it as its starting point. Moreover, it matches our mundane conceptions, our everyday idea, of what we usually think of as knowledge. The Oxford English Dictionary for example includes the following definition for 'knowledge':

> 1. Acknowledgement or recognition of the position or claims (of any one). 2. The fact of recognising as something known, or known about, before; recognition. 3. The fact of knowing a thing, state, etc., or (in general sense) a person; acquaintance; familiarity gained by experience. 4. Personal acquaintance, friendship, intimacy. 5. Acquaintance with a fact; perception, or certain information of, a fact or matter; state of being aware or informed; consciousness (of anything) [...]

From the OED definition and from the TJB perspective, it makes sense to talk of knowledge in terms of particular epistemic subjects — individual persons — notice words such as "recognising", "personal acquaintance", "acquired", "experience", "understanding" in the OED definition. This philosophical subject-centered definition talks about beliefs, acquaintance, and other personal states of the mind. In traditional epistemology and in our common manner of speech one may very well speak of a person that knows certain propositions, and call those propositions knowledge. Knowledge in philosophical accounts refers to an individual's actions, states of mind, and beliefs.

A more clear example can be gained by browsing modern, professional, epistemological texts, where in exemplary expositions it is standard usage to begin discussions about knowledge-claims with phrases such as "suppose that Tiago believes that ..." and work around the facts surrounding Tiago's beliefs, Tiago's utterances, Tiago's claims, etc. and examine them in the light of the TJB definition (or whichever alternative epistemic criteria is alternatively proposed).[18] Similarly, in everyday speech one is inclined

the next World Cup, and being the best team in the world at that time (according to say, a public opinion poll), my previous statement turns out to be both true (the best team won) and justified (by the information available at the time, it was a well supported claim). But one could hardly say that my statement was really knowledge. It just happened to be true by *coincidence*.

[18]Even texts that try to subvert the traditional definition, such as Gettier's, insist on this subject-centred position; Gettier wrote about the beliefs of particular men — Smith and Jones, Jones and Brown— and their subjective beliefs. Although some accounts from the relatively new school of 'social epistemology' have attempted to grasp the concept of a 'collectivity-based' epistemology, these are rather crude attempts that have ignored basic sociological investigations on the subject; see for example Goldman (2010) and Strevens (2010).

to say that one knows something whenever one feels confident in one's beliefs or in the facts that one is aware of, and call that knowledge.[19]

1.8 The epistemology of the social analysis of scientific knowledge: thought collectives

In another example of how he anticipated contemporary sociological accounts of science, Fleck (1935) noted that statements, data, observations, theories, principles and concepts do not become 'true' by the effect they have on individual minds, but by the way in which they are taken up by relevant social groups and become accepted en masse. According to Fleck, accepted statements only become knowledge when they become part of *thought collectives*. In explaining the development of the concept of 'syphilis', (Fleck, 1935, p. 41) noted that "not only the principal ideas, but all the formative stages of the syphilis concept, however, are the result of collective, not individual effort. Although we spoke of Schaudin as the discoverer, he really no more than personified the excellent team of health officials whose work [...] cannot be easily dissected for individual attribution." Fleck argues that even when individuals working on their own make discoveries, they only become widely accepted as truths once they are incorporated into the social pool of accepted knowledge. In this sociological epistemology, a subject's claim-of-knowing is no longer 'knowledge' for the element of analysis is a socially and

[19]In the holistic Russell (1946, p. 516) the author notes that there are two elementary traditions in modern philosophy concerning the relationship between knowledge and the knower. Cartesian methodological doubt is the foundation for the first tradition, whereby the world is abolished and all that is left to base knowledge upon is the subjective mind. "Most philosophers since Descartes have attached importance to the theory of knowledge, and their doing so is largely due to him. 'I think, therefore I am' makes mind more certain than matter, and my mind (for me) more certain than the minds of others. There is thus, in all philosophy derived from Descartes, a tendency to subjectivism, and to regarding matter as something only knowable, if at all, from inference from what is known of mind. [...] Modern philosophy has very largely accepted the formulation of its problems from Descartes, while not accepting his solutions." The second tradition is empiricism, which Russell introduces mainly through the work of Hume, whose most salient criticism against Cartesian philosophy is elaborated through the elimination of the 'Self'. Hume notes that even when going into the deepest sort of deep self-reflection, one never really experiences 'oneself', but only a series of sensations and perceptions. "[T]he self, as defined can be nothing but a bundle of perceptions. In this I think that any thoroughgoing empiricist must agree with Hume." Thus, the empiricist tradition supposes that the foundation for any knowledge, even philosophical and psychological knowledge, can only come from observation. However, Russell also argues that the continuation of the Humean argument (which Hume himself in the end abandoned) leads to radical scepticism and an even deeper subjectivism, and is a philosophical cul-de-sac.

collectively constructed truth-reservoir, and not the individual idiosyncratic belief.

The idea of knowledge espoused by Fleck resonates once more with the philosophical project set out by Wittgenstein. Wittgenstein (1953, §43 p. 18e) recommended that in order to understand a word's meaning, philosophers should turn away from trying to capture what the word represents, and rather focus on how it is used: "For a large class of cases — though not for all — in which we employ the word "meaning", it can be defined thus: the meaning of a word is its use in the language. And the meaning of a name is sometimes explained by pointing to its bearer." Sociological analyses of knowledge have embraced this proclamation, rejecting traditional epistemological analysis of knowledge claims and instead focusing on the question of when and how statements, belief and data sets produced by individuals become linguistically incorporated into the pool of accepted scientific knowledge. Thus, 'scientific knowledge' for social studies of science is not the set of beliefs that the philosopher can give good reasons to believe himself; scientific knowledge is the set of statements that scientists (as a collective) choose, declare and demand to call knowledge. As the pioneers of sociology of scientific knowledge noted early on, the role of the sociologist is to develop a naturalised epistemology that is not concerned with what should or should not be called knowledge in traditional terms, but on what is in fact called knowledge by those who produce it.[20]

For the Wittgensteinian epistemological project, the phrase "Tiago believes that ..." cannot lead to a conclusion about knowledge, but only about what Tiago as a person believes to be the case. That by itself is not sociologically defined knowledge, because it does not incorporate any notion of how the individual actually uses that bit of information within a social context. Returning to Fleck and Wittgenstein, the usage of a concept, of that bit of knowledge, is established by social convention, and thus 'knowledge' cannot be analysed outside its social context in any meaningful way. The individual is of no concern other than as an embodiment of socially-instantiated knowledge, whose location is within communities and epistemic collectivities.[21]

[20]Barnes (1977, ch. 1)

[21]Sociologists would not say that one definition is better than the other, but that in each case 'knowledge' is referring to two very different though connected phenomena. In everyday life we say 'I know' to mean 'personal-knowledge', but also use 'knowledge' to mean 'collective knowledge'. Perhaps if there was a widespread substitution of the verb 'to know' (as in 'I know that...') for something like the Scottish verb 'to ken' the confusion might not arise, but this is of course impractical.

1.9 Matching theory and experiment: contemporary philosophy of science

Philosophical studies of science have undergone a deep transformation during the past two decades.[22] An important and often referenced publication in the field is van Fraassen's *Scientific Image* and its proclamation that the aim of science is 'to save the phenomenon', i.e. to account for the empirical facts. This central tenet of van Fraassen's 'constructive empiricism' was adopted by other philosophers and produced an important disciplinary rearrangement that brought philosophy of science closer to 'phenomenological' physics, as can be seen for example in the work of Cartwright (1983) and Giere (1990, 1999). All these authors proclaim that experiment ought to be retaken as the primary guidance for knowledge creation in physics, while also downgrading the role of high-theory. Cartwright (1983, p. 8) for example writes, "I have repeatedly said I do not believe in theoretical laws" and Giere proposes a version of 'science without laws'. For this philosophy the new approach is to highlight the role of models in physics, mathematical statements with only localised validity that are created with the sole purpose of accounting for the phenomenal world. As Lenoir (1988, p. 22) explains, this new phenomenologically oriented philosophy tries to address the situation that "experiment, instrumentation, and procedures of measurement, the body of practices and technologies forming the technical culture of science, have received at most a cameo appearance in most histories. For the history of science is almost always written as the history of theory".

In this way contemporary philosophy of science has traded 'model' for 'theory' as its central point of interest. This is fortunate because it has put the importance of modelling real phenomena in physics into the spotlight, and model building had been a piece of theoretical practice that had indeed long been neglected by science studies. Nevertheless, in doing this many philosophers of science seem to have swung the pendulum to the other extreme, declaring theory dead, which very much contradicts the role that pure theory holds within the world of physics.

Talking to theoretical physicists shows that many do indeed devote a good deal of their time to exploring models and experiments rather than concentrating on pure the-

[22] See Portides (2011) and references therein for a more comprehensive overview.

ory. Although phenomenology is often seen as second-rate theoretical physics, in terms of the number of phenomenologists versus pure theoreticians, it is equally if not more significant. As a necessary bridge between autonomous theory and experiment, phenomenology, applied physics, computer simulations of theoretical predictions, computer simulations of experimental setups, and the interpretation of the data churned out by laboratories is massive work. This is not meant to undermine the role of pure theoretical physics, but rather to highlight the importance of all the pieces of the horseshoe diagram in holding modern physics together. Thus, an initial purpose of this work is to highlight the equal importance of phenomenology, theoretical and experimental physics, in order to understand the the connections between these domains.

CHAPTER 2

Theoretical styles

The senses are too gross, and he'll contrive
A sixth, to contradict the other five,
And before certain instinct, will prefer
Reason, which fifty times for one does err;
Reason, an ignis fatuus of the mind,
Which, leaving light of nature, sense, behind,
Pathless and dangerous wand'ring ways it takes,
Through Error's fenny bogs and thorny brakes;

— John Wilmot, Earl of Rochester, from 'A Satyre Against Mankind'

2.1 Theoretical thought styles between theory and experiment: phenomenology

This chapter will focus on *phenomenology*, theoretical physics that lies between fully theoretically and fully empirically oriented physics, in order to illustrate how the different micro-cultures of the lower half of the horseshoe diagram — understood as types of Fleck-type thought styles — shape theoretical practice. It will also exemplify how the disunity thesis arises in theoretical activity and theoretical discussions, and thus allow the disunity between different micro-cultures to be better appreciated.

Phenomenology is concerned with creating descriptions of actual empirical phenomena. However, theoreticians approach empirical phenomena from different angles. I identify three main 'styles' for carrying out empirically-oriented work, and for creating descriptions of empirical phenomena, which I have termed *first-principle modelling*, *data-fitting modelling* and *simulation*. These are the thought styles that make up the central-lower portions of the horseshoe diagram, at that form the interface between pure theory and pure experiment.

Knorr–Cetina (1999, p. 1) has proposed a category that is similar to Fleck's thought styles in her concept of 'epistemic styles', which she defines as "those amalgams of arrangements and mechanisms — bonded through affinity, necessity and historical coincidence — which, in a given field, make up how we know what we know. Knorr–Cetina (1995) argues that the notion of epistemic cultures draws attention to the fact that different scientific communities have their own 'strategies' and 'styles' of consensus-formation. In other words, the reasons that scientists give to justify their knowledge claims can vary from group to group, and the different modes of justification can be grouped together into distinct classes. The first-principle style and the data-fitting style of modelling can be seen as an extension of Knorr-Cettina's analysis to theoretical practice. An interesting point is that while Knorr-Cettina has posited 'simulation' as a form of experimental style, it is mostly theoreticians that carry out simulations in physics (although these same theoreticians constantly highlight how simulation has many of the same characteristics of experimental practice).

The principal *products* of phenomenological theoreticians' work are models/theories (the ambiguity between these concepts will be discussed from a sociological standpoint) and simulations. I will first examine the general use of models in physics and in a later section that of simulations.

'Modelling' in physics is generically used to denote a theoretical description of a piece of the physical world, as opposed to theory that deals with the actual phenomenal and tangible world very indirectly. Models and their role in theoretical physics have received a lot of attention from science studies, particularly from philosophers, and thus Bailer-Jones (2003, p. 59) writes that "scientific models represent aspects of the empirical world", while Cartwright (1997, p. 292) states that models "mediate between our various parcels of general and specific scientific knowledge and the world that that knowledge is about". Scientific models are therefore generally understood as theoretical means to represent the phenomenal world, where 'representation' should be taken in the widest sense possible.

2.2 Sidestepping the philosophy of models

Concerning scientific models, contemporary philosophy of science tends to take a more practice-based approach than is typical for the field. As Knuuttila et al. (2006, p. 4-5) explain, although "there has been some division of labour between philosophers of science and STS researchers [...] the studies of models by philosophers and STS scholars can be seen to interact with, intersect and complement one another, with the practice-orientation laying out a bridge between the two". Frigg & Hartmann (2009) for example focus on the utility of models in different practical contexts rather than turning to ontological, essentialist or methodological categories, partitioning the usage of models into: the role of models as *representational tools* of real phenomena; the role of models as *data-organisers*, so that a model is a "corrected, rectified, regimented, and in many instances idealised version of the data we gain from immediate observation, the so-called raw data"; models as *instantiations of a formal language*, in the sense of mathematical logic, where "a model is a structure that makes all sentences of a theory true, where a theory is taken to be a (usually deductively closed) set of sentences in a formal language".[1]

Yet the semantic multiplicity of models is complex enough that 'model' — for both scientists and science studies scholars — can mean, amongst other things: idealised descriptions of the physical world, half-baked descriptions of the physical world, approximate descriptions of the physical world, preferred descriptions of the physical world, standardised descriptions of the physical world, pedagogical illustrations of how to describe the physical world, archetypes of the physical world, explorations of the physical world through theory, amongst many, many other uses. Or as philosopher N. Goodman describes the situation, 'model' is an extraordinarily *promiscuous* term, so that "a model is something to be admired and emulated, a pattern, a case in point, a type, a prototype, a specimen, a mock-up, a mathematical description — almost anything from a

[1] Electronic resource, no page numbers.

naked blonde to a quadratic equation — and may bear to what it models almost any relation of symbolization".[2] For example, philosopher such as Black (1968) have embraced this richness of meaning by linking 'models' to the linguistic object of 'metaphor', itself a semantically exuberant concept. Rather than trying to work out a pseudo-philosophy of models and attempt to address questions concerning what models 'are' or the general way in which models are used in a general framework of science, I will try to draw out the essential *sociological* features of models and simulations.

In order to avoid getting lost in discussions that might obscure the sociological dimensions of phenomenology, it is necessary to start out from as simple a working-definition of models as is possible. As mentioned previously, the most general feature of all models is that they are the media through which theoreticians incorporate the physical world *directly* into their work. But — a cunning reader may object — isn't this what *all* theoretical physics is about, describing bits of the physical world ?

To proceed in understanding the use of models, I will juxtapose them against another important theoretical product that is tied to the phenomenal world: theoretical laws. When theoreticians refer to *the laws of physics* they typically mean statements about the physical world that are universally acknowledged — for all practical purposes — beyond doubt. I will briefly examine a paradigmatic example, the Law of Conservation of Energy, also known as the First Law of Thermodynamics. Although it can be stated in a variety of ways, a typical one is the following statement by Fermi (1936)

> *"The variation in energy of a system during any transformation is equal to the amount of energy that the system receives from its environment during a transformation".*

An important feature of the First Law is that it is, in the strictest sense, universal. It is not valid for one particular system, or class of systems but for *every* physical system in existence in the past, present and future. Feynman et al. (1964, Section 4-1) describe a law as being "a fact [...] concerning *all* natural phenomena that are known to date. There is now exception to [a] law — it is exact so far as we know". As Feynman explains concerning the First Law in his fantastic 'Dennis the Menace parable', when there seem to be 'violations' of the First Law, physicists move all sorts of resources — intellectual

[2]Cited in Winsberg (1999).

and material — in order to find flaws in the analysis, argument or experiment that lead to the cancellation of the violation. Feynman describes the conservation of energy as analogous to a conjecture by Dennis the Menaces' mother when Dennis keeps losing toy construction blocks. When Dennis loses a block (analogous to when a physicist 'loses' a bit of energy which appears as a the violation of the First Law) the mother conjectures that it *cannot* have simply disappeared. If the mother then looks hard enough, then the missing blocks will be found, under a rug, behind a sofa, etc. If there are missing blocks, or extra blocks, the mother may be sure that Dennis has brought them from or taken them from outside the room, and thus that 'the Law of Conservation of Blocks' has not been broken. According to Feynman, this is how physicists go about *using* the First Law, by *believing* in it as a hardcore and honest truth. Moreover, says Feynman, "it is important to realise in physics that we have no knowledge of what energy *is*. [...] It is an abstract thing in that it does not tell us the mechanisms or *reason* for the various formulas" (emphasis in the original text).

2.3 Theories and Truth, Models and Contingency

Laws in physics are thus statements that are, for all practical purposes, beyond doubt. But this doubt cannot refer simply to *personal* doubt, as one *can* find instances where particular physicists may be momentarily convinced of a potential violation. It is *collective* truth status that a law commands, and that orients physicists in finding strategies to 'fix' anomalous violations. A physical law is not 'true' by personal conviction (although it can of course be probed by individuals), but made 'true' by collective belief *through* generalised personal conviction. To use a phrase coined by Zimmermann & Thorne (1980), a law like the First Law of Thermodynamics is one of the physics community's *most cherished beliefs*, one that would be hard to give up without good reasons despite the fact that, like all scientific knowledge, physicists are aware that it *may* be fallible. 'Theory' is thus sociologically standardised belief. Theories, as generalised and universal beliefs thus contrast strongly with models as more *localised* descriptions of the physical world, which in their idiosyncrasy necessarily have a lower level of universality, and of belief.

But truth, as sociology of science knows full well, is a category that evolves with time, and this is reflected in the eclectic usage of both 'law' and 'model' referred to earlier. Emch (2007) has used examples from physics that reflect the semantic flexibility given to the distinction between 'model' and 'theory' in its real world usage. Emch cites the Stanford Linear Accelerator's webpage description of the Standard Model of particle physics:

> The Standard Model is the name given to the current theory of fundamental particles and how they interact. [...] Today, the Standard Model is a well-established theory applicable to a wide range of conditions. [...] One part of the Standard Model is not yet well established. [...] Thus, this one aspect of the Standard Model does not yet have the status of theory but still remains in the realm of hypothesis or model.[3]

Frigg & Hartmann (2009) similarly note that "in common parlance, the terms 'model' and 'theory' are sometimes used to express someone's *attitude* towards a particular piece of science. The phrase 'it's just a model' indicates that the hypothesis at stake is asserted only tentatively or is even known to be false, while something is awarded the label 'theory' if it has acquired some degree of general acceptance" (emphasis added). Yet Frigg & Hartmann add that "this way of drawing a line between models and theories is of no use to a systematic understanding of models"; while possibly true for philosophy, this is definitley untrue for sociology of science. The essential sociological distinction between models and theories *is* in fact the manner in which this line is drawn.

Further elaboration on this point is given elsewhere in SLAC's internet site, in a page that is titled "Is the Standard Model a theory or a model?", which again illustrates how models seep into scientific language as less stable forms of knowledge:

> To scientists, the phrase "the theory of..." signals a particularly well-tested belief. A hypothesis is an idea or suggestion that has been put forward to explain a set of observations. It may be expressed in terms of a mathematical model. The model makes a number of predictions that can be tested in experiments. After many tests have been made, if the model can be refined to correctly describe the outcome of all experiments, it begins to have a greater *status* than a mere suggestion. (emphasis added)

[3]Stanford Linear Accelerator Virtual Visitor Center: The Standard Model, `http://www2.slac.stanford.edu/vvc/theory/model.html`.

Nevertheless, the same text points towards the sometimes ambiguous use of both 'theory' and 'model'. In particular, it discusses how the Standard *Model* of Particle Physics — one of the crowning achievements of 20th century physics that aims to describe *all* the fundamental interactions between matter/energy — is in reality a *theory*:

> Scientist do not use the term "the theory of..." except for those ideas that have been so thoroughly tested and developed that we know there is indeed some range of phenomena for which they give correct predictions every time. (But, language being flexible, scientists may use "a theory" as a synonym for "a hypothesis", so listen carefully.) [...] The fact that we have a theory with the name "Standard Model" is a bit peculiar. There were a number of similar competing models. The one that kept passing the experimental tests became the Standard Model and eventually this became the theory of fundamental particles and their interactions. Physicists continue to use the name Standard Model, but add capital letters to denote its status as more than just a model![4]

So in fact at some point in time the Standard Model was 'just a model'. As a theoretician who is a specialist in Standard and beyond-Standard Model theory briefly summarised:

> Mondragón: If you look at how the Standard Model was built, it was built phenomenologically; this symmetry works here, this one doesn't, etc. That is how physics is done. That is how physics is built. The Standard Model that now everyone accepts was made by trial and error: "this symmetry works, but this gives me more information; this charge is not as I thought..."

The transition of the Standard Model from being classified as a 'model' to a bona fide 'theory' was simply a reflection of it's mutation from *hypothetical* data organiser that competed with other models, into standardised commonly-accepted physicists. In perhaps the most detailed historical account of the development of the Standard Model,

[4]Stanford Linear Accelerator Virtual Visitor Center: Is the Standard Model a theory or a model?, `http://www2.slac.stanford.edu/vvc/theory/modeltheory.html`.

Pickering (1984, p. 46, 60) has identified two stages in the theoretical development of high-energy physics theory, the initial stage being clearly an empirically-driven *phenomenological* phase, and the second one being guided mostly by a 'fundamental theory' approach.

Pickering refers to the first stage as 'the old physics' of the early 1960s, which "was characterised by its common sense approach to elementary particle phenomena. Experimenters explored high cross-section processes, and theorists constructed models of what they reported." In fact, the construction of these models was a massive effort of *data-fitting,* where "the use of conservation laws, symmetry principles and group theory brought some order into the proliferation of particles". Pickering shows how the field-theoretic, symmetry-based explanations that laid the ground for the Standard Model appeared as a result of trying to gain further insight into the 'population explosion' of particle physics whereby the number of fundamental particles discovered in accelerator experiments grew from a handful to over seventy types in just a few years. Pickering points out that the 'symmetry' school that gave rise to the Standard Model was one of two major theories to tackle the population explosion problem, and that in fact by the later part of the '60s the alternative classification known as S-matrix/ bootstrap theory dominated the field in terms of publication numbers. Eventually this alternative was surpassed by the field-theory/symmetry approach, but during the late 1960s the nowadays dominant field theory approach was not yet established truth.The most sociologically relevant point is the transformation in terms of levels of belief. In time, the theoretical data-fitting model started gaining distance from experiment and, importantly, started making successful predictions of unseen phenomena, until eventually it mutated into a 'theory' that could advance without being tied to the data fitting process.

The differentiation between models and theories as more or less stable elements of belief are found throughout theoretical physics' parlance, and descriptions of the Standard Model as a 'permanent achievement' is quite common. Weinberg (1998, p. 6) for example explains how "after our theories reach their mature forms, their hard parts represent permanent accomplishments. If you have bought one of those T-shirts with Maxwell's equations on the front, you may have to worry about its going out of style, but not about its becoming false." The Standard Model is one of such permanent accomplishments. At this point in the history of physics, the Standard Model is acknowledged to be the 'best' theoretical explanation available to theoreticians, and so it acquires airs of 'truth' and 'theory' rather than of a tentative 'hypothesis' or a 'model'.

There is of course a certain tension between seeing a theory as 'true' — which is a scientists' natural attitude towards their work, and seeing it as merely 'socially fashionable' which requires a second-order analysis of the discipline.[5] But as the following exchange illustrates, this is at the heart of the semantic flexibility and occasional interchangeability of 'model' and 'theory'. In it, the interviewee rapidly and often switched from 'model' to 'theory' when describing theoretical elements that were outside/within his own area of expertise:

> Reyes: What is a theoretical model? Could you give me an example of one?
>
> Fairhurst: Sure. String *theory* is one. It's a huge *theory*. [...] It's a *model* of possibly the entire Universe. [...] People use these theoretical models of galaxy formation and star formation to predict the rate of black hole mergers there are going to be in the Universe. It's not directly based on observation, or it may have some observation input, but then there's some theoretical model where you have to assume things which aren't measured. I guess the other big one is QCD; they're looking for the Higgs. So the Higgs has been theoretically predicted, but it's not been found. (emphasis added)

In order to highlight this ambiguity, I tried to juxtapose the dichotomy of model versus theory, which the interviewee immediately understood, although he again entangles their usage:

> Reyes: It seems funny that you call string theory a 'model', because I think many of the guys that do string theory would call it a 'theory', no?
>
> Fairhurst: But it's a theory to model the Universe, right? Strings theorists are on the borderline. Half of them are in maths departments and half are in physics, and so some of the guys in maths departments are just doing 'really cool maths.' [...] Some of the ones in the physics department are

[5]Where *natural attitude* should be understood as in the traditional spirit of phenomenological analysis, that is, the suspension of doubt concerning the immediate social and material world; see Schütz (1932, p. 98).

> actually claiming and trying to work out how you can take this elegant theory and predict something about the Universe, something we could measure, be it in the very early Universe, at very high energies, or very small length scales, whatever. In that sense it's a model, a model of the Universe. Just like Newton's gravity is a model of the Universe. [...] 'Theory' and 'model' are almost interchangeable.[6]

Fairhurst here mentions another important element that gives 'theories' their power: prediction (and retrodiction). As prediction extrapolates the applicability of a 'theory' beyond merely a localised set of data, it is a constitutive element of building up theoretical belief in what is initially a model (as Pickering points out concerning the Standard Model). Continuing from the above Fairhurst returned to the topic of a theory as an attempt to "model the universe". Nevertheless, when referring to his *own* work in General Relativity — where GR is the basic and unchanging mathematical framework that needs to be taken as the natural attitude simply in order to begin computational work — he identified GR as a theory:

> Reyes: When you were doing things as a pure theoretician, were you working with models or were you working with theories? How would you put it?
>
> Fairhurst: It was within General Relativity, which I guess I would call a theory. And I guess *for the same reasons string theorists would call string theory a theory*. It is trying to model the Universe. What we tried to develop was a theoretical model of black-hole horizons. There you go, I used both words! It's not 'these are theories and these are models and I can draw a line between them.' (emphasis added)

To sum up this section, theoreticians' usage of the terms 'model' and 'theories' is highly flexible, but nevertheless exhibits two general features. The first are mostly understood as empirical, localised and hypothetical pieces of theory, while the second

[6]Notice how Fairhurst is again pointing to the connection between models and the empirical world. Nevertheless, the standard SM textbook Cottingham & Greenwood (2007, p. 153) unambiguously identifies the QCD as being a theory, "In the Standard Model, the strong interaction also is described by a gauge theory. [...] The theory is known as quantum chromodynamics (QCD)".

are usually reserved for more stable theoretical statements. Of course, what is 'stable' within a form-of-life is a strongly-socially mediated category, with 'stability' encompassing the elementary parts of an individual's social world. For Fairhurst, working as a data-analyst and simulator requires him to suspend belief in General Relativity, speaking of it as a theory. String theory, which lies outside of his immediate social world, can then be seen as a model, although he clearly understands why a string theorist himself would posit it as a theory: it is part of each form-of-life's immediately stable knowledge.

2.4 Two theoretical styles of phenomenology

Recalling Knorr-Cettina's 'epistemic styles', we can now identify two epistemic approaches to modelling, at the interface between pure theory and phenomenology. On the one hand is a 'theory' driven approach, where theory is to be understood as in the above sections, that is, as socially stabilised belief in theoretical-mathematical laws. Once a theoretical law-like belief-system becomes stable and mathematically formalised within a community (what could broadly be described as a Kuhn-type paradigm), then a theoretician can proceed in terms of 'applying' this paradigm to derive results that match empirical observation (prediction or retrodiction). Theoreticians who fundamentally place *belief* in an established piece of theory and its 'principles' work within what I will refer to as the *first-principles style*, in reference to the way that mature theories tend to be stated in terms of mathematical axiomatisation-like schemes (e.g. Newtonian dynamics and the Three Laws, Maxwell electrodynamics and the four Maxwell equations, quantum mechanics and its four 'postulates' and bridge principles, etc.) The first-principles style thus places belief in this method of production of theoretical results, or paraphrasing Knorr-Cettina, the belief in the method and the postulates allows theoreticians to understand "how they know what they know". This style pulls on the lozenge labelled 'modelling' in the horseshoe diagram towards pure theory.

On the other hand, phenomenology may be driven by what I term a *data-fitting style*, or a *belief* in the primacy of matching theory with experimental results, possibly in determent of established first principles. Thus, if an 'anomalous' experimental result seems to contradict a stable theoretical description, a data-fitter would have no problem revising what is apparently stable theory in order to achieve empirical adequacy. For example, a theoreticians could introduce non-trivial modifications to an established

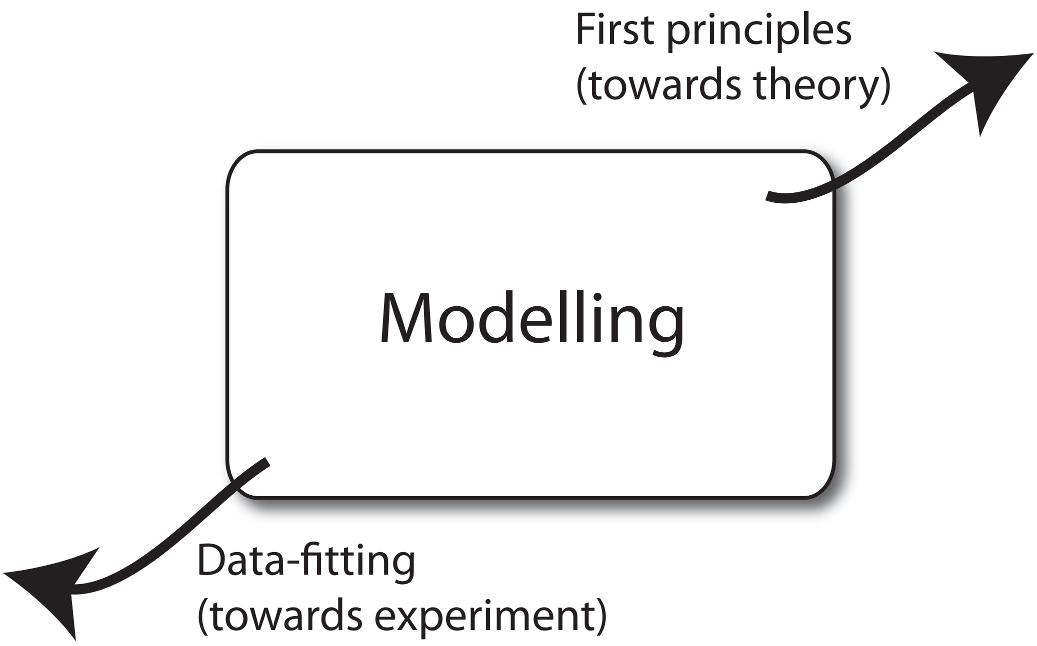

Figure 2.1: The tension inherent to 'modelling' is due to the convergence of two thought styles: data-fitting and first-principle theorising.

theory (or a semi-stable theory/model), even if those modifications have no immediate 'physical' justification. This does not mean that a data-fitter does not care about producing theory that is physically justifiable in terms of first-principles, but only that the creation of theory is more closely connected to the empirical tradition than to pure theory.

These two styles can be visualised in terms of the *tension* they create on modelling as a whole. In fact, the social worlds of the two styles are so interconnected that placing 'modelling' within the family of 'high theory' is only a mere convention if one focuses on the first-principle approach, as it could also be seen as part of phenomenology if emphasis is laid on the data-fitting efforts. This is encapsulated in Figure 2.1. Summing up, these styles are characterised as:

1. First-principle style: the justification of empirical claims follows an 'axiom-theorem' account of truth that borrows terms from logico-mathematical parlance. In this case the modeller believes that the fundamental 'axioms' of a theory, i.e. the 'laws' that govern the system can be firmly established, and that once these laws are stated one only has to find the actual description of the system or the initial

conditions and solve a set of equation to obtain the necessary solutions. The idea is connected to mathematical foundationalism, so that if the axioms are 'true', the truth value is inherited by all the theorems derived from them. For physics, this leads to the belief that if the laws or first-principles are true, then all models that are consistent with the first-principles are valid descriptions of a possible reality.

2. Data-fitting style: in this case, the belief is placed most strongly on the data. One supposes that the data is a true and accurate description of a physical system's properties. If a set of equations can be made to fit this data, then it is a good model of the systems, and the truth-value of the data is inherited by the model. The data-fitting strategy is, however, still a general strategy for constructing theories and not simply fitting numbers to a meaningless equation. Fitting equations to a set of number data may be part of a data-fitter's methodology, but it is not the final aim in itself.

2.5 A debate on dark matter: a clash of epistemic styles

A recent debate on how to approach one of the most important problems of contemporary physics will serve to illustrate the difference between the first-principles approach to phenomenology/modelling and the data-fitting one.[7] One of the debaters was S. Sarkar, who presented himself as a 'theoretician', and whose work involves the production of cosmological models and the associated phenomenology starting from fundamental principles. His opponent A. Jaffe described himself as an 'astrophysicist' whose work focuses "on various topics including the formation of structure in the Universe and gravitational radiation" while also being "involved in the analysis of data from various experiments". Sarkar embodied the ideal pure theoretician doing first-principle modelling, and Jaffe the ideal data-fitting phenomenologist.

The debate centred on the plausibility of the dark energy hypothesis that was to be defended by Jaffe. Dark energy is one of the most intriguing topics in contemporary physics that is used to explain several anomalous astronomical observations that physicists interpret as evidence that the universe is expanding at a faster rate than was pre-

[7] Imperial College public debate *"The Big Questions: Does Dark Energy Exist?"*, recorded on 21/7/2009.

viously though. The expansion of the universe is described by using Einstein's General Relativity's field equations. Famously, at one point Einstein modified his equations and plugged in a term that is known as the Cosmological Constant, a parameter that can be given an arbitrary value without this having consequences over the general mathematical features of the theory, although it affects the type of solutions that are admissible.[8] Einstein initially introduced the constant to allow his field equations to have a solution where the Universe was static at cosmological scales, as it was thought to be at the time; if the constant is zero then only expanding universe solutions are allowed. Hubble's observations on galaxy redshifts later suggested that the Universe is not static, but seems to be expanding, and so Einstein removed the constant.[9]

The constant has been revived by so-called Standard Cosmology (also known as Λ-CMD, or 'Lambda-Cold Dark Matter'), a theoretical framework that unites theoretical proposals from cosmology, astrophysics and particle physics to 'explain' anomalous data. Jaffe works within this framework. Amongst these suppositions is that dark energy exists, and that this is reflected in Einstein's equations through a positive (non-zero) Cosmological Constant.[10]

The debate was centred on whether dark energy is a solid theoretical proposal, with Jaffe being in favour and Sarkar against the concept. Jaffe's arguments were mostly concerned with the match between the accepted data and Λ-CMD, while Sarkar stressed that he did not "consider the fact that a model fits the data at any given time to be critical." Sarkar continuously referred to Einstein's supposed dismissiveness of experimental data to support his view that observation, though an important guide, is considered secondary to working from fundamental principles in theoretical physics, as "Einstein famously did not believe in looking at data. He relied on his instinct and his intuition and of course as we know, served him very, very well". Sarkar pointed out that all of the accelerated redshift observations could be misled if it happened that our galaxy was in the middle of a relatively low-density portion of the Universe. "We don't know whether we might be in a void in which case all the observations that Andrew talked about can

[8] See Lawden (1962).

[9] Creating another one of physics great stories, Gamow (1970) wrote in his autobiography how Einstein confided that he deeply regretted this mistake, with Einstein remarking that introduction of the cosmological term was "the biggest blunder of his life".

[10] The energy's source is still quite controversial, but it is generally agreed that the most plausible candidate as of now is the energy of the quantum vacuum.

be mimicked".

The important difference between a first-principle approach such as Sarkar's and a data-fitting one like Jaffe's was summed up by Sarkar as follows:

> Sarkar: The reason why I think Andrew and I differ in our perspective [...] is simply that he is an astronomer and I am a theoretical physicist. The difference between us is that Andrew looks at the sky and he interprets the data according to a model and that has given him this idea that three quarters of the Universe is made of something called dark energy which apparently none of us has ever seen [...] whereas as a theoretical physicist [...] I will show you that Nature has solved that problem even if we have not. My *faith* is in fundamental physics. (emphasis added)[11]

Sarkar thus linked Jaffe's phenomenological work with the use of models and data as the starting point, while his own work is based on more 'fundamental' elements, that is, based on established theoretical suppositions that are completely data-independent. Commenting on the debate some weeks later, and again illustrating the tension between the Standard Model-as-a-model and Standard Model-as-a-theory dichotomy, Sarkar wrote:

> Sarkar: [We] must be precise about the distinction between experimental data concerning a theoretical prediction and, as in the present case, *fitting the observations to the parameters of an assumed model*. To illustrate, the "Standard Model" of particle physics (which should really be called the'Standard Theory") predicted that the W and Z bosons should exist with specific masses — I was at CERN in 1983 when they were discovered. That is indeed experimental evidence and confirmed that the electroweak unification theory is correct...and every test since then has continued to confirm the theory. It has even been tested at the quantum (1-loop) level and found to be correct. The only question today is at what energy does it break down, as it surely must since it does not unify gravity (but it is quite possible that it might hold up to the Planck scale). By

[11]Jaffe actually described himself as an astrophysicist, not an astronomer, jokingly adding that he had never actually done telescope work himself.

> contrast the'Standard Model" of cosmology does not predict anything—it is the simplest possible model of the universe based on assuming maximally symmetric space-time containing 'ideal' fluids with the dynamics determined by the theory of general relativity. (emphasis added)[12]

Sarkar embodies a theoretician working from 'first-principles', whose trust in experimental data and the knowledge that can be derived to it is secondary to 'fundamental' arguments, while Jaffe is the prototypical 'data-fitter' who is guided mainly by experimental data. An important point to notice is that both theoreticians are confronting their styles of approaching phenomenology while focusing on the *same* object of research: dark energy. Sarkar does comprehend Jaffe's data-driven approach perfectly, and vice versa, but chooses to place his *belief* on first-principles. Jaffe mentions the 'Standard Model of Cosmology', which is of a much more recent creation and although consolidated as standardised belief within the astrophysics and cosmology community still has a fair share of 'fundamental physics' lacunae. As an outsider to the cosmology community, Sarkar does not accept their standardised belief even though it has the same phenomenological origins as the SM of particle physics. Although it of course has epistemic elements, this is clearly a sociological judgement from one thought style upon another.

2.6 Theoretical styles in early quantum theory

In the following pages I will present some historical studies that further illustrate the differences between the two modelling styles. This will also serve to make the point that a thought style does not *determine* theoreticians' approaches to their practice, and in fact some of the most able theoreticians were known to be quite the 'epistemological opportunists'. Nevertheless, theoretical styles do tend to shape a physicists' overall work (as will be illustrated throughout the rest of this thesis), and even the most 'opportunistic' theoreticians tend to have periods where they practice one style over another, and are uncomfortable with results that are produced from outside their 'natural' style (see sections on Planck and Einstein below).

[12]Personal communication with the author.

In a historical analysis of the early history of quantum physics and of Arnold Sommerfeld's theorising and pedagogy, Seth (2010) has labeled Sommerfeld's approach to physics as a *problem driven style*, which emphasised "a new, more physical perspective [where] technical applications were blended with mathematical and physical methods" which Sommerfeld carried over from his engineering-centred background. As Seth (2010, p. 16) explains, his theoretical method 'shows a striking similarity to part of the Königsberg paradigm for theoretical physics: essentially the mathematical analysis of experiment". This Seth juxtaposes with the approach of Sommerfeld's collaborator, Felix Klein, whose theoretical approach was much more driven by pure mathematical considerations. In fact, Seth argues that while in his earlier stages Sommerfeld described his style as that 'of a mathematician", Sommerfeld gradually refashioned himself to a data-driven approach.

However, these historical styles need not map perfectly to the styles as outlined in the previous section. For example, Seth (2010, p. 33) describes how despite its empirical adequacy, Sommerfeld remained unconvinced that Planck's formula for the black body radiation problem was fundamentally correct, because Sommerfeld was immersed in what Seth calls 'the electromagnetic worldview', while Planck's method came from the old mechanical worldview' of a previous generation of theoreticians. Yet, a connection does seem to appear between them, as Seth (2010, p. 43) similarly differentiates between "Sommerfeld's *physics of problems* from Planck's *physics of principles*". This does not lead to a contradiction between these historical findings of my account, but rather suggests that — as would be expected — theoretical styles change across time; worldviews are historically contingent. Nowadays, the electromagnetic vs. mechanistic *worldview* distinction no longer exists as such, these two world-views having been both stabilised within canonical theoretical physics. As Seth explains, it is not that a particular style or worldview determines or gives a final shape to theoretical outcomes, nor that theoreticians are blinded to other approaches, but the more modest conclusion that framing a particular problem from within particular conceptual frameworks *moulds* the pathway — the actual theorising — which a physicist adopts.

These styles of theorising are not meant to be understood as 'recipes' to solve a problem, but rather as a loose network of attitudes and very general methodologies towards what constitutes the ideal ways to approach a problem. A first-principles theoretician would tend to frame a particular problem, the general puzzle related to the phenomenology of dark energy, from a Standard Model approach, while a data-fitter would first and foremost start from a desire to explain anomalous data as a starting point. What this does entail is that the general methods to carry out such work will change depending on this framework. As historical studies on theory have shown, this is sociologically relevant in that these 'styles' are not just a matter of pure personal choice, but in fact create 'schools' or networks of collaborators who share these methodological commitments. Thus Sommerfeld's style not only shaped his own theorising, but also that of his numerous students, just as he himself had been immersed in the electromagnetic worldview that marked his own generation. Likewise, Kaiser (2005a) has shown how adherents to particular schools in mid 20th century physics relied deeply on training of doctoral student, postdocs and colleagues to disseminate particular methods of resolution that were also tied to specific flavours of technique and — to use the terminology coined by Lakatos (1978) — competing *research programs*.

2.7 Max Planck, the awkward data-fitter

A further example of how theoretical styles do not *determine* the outcomes of theory can be glimpsed in Max Planck's method to arrive at his famous black body radiation formula. Kuhn (1987) has detailedly described the processes that lead from the empirical problem of 'black body radiation' in 19th century German experiments, to M. Planck writing up its definitive equation which marked the beginning of quantum physics. The problem of black body radiation entailed the necessity of devising a theoretical explanation for the measured radiation spectra of a 'black body' (essentially, the radiation emerging from a closed oven at a given temperature). Since it's mid-century formulation up to the turn of the century, various formulae had been proposed to make sense of the spectrum by some of the brightest German physicists, mainly the Wien distribution and the Stephan-Boltzmann equation). When these formulae were compared with the actual spectrum, some were seen to match the spectrum well at high frequencies but failed for the low ones, or vice versa; no formula was seen to match the entire spectrum. All major theoretical proposals were based on 'physical principles', that is, could be derived from very basic suppositions that did not have anything to do specifically with the black body problem.

Planck, who became interested in the problem in the 1890s, was particularly interested in the formal connection between electromagnetism and the second law of thermodynamics, and thought that the blackbody problem should be solvable solely from these considerations since these were for him two fundamental basic principles (Kuhn argues that it was Planck's orientation towards a particular interpretation of the Second Law as a statistical law which actually set the ground for a solution to the problem). The problem for Planck then had then two elements:

1. Determining an equation that would fit the spectral data.

2. The equation had to be derived from basic principles.

In 1900 Planck gave a brief seminar presentation in which he put forth a tentative solution to the problem. He started from these theoretical assumptions:

1. Planck (1900) stated that the Wien distribution "at most [had] the character of a limiting case, the simple form of which was due only to a restriction to short wave lengths and low temperatures" according to the best experimental data at that moment.

2. That the solution ought to be found by combining the use of the Second Law of thermodynamics and combining it with Wien's ideas of the walls being thought of as being made up of electromagnetic oscillators. The general strategy was that to solve the problem by first giving the expression for the entropy of the system (the entropy is the physical stuff with which the Second Law deals with directly) and from there to derive the spectrum. Planck (1901) explained that "the physical foundations of the electromagnetic radiation theory, including the hypothesis of 'natural radiation', withstand the most severe criticism; and since to my knowledge there are no errors in the calculations, the principle persists that the law of energy distribution in the normal spectrum is completely determined when one succeeds in calculating the entropy S of an irradiated, monochromatic, vibrating resonator as a function of its vibrational energy U".

3. That the solution had to follow the 'Wien displacement law,' which limits the possibilities of the mathematical form the equation can take (the equation for the energy at a particular temperature could only depend on frequency divided by temperature).

Planck (1900, p. 2) stated, "I have finally started to construct completely arbitrary expressions for the entropy which although they are more complicated than Wien's expression still seems to satisfy just as completely all requirements of the thermodynamic and electromagnetic theory". Because of all these considerations Planck knew that, whatever the expression he needed might be, it had to be reduced to a Wien type equation at high energies. He proposed a specific modification to the Wien law, justified because "it is by far the simplest of all expressions which leads to S as a logarithmic function of U— which is suggested by probability considerations— and which moreover reduces to Wien's expression for small values of U". From these considerations Planck then directly derived an equation for the energy, which is the now-famous Planck distribution law. The equation is very often wrongly said to have been derived supposing that the oscillators radiated 'quanta' of discrete energy; Planck never mentioned the concept of a 'quantum' in the early papers, nor did he ever seem to look favourably on the quantum hypothesis. As is well-known, Planck described his turning to a data-fitting approach as a desperate attempt to come up with a phenomenologically favourable formula.

Thus in his Nobel prize acceptance speech, Planck (1920) stressed how he still viewed the new quantum physics as fundamentally incomplete so that, "The difficulties which the introduction of the quantum of action into the well-tried classical theory has posed right from the start have already been mentioned by me. During the course of the years they have increased rather than diminished, and if, in the meantime, the impetuous forward-driving research has passed to the order of the day for some of these, temporarily, the gaps left behind, awaiting subsequent filling, react even harder upon the conscientious systematologist. What serves in Bohr's theory as a basis to build up the laws of action, is assembled out of specific hypotheses which, up to a generation ago, would undoubtedly have been flatly rejected altogether by every physicist. The fact that in the atom, certain quite definite quantum-selected orbits play a special role, might be taken still as acceptable, less easily however, that the electrons, circulating in these orbits with definite acceleration, radiate no energy at all. The fact that the quite sharply defined frequency of an emitted photon should be different from the frequency of the emitting electron must seem to a theoretical physicist, brought up in the classical school, at first sight to be a monstrous and, for the purpose of a mental picture, a practically intolerable demand". The data-fitting success of the quantum hypothesis did not, from Planck's perspective, provide a successful solution to the original problem as it was lacking the motivation of a solid first all-out principles approach.

2.8 Einstein the epistemological opportunist

Finally I wish to address a piece of theoretical mythology which can be found in many popular science texts, regarding the difference between high-theoretical methods and phenomenological ones. This is the myth of 'beauty' as the prime mover of theoretical practice. It is often claimed by high-theorists that the search for 'simplicity' or 'beauty' is the crucial feature and the culmination of modern physics. In such popular accounts, theoreticians often point out that the entire enterprise of twentieth century theoretical physics can be seen as a march towards a Grand Unified Theory of physics, that is, a single physical theory to understand all interactions. Most accounts begin this story by pointing to Maxwell's unification of electricity and magnetism as an important piece of this enterprise, and then discussing Einstein's unification of time and space, and finally the unification of the electromagnetic, strong, and weak interactions that ended with the Standard Model of particle physics. This development is said to be correlated with the search for 'beauty' inherent to physical unification. Gell-Mann (2007) for example has stated that

> Beauty is a very successful criterion for choosing the right theory. And why on Earth could that be so? Here's an example from my very own experience, fairly dramatic actually to have this happen. Three or four of us in 1957 put forth a partially complete theory of [the] weak force, and it was in disagreement with seven — seven, count them, seven — experiments. Experiments were all wrong. And we published before knowing that, because we figured it was so beautiful, it's gotta be right! The experiments had to be wrong, and they were. Our friend over there, Albert Einstein, used to play very little attention when people said, 'you know, there's a man with an experiment that seems to disagree with special relativity. D. C. Miller, what about that? And he would say, 'that will go away'.

In this story Einstein is often portrayed as the father of the unification ideal, and his search for the 'symmetry' and 'beauty' fills theoretical mythology. Zee (1986) for example writes, "my colleagues and I in fundamental physics are the descendants of Albert Einstein; we like to think that we too search for beauty. Some physics equations are so ugly that we cannot bear to look at them, let alone write them down", adding that, "when presented with two alternative equations purporting to describe Nature, we always choose the one that appeals to our aesthetic sense. [...] Such is the rallying cry of fundamental physics". Zee then juxtaposes the attitude of 'fundamental physics' with those of "phenomenological theories, constructed simply to 'explain' a given phenomenon. Theorists craft such theories to fit the data, and get out as much as they put in. They lead their phenomenological theories, rather than the other way around. Such theories may be of great practical importance, but typically they tell us little, if anything, about other phenomena, and I find them to be of no fundamental interest". According to Zee, the ideal of beauty for these physicists is captured in the idea of symmetry in nature. "When I think of the intellectual history of symmetry in physics, I like to picture two schools of thought, united in their devotion to symmetry but differing in their outlooks on the character of symmetry. On one side stand Einstein and their intellectual descendants. To them, symmetry is beauty incarnate, wedded to the geometry of spacetime. [...] on the other side stands Heisenberg with his isospin, shattering the aesthetic imperative of exact symmetry".

This does not mean that symmetry arguments are not crucial to modern physics, of course. Symmetry arguments have roots in geometry, but nowadays are stated in mathematics through the algebraic language of group theory. Noether's (first) theorem, one of the most important results of mathematical physics, dictate that if the equations that describe a physical system exhibits a certain kind of symmetry, then one can derive a 'conserved quantity' for the system. Time invariance in the Lagrangian can be associated to the conservation of energy via Noether's theorem: if the so called Lagrangian function (which completely describes the dynamics of a system) does not depend on time explicitly, then the system's energy is conserved. Conservation of linear momentum and angular momentum can be shown to follow from the homogeneity and isotropy of space, respectively, which correspond to a system's invariance under translations and rotations. The majority of modern particle physics involves the construction of equations that follow particular symmetries or combinations of symmetries.[13] Nev-

[13]The collected essays in Wigner et al. (1997) beautifully close the gap between popular treatment and technical argumentation concerning symmetry.

ertheless, romanticised versions of the history of physics that portray symmetry as the prime mover of theory do great injustice to phenomenological approaches.

Yet Einstein himself is a counterexample to the 'beauty is the prime mover of theoretical physics' myth. Norton (2000, p. 135) has argued that although "Einstein proclaimed that we could discover true laws of nature by seeking those with the simplest mathematical formulation [...] he came to this viewpoint later in his life. In his early years and work he was quite hostile to this idea", and further on that "his indifference to mathematical simplicity persisted up to and through the years in which he worked on completing the theory [of General Relativity]". Norton describes Einstein's attitude towards a theory of gravitation that Abraham published in 1912; although he initially received it enthusiastically, Einstein soon came to disregard it as fundamentally flawed. Norton cites a letter from Einstein to Besso in 1912 that sheds light on young Einstein's thoughts regarding what he saw as an erroneous application of 'simplicity' arguments:

> Abraham's theory has been created out of thin air, i.e. out of nothing but considerations of mathematical beauty, and is completely untenable. How this intelligent man could let himself be carried away with such superficiality is beyond me.[14]

Norton's point is not that considerations of beauty played no part in Einstein's theorising, which they clearly did in the development of General Relativity, but that his brilliance came from a deep understanding and ability to tackle problems using many heuristic devices without limiting himself to one exclusive type. These included mathematical, physical, aesthetic and phenomenological resources whenever any of them seemed appropriate. Norton (2000, p. 148) cites another letter to Besso in 1914 where he conveys how invariance arguments had at times proven counterproductive for Einstein:

> At the moment I do not especially feel like working, for I had to struggle horribly to discover what I described above. The general theory of invariants was only an impediment.

Einstein's final completion of General Relativity nevertheless did come about thanks to a final push to incorporate covariance into the theory, and this is the reason that Nel-

[14]Cited in Norton (2000, p. 143).

son believes led Einstein to overstress the importance of 'beauty' and 'simplicity' arguments in his later career over all others, in opposition to the methodological diversity of his earlier years.

Hon & Goldstein (2006, p. 657) have also questioned whether the relevance of symmetry arguments can be extended as far as popular and philosophical accounts claim, and in reference to Einstein the authors consider that the story that he was motivated by symmetry arguments in the creation of special relativity is inaccurate. Although Einstein used the word 'symmetry' in his 1905 paper on relativity six times, Hon and Goldstein's detailed historical analysis shows that Einstein was not thinking of symmetry in the modern sense, but was referring to a long standing discussion in the literature that had been going on since the 1880s when Rowland introduced the term in reference to the unification of electric and magnetic phenomena under one single physical scheme, that is, as manifestations of a unique phenomenon. Although 'simplicity' was a key heuristic, the authors have argued that there is no proof to show that there is any 'aesthetic' component to Einstein's argument, for "Einstein sought to unify the phenomena, that is, to come up with minimal presuppositions that underlie the variety of phenomena— notably those of electrodynamics and mechanics— and realised that indistinguishability is the key rather than interchangeability. This is neither an aesthetic consideration nor an epistemological one. It is the result of a methodology in which physical arguments play an essential role. Einstein responded in a most original way to a discussion in the literature that had been going on for some 25 years". No one exemplified methodological multiplicity better than Einstein, and to lose sight of this is one of the greatest untruths of modern physics' myth-based history, for as Einstein stated

> Science without epistemology is — insofar as it is thinkable at all — primitive and muddled. However, no sooner has the epistemologist, who is seeking a clear system, fought his way through to such a system, than he is inclined to interpret the thought-content of science in the sense of his system and to reject whatever does not fit into his system. The scientist, however, cannot afford to carry his striving for epistemological systematic that far. He accepts gratefully the epistemological conceptual analysis; but the external conditions, which are set for him by the facts of experience, do

> not permit him to let himself be too much restricted in the construction of his conceptual world by the adherence to an epistemological system. *He therefore must appear to the systematic epistemologist as a type of unscrupulous opportunist*. (emphasis added)[15]

Kennefick (2007) and the exhaustively meticulous historical studies of Janssen (2007) both support the view of Einstein as 'methodologically opportunistic'. Thus Janssen (2007, p. 819) notes that Einstein did not hesitate in varying his viewpoint on 'fundamental issues', for "when he was under the impression that the rotation metric was a vacuum solution of the field equations, he claimed that rotating and non-rotating frames of reference are equivalent in his theory. When he was under the impression that it was not, he claimed that the theory explained why rotating and non-rotating frames of reference are not equivalent. I am not saying that this was an unreasonable thing to do. On the contrary, it would have been foolish for Einstein to hold on stubbornly to the letter of his heuristic requirements if he felt that an otherwise attractive theory simply lacked the resources to meet this or that requirement. Creative scientists may need a healthy dose of opportunism".

Although Einstein was a strong believer in working from fundamental principles, the assertion that Einstein was an exclusively a first-principles theoreticians that chose to ignore experiment does not stand up to historical scrutiny. Galison (1987) has shown how Einstein was heavily involved in the elaboration of at least one important experiment, the measurement of the electron's gyromagnetic ratio, and how this stemmed from a deep regard for experimental technology which he had been developed ever since working as a high-level officer in a Zurich patent office. As is also well known, Einstein worked in phenomenological topics throughout his life but particularly in the early stages of his career, including the photoelectric effect, the specific heat of solids, Brownian motion in liquids, critical opalescence, and many others. It is unfortunate that the 'official' history of physics reminds us of only a very partial segment of his work, the theories of relativity, that he held towards the end of his life.

Ironically, it is often said that Einstein was motivated into producing the Special Theory of Relativity looking for an explanation to the Michelson-Morley experiment, a statement that historical studies have debunked. Holton (1969) has argued that that

[15] In Schlip (1949).

although Einstein may have been aware of the Michelson-Morley experiment before his work on Special Relativity was published, it certainly was not determinant at all as an experimental source if we are to believe Einstein's own testimony. Holton cites, amongst others, an interview with Shankland where Einstein clearly stated (to Shankland's surprise) that Michelson's work had not been crucial in any way to his first publication, although he knew of the result and came to greatly value Michelson's work in later years:

> The first visit [4 February 1950] to Princeton to meet Professor Einstein was made primarily to learn from him what he really felt about the Michelson-Morley experiment, and to what degree it had influenced him in his development of the Special Theory of Relativity.... He began by asking me to remind him of the purpose of my visit and smiled with genuine interest when I told him that I wished to discuss the Michelson-Morley experiment performed at Cleveland in 1887... When I asked him how he had learned of the Michelson-Morley experiment, he told me that he had become aware of it through the writings of H. A. Lorentz (Arch. Neerl. 2, 168 [1887], and many later references), but only after 1905 had it come to his attention! "Otherwise," he said, 'I would have mentioned it in my paper." He continued to say the experimental results which had influenced him most were the observations on stellar aberration and Fizeau's measurements on the speed of light in moving water. 'They were enough," he said. [cited in Holton (1969, p. 154)]

2.9 Theoreticians and computers

Although theoretical physicists increasingly rely on computers to carry out their work, it is important to distinguish between computers as *calculating machines*, and computers as *sites of simulation*. In the first case, a computer may be used simply as a glorified pocket calculator to obtain a numerical result which it is not practical to obtain by hand calculations. Consider for example as simple an integral as the following:

$$\int \sqrt{\sin x}$$

The solution to this integral is a function called an *elliptic integral of the second kind*:

$$\int \sqrt{\sin x}\, dx = -2E\left(\frac{1}{4}(\pi - 2x)|2\right)$$

A definite numerical value for the integral can be calculated using various methods, all of them cumbersome but not particularly difficult. Nevertheless, a program like the very popular *Mathematica* can provide an accurate numerical answer in a fraction of a second by simply entering the command:

$$EllipticE\left(\frac{1}{4}(\pi - 2x), 2\right),$$

(where x is substituted for a numerical value to turn the indefinite integral into a definite one) and pressing a button. This is no different to using a calculator to obtain the value of a trigonometric function like $\sin x$, which may also be calculated by hand, but seldom is.

In fact, programs like *Mathematica* can be used for much more complicated tasks such as solving complicated differential and integral equations which would prove incredibly time-consuming through purely human means. Moreover, *Mathematica* has one characteristic that also makes it an interesting theoretical tool, the ability to do symbolic mathematics, that is to 'do algebra'. But, though *Mathematica* can also function as a platform for higher-end numerical computing, it has one characteristic that many physicists find undesirable: that most of its underlying mathematical engine is 'black boxed', meaning that users cannot manipulate the inner core of the program's computing engine.

Before the age of easily available desktop computers, physicist relied on hardcore programming skills to obtain numerical results using lower-level programming, with *FORTRAN* being a particularly popular language in physics, even to this day when more advanced programs like *C* have rendered *FORTRAN* obsolete elsewhere. Unlike *Mathematica*, *FORTRAN*'s numerical algorithms are open for users to inspect and modify, and are thus in principle 'verifiable'. *FORTRAN*'s numerical libraries can be modified, built upon and tinkered with, and so fit very well with physics' hands-on-approach to problem solving, corresponding to what Turkle (2009, p. 32) has called "the aesthetic of transparency".

Most theoreticians at the phenomenological end of the spectrum require numerical results of some sort, and so some degree of programming skills are nowadays part of the skill set of all but the most mathematically oriented theoreticians:

> Now we include computer work as part of the curriculum too. I learnt how to use *FORTRAN* when I was still very young. I tried switching to *C*, but then I became frustrated when I realised that even though *C* was superior to *FORTRAN*, it turns out that when you look for a routine or a subroutine online, or in software libraries, they're written for *FORTRAN*. It's absurd! It turns out you have to translate them, or use a converter, and I said...to hell with it. When the Cray first appeared, everyone said, use *FORTRAN* — of course — so I switched back to *FORTRAN*. I still use *FORTRAN*. Around 1991 or 1992 I discovered *Mathematica* and I loved it. I even wrote a program that did some differential geometry because I was then working on a two dimensional differential geometry problem and wrote a program to handle repeated indices and to calculate some minor stuff, nothing really big. Nevertheless, with time I grew disappointed by *Mathematica* because when you used it to make hard calculations it got stuck. I was very surprised that as years went by...Computers are much, much better nowadays than back then. The computer here on my desktop is quite superior to that Cray we used twenty years ago; processors, etc. I rediscovered Mathematica a couple of years ago, and now I've become fascinated by *Mathematica* again. I've again become a 'Mathematicaoloist'. The things that I did with *FORTRAN* I do now with *Mathematica*, because it's got enough power; the number crunching power that it lacked. The algebraic manipulation power is still there, it's absolutely marvellous, in addition to the numerical power. It's another tool you use, *Mathematica* or *FORTRAN*.

2.10 Simulations and complexity

Unlike simple 'number crunching', computer simulations are not just a tool that makes standards theoretical work easier or faster, but are in themselves a third style of phenomenology that has experienced tremendous growth in recent times. It is necessary to somewhat delimit the concept, since as with the term 'model', 'simulation' does not have a solidly fixed meaning and is sometimes used to describe any sort of computer-mediated theorising; Sismondo (1999) writes that "Models and simulations do not [...] form a homogeneous category [but] form a continuum, from spare symbolic entities to somewhat more complex sets of equations that are computerized largely for ease of calculation and manipulation, to computer programs so large and intricate that no one person understands how they function." Nevertheless Sismondo finds that this continuum has two endpoints, so that "the endpoints of this continuum are large enough that complex computer simulations can be said to use models, of many different types, or to have some particular models at their heart. *Simple* models and *complex* simulations, then, are in at least this way different types of objects, while they are related as endpoints on a continuum" (emphasis added).

The simulation that I will deal with here are at the 'complex' end of the continuum described by Sismondo. Hence I will use simulation in the same sense as Knorr–Cetina (1999, Ch. 1), that is, simulations as representations of phenomena that attempt to reproduce the structural features of the phenomenon to be represented as closely as possible. Simulations in this sense need not be limited to computer programs. Knorr-Cettina presents the example of sandbox war games as a type of simulation. These games, often portrayed in historical films where a general is overviewing an upcoming battle with his field commanders by placing enemy and friendly 'troops' on a miniature version of the battlefield, are carried out by supposing that each piece has a predefined possible movement (depending for example on the type of troop, and the terrain it sits on) and predefined effect on close enemy troops. Chess can be seen as a particularly simplified example of a simulation of sorts in this sense. A war game will evidently be 'better' the more it resembles the way an actual complex battle is carried out. Of course, this would in turn require extremely complex 'rules' which can never fully capture the 'real' battlefield's and the troops' features, but the point of an ideal game would be to increase the complexity of the game as much as is manageable. An *ideal* simulation would then be a pocket-size version of the real thing.

In striving for an isomorphic relationship between a phenomenon in the real world and the simulation, a physicist would be working against the traditional way in which theoreticians pose their problems, that is, by making the system under investigation as *simple* as possible.[16] This search for simplicity is often of a pragmatic dimension tied to the search for *exact results*, which are the ideal outcome of pure theory; theoreticians are never happier than when their results can be reduced to an exact algebraic equation or a definite algebraic number. But in order to accomplish this, theoreticians tend to simplify physical systems so much that they often end up studying systems that look nothing like the original piece of the world that inspired their investigation. An old joke — of which there are many versions — that went around the science school where I carried out my undergraduate physics studies reflects this. I write it here more or less as I recall it from memory:

> *A millionaire racehorse breeder sets up a million-dollar scientific challenge. The challenge is to use any means whatsoever to produce horses that run as fast as possible. Three scientists — an engineer, a biologist, and a physicist — are invited to provide an answer to the challenge. After one month, the millionaire summons all three and asks for their answers.*
>
> *The engineer says, "I have come up with a new horseshoe design based on space-age materials that will increase any horse's speed by at least 15%". The millionaire is of course quite impressed and congratulates the engineer and his team.*
>
> *The biologists says, "I have drawn up an aggressive genetic manipulation and breeding program that will yield horses that are 30% faster within two generations". The millionaire can hardly believe his luck.*
>
> *The physicist approaches the millionaire and dumps a large bunch of dirty, hand-scrawled paper on his desk, and haughtily says, "I have devised a means to make a horse gallop unaided at the speed of sound." The millionaire is about to shout with joy, but then the physicist adds in a hushed voice, "the only caveats are that it must be a perfectly spherical and frictionless horse in a vacuum".* [17]

[16] See Winsberg (1999).

[17] There is a similar 'spherical cow' joke that appears to be very popular in Russia. See: `http://en.wikipedia.org/wiki/Spherical_cow`.

2.11 Simulations as strategies to tackle complex physical systems

A central difficulty in producing a model of anything 'in the real world' is that no mathematical model can ever mirror, exactly, the complexity of real physical phenomena. Here complexity is meant in terms of the amount of variables that would be needed to make a mathematical set of equations that could 'mirror' the properties of a real system. In general, the first-principle approach and the phenomenological approach are not well suited to describe complex systems, and much effort is carried out to simplify the system's description as much as possible.[18] In particular, the first-principle approach is severely limited to often very simple and idealised systems, as one theoretician explained

> Volke: Theoreticians nowadays are increasingly turning to numerical experimentation. It's not that there aren't analytical things to do. There will always be analytical things, but it's only so far that you can go with analytical things. [19]

Consider for example a simple gas, helium, inside a cylindrical container, for which one wants to create 'a model' of the gas to describe its behaviour. Even a small container would contain millions of millions of molecules. From the first-principles perspective, a model for the gas at this level would include several equations for each molecule, given that the interaction between them is known, and thus the final solution would require to solve a set of equations with an absurd amount of variables, which is of course not feasible 'by hand' if one is to treat the gas molecule-by-molecule. An alternative first-principle approach would be to 'simplify' the problem in some manner, for example, to work from thermodynamic, macroscopic frame and come up with a three-variable, perfect gas model which would work reasonably well in describing how changes in the gas macroscopic quantities of temperature, volume and pressure are correlated. The model being much, much simpler than the actual system, the results could be reasonably good

[18]See Schweber (2000) for an analysis of the relationship between 'complexity' in physical systems and the increasing use of computer simulations over 'pencil-and-paper' methods in contemporary physics research.

[19]See also Winsberg (1999, §1).

but not perfect in the sense of it being a picture-perfect depiction of what an actual gas looks like. Another first-principle approach would be to work from a statistical mechanics perspective, and think of the molecules as simple bouncing balls with a certain statistical kinematic distribution, from which statistical information can be drawn to describe the thermodynamic variables. Needless to say, molecules are not rigid balls, and the result would be expected to differ from the real system in some ways. A phenomenological approach could start, for example, by measuring the actual response of the gas to changes in pressure and temperature, and then one could establish a polynomial equation of arbitrarily high degree that would describe the changes with a few thermodynamic variables. This would provide a much better 'description', but with a loss of explanatory power compared to the 'first-principles' approach

The simulation approach would turn the problem around, tackling the complexity of the problem head on. Simulationists have come up with a family of programs known as *Molecular Dynamics* in which a space 'grid' (a discretised Cartesian space) is created, and underlying it is an equally discretised time-dimension. Molecules are 'placced' within this grid, and given precise velocities. An equation relating the interaction between individual molecules is chosen (it can be a data-fitting or a first-principle sourced one) to be used as a theoretical base. The simulation tracks the movement of all molecules and all specified interactions between molecules (usually within a finite range) at each step, calculating what each molecules position and velocity will be at the next step. The choice of the equation for the interaction is of course crucial, as are the initial parameters, the range of the interactions, the algorithms to calculate the interactions, amongst many other possible variations (one can include probabilistic quantum effects, for example).

These type of simulations have what one may call a 'building-block' approach, where the object to be simulated is literally built upon a virtual 'space' piece by piece. These simulations as the extreme end of complexity have distinct characteristics:

1. Simulations, before they are coded into a computer, must borrow equations and relations from theory, and thus have the support of 'established' theoretical knowledge.

2. Simulations try to 'mirror' the system that is to be described as closely as possible, within the computational and time resources that the simulationist can afford.

Simulations offer as close a 'picture' of a phenomenon as is materially possible to achieve, compared to the other two phenomenological approaches.

3. Simulation work can often be *directly* compared to experiment.

4. The objects that are entered into a simulation are made to closely resemble the objects in the empirical world as much as possible, unlike many 'ideal' first-principle systems which are abstract enough that the similarities between them and the 'real' system may be only superficial.

'Building block' approaches have an added strongpoint that should not be overlooked: the possibility of translating the simulation into a *direct* visual representation of the object being simulated. Simulations can 'seduce' merely by the impact they produce through visual means; Turkle (2009, p. 28) describes how "physics students described feeling "closer to science" and "closer to theory" when their laboratory classes began to use software for visualization and analysis" and how "faculty acknowledged that there they would increasingly be competing with the seduction of screens", amongst many other case studies where visualisation plays a crucial role in simulations' gain of epistemic acceptance, and epistemic power. Turkle (2009, p. 17) thus finds that "screen versions of reality will always leave something out, yet screen versions of reality may come to seem like reality itself. We accept them because they are compelling and present themselves as expressions of our most up-to-date tools. We accept them because we have them. They become practical because they are available. So, even when we have reason to doubt that screen realities are true, we are tempted to use them all the same".

2.12 Simulations as autonomous domains of practice

Turkle (2009) has also amply investigated processes which lead to the paration of the world of simulation from their wider professional setting across various professions. Turkle found that across professions as varied as architecture, design, biology and physics simulations become highly autonomous practices to the point that not only the simulation itself becomes an alternate reality, but simulationists become technically independent from their wider professional settings. Simulations then become black-boxes to all but the most dedicated experts.[20]

Simulations have become such areas of specialisation, now having become scientific expertises that can boast numerous specialised journals, professional and educational associations, and many other markers of a mature autonomous science.[21] Programming and coding have become sets of skills used by the simulationist that are not part of the 'traditional' theoretician's tool bag. Debugging for example is thought to make up at least 50% of a dedicated programmer's work.[22] Debugging is relatively trivial in programs such as *Mathematica*, but it becomes a daunting task in the low-level programming work using programs like *FORTRAN* where the computer code may become tremendously large. Computational techniques such as Molecular Dynamics, can include code with over 150,000 lines.[23] The use of these massive codes of course requires much more serious programming work, including both debugging and structural planning that are not encountered in 'traditional' theory's simple computational tasks.

Debugging, the 'art' of finding errors in computer code, is a particular example of a skill that simulationists posses which is outside the scope of the traditional theoretician. This hands-on skill requires careful consideration of how code is built up, even in cases where the code used is very familiar to the programer. As a simulationist explained,

> Loke: At each step [of writing the code] it's wise to test each step. [...] We develop toolboxes: tried and true components that have been used over and over again, so we're at least confident that some parts are complete [...] We try to build these separate components then use them together and eliminate as much [error] as possible each step. We try to check each step. When we didn't, when we've approached it like, 'Ok, just chuck everything together and we'll test it at the end of it', you'll find that you spend

[20]See also Dowling (1999, p. 265).

[21]A non-exhaustive list of journals includes *Communications in nonlinear science & numerical simulation, Computational geosciences, International journal of modelling & simulation, International journal of simulation: systems, science & technology, Journal of applied mathematics and simulation, Macromolecular theory and simulations, Engineering, Modelling and simulation in materials science and engineering, Simulation, Simulation digest, Simulation modelling practice and theory, Transactions of the Society for Computer Simulation*.

[22]See Lee & Wu (1999).

[23]The CPMD consortium (2011) gives this figure for the CPMD (Car-Parrinello Molecular Dynamics) code, which is "a plane wave/pseudopotential implementation of Density Functional Theory, particularly designed for *ab initio* molecular dynamics".

> a lot more time debugging if you did that than if you write a little, test a little, write a little, test a little. It saves you more time in the end. You have to check, 'At this stage, the field at this second or third step it should be, according to the theory...does that look right or is your sign is totally out? Oh yeah, you're out of the ballpark!' So you've got to know, as much as possible, to know what you're expecting, within reason. Some things are unexpected, and some new scenarios can be surprising but at least its within a certain range.

Because debugging actually makes up such a huge proportion of programming practice, this ability is recognised as a mark of expertise in computer programming. Vessey (1985) concludes that debugging techniques are significantly different between novice and expert programmers, concluding that while novices tend to scan code line by line, experts are more likely to look at a code in 'chunks' and thus get a better feel for the general structure of the code, which also tends to make their work more efficient. This differentiation is one clear indication of how simulationist practice has departed from traditional theory. Simulationists can even develop a sort of 'debugging intuition' as a senior computational physicist explained:

> Noguez: The 'gut instinct' you develop [to avoid bugs or bad program structures] that I'm talking about is simply experience translated into knowledge, and it's just like intuition. There may be illuminated geniuses that are born with these skills, but I've yet to meet one amongst my colleagues, although they might think differently about themselves. I spend most of my life in front of the computer. Literally.[24]

Dowling (1999, p. 269) has similarly argued that "A sense of direct manipulation encourages simulators to develop a "feel" for their mathematical models with their hands and their eyes, by tinkering with them, noticing how they behave, and developing a practical intuition for how they work", and that "A large element of skill and tacit knowledge is involved in developing this "intuitive feel" for a computer simulation".

[24]Intuition will be introduced in chapter 6 as a form of tacit knowledge - *somatic* tacit knowledge; somatic tacit knowledge is a marker of an autonomous skill domain which can only be gained through *hands-on immersion* into the actual practices of the domain, as happens with debugging.

The development of a set of skills independent from that of traditional theoretical work is a marker of the fragmentation of computational physics from pure-theory, and even from modelling. Though simulationists necessarily need to use knowledge from the upper levels of the horseshoe diagram, this knowledge is not per se part of their core skills *as simulationists*.

2.13 Sociological evidence for simulations' epistemic autonomy: regress phenomena

Further evidence for the autonomy of simulation practice from that of either theory or experiment can be garnered from the existence of 'regress phenomena' which parallel Collins *experimenters' regress*.[25] Briefly put, regress phenomena arise in science when two autonomous knowledge domains face off discrepant result concerning the same piece of the phenomenal world, i.e. scientific controversies. Because of their epistemic autonomy, the domains — which are typically in positive epistemic feedback when there are no controversies — have no way to set up common standards of settling the dispute, and the traditionally positive epistemic loop instead turns into a negative vicious circle which according to Collins can only be broken either by social 'negotiations' or through the arbitration of a third, independent domain (although in the latter case the creation of further regresses is always possible).

The 'classic' experimenter's regress is encountered when an experimental community acknowledges results which are in disagreement with the accepted results of a theoretical community. A match between theory and experiment is a common 'objective' standard of epistemic strength for both theory *and* experiment (Collins thus rejects either an traditional empiricist or idealist philosophies of science), but in such a controversy there is no absolute standard to judge either theory or experiment wrong. The appearance of a regress is tied to the fact that in real situations, one can never be assured that either theory or experiment have been performed in an absolutely correct manner; partly because of the complexity of real-life scientific enterprises, and crucially also because of the role that tacit knowledge plays in experimental practice that disallows absolute 'internal' standards of performance within a community to exist (thus

[25] See Collins (1981, 1985).

invalidating in practice regress-breaking arguments that seek to use standards of replication as a standard of successful performance).

Sociological studies about simulationists' practice have discovered regress phenomena when simulations are faced off against other theoretical domains. In the case of simulation versus high-theory, Kennefick (2000) has described a controversy where a debate was carried out between simulationists and first-principle modellers tackling one same phenomenon in gravitational wave astronomy. A group of simulationists presented results to a mostly first-principle community that clashed with the expected theoretical results. Kennefick (2000, p. 18) discusses how the group that presented the controversial simulations results had a very hard time being listened to by the General Relativity community where these were most relevant, mainly because "they had a quite different institutional background from their opponents."

Kennefick (2000, p. 21) also presents evidence to attest that one of the main reasons why the high-theoreticians did not accept the results was the simulationists' "failure to speak the correct language", and also because high-theoreticians were "failing to make their arguments in a language which [the simulationists] would appreciate", what Pinch (1986) has referred to as the 'evidential context' of the claims. Turning back to Fleck's terminology, the episode shows how the simulationists were unable to grasp the 'thought style' which would have allowed that result to be presented in a manner that would have made it understood — if not agreed upon — by the GR community. Kennefick (2000, p. 27-28) understands the impossibility of understanding between these groups based on the differences between their theoretical 'styles', noting that "assessments of theoretical work take into account the style of the theorists, the manner in which they do theory, and this is an important factor in any theoretical controversy. [...] A theorist's style affects his or her choice of topics on which to work, how he or she looks at a problem once selected, and the choice of methods when tackling it", thus giving clear evidence to the fragmentation of simulation work from that of first-principle theory.

In a second, more general analysis of simulations confronted with external standards, Gelfert (2011) has reconsidered Kennefick's case study among others and analysed the role of both the software and hardware dimensions of the problem, by introducing the concept of the *(computer) simulationist's regress*. Gelfert's analysis encompasses the following steps:

1. The open-endedness of modelling and simulation do not in general permit the simulationist to unambiguously affirm that the simulation has been correctly implemented, once the simulation situation is of sufficient complexity.

2. Because simulations are often used to probe situations which one cannot have easy, immediate or practical empirical access to, one cannot use external 'data' to verify that the simulation is working properly.

3. Therefore there is no unambiguous way to say that the situation is working 'correctly' using 'external' parameters, and complexity prevents 'internal' parameters from being leading to absolute surety.[26]

The end result is that the simulation process can become so complicated that the programmer is left clueless as to where the specific mistake might be at all. The point to emphasise is that in a intricate program, the situation may be complex enough that no amount of minute examination of the code itself may give the programmer full assurance that the program is giving out 'the correct' result, even if such a thing were possible. Many if not all commercial programs are typically released with many known but unfixed bugs.[27]

Gelfert has put forth criteria which he considers 'break' the regress, i.e. criteria which allow the simulationist to affirm that a simulation has indeed been correctly implemented at all levels (callibration of simulations against exact analytic results from theory, calibration against other simulations, arguments from 'robustness').[28] These criteria are not incompatible with Collins' original thesis, although Gelfernt has positioned them as philosophical criteria, when in practice they are in fact instantiations of social 'negotiations' in Collins' sense. Therefore the social epistemics of simulation have a structure that exactly parallels traditional accounts of experimentation. In fact, this parallelism is not only well-noticed by simulationists, but is in fact seen as one of the strengths of simulation work:

> Volke: Numerical experimentation is a bit similar to...well...real experimentation. In programming the same things can happen, that things come

[26]See also MacKenzie (2001, ch. 7)) for a detailed case study of 'bugs' in a microprocessor's architecture and its relation to errors in arithmetical calculations and mathematical proofs.

[27]See for example `http://en.wikipedia.org/wiki/Criticism_of_Windows_Vista`.

[28]See also Gelfert (2005).

> up which you hadn't even considered. Numbers will shoot off and then your result won't be trustworthy. Things don't converge, sometimes, and you'll have to go to plan B. [...] In numerical computation you do find things there that you never expected to find; you have to rule out that it's not an error in your program. I do think experiment and computation have many similarities. I think the people that really know how to do numerical analysis, and numerical experimentation — because it *really is* experimentation — have a really powerful tool in it. It's a growing practice. We increasingly want to see what is happing with a graph, with a scheme, with a diagram. And it makes us understand things better. In my experience — I do numerical things myself— numerical experimentation is very similar to laboratory experimentation.
>
> Reyes: Sometimes you're running a simulation and then you get some singularity that you don't know the origin of. You don't know if the program is wrong or the results are wrong.
>
> Volke: Exactly, and the same thing happens with experiments. The same thing happens. Suddenly you start getting strange results.

Yet like Gelfert notes, regress phenomena are indeed resolved — when the result is non-controversial — through mechanisms such as calibration and robustness, and in these cases a 'double' positive feedback loop arises between *experiment-simulation-theory* which permits the full connection of the epistemic chains to proceed:

> Reyes: So when you want to find 'the' final solution to a problem do you use different methods and compare them?
>
> Loke: Yes. One example is that there are lot of non-analytical solutions for scattering off spheres. You can use a homogeneous sphere in free space, that's Mie's solution. Two of the methods that I developed were... their finite difference frequency domain in vector spherical wave function boundary conditions, hybrid system, for modelling an arbitrary shaped object with continuous rotational symmetry. You can build up like a sphere, and then see if your scattering results are the same as those of the Mie solution

> or use this Discrete Dipole Approximation with discrete rotation symmetry. I suppose a sphere doesn't have discrete rotation symmetry but all...you can say it has infinite discrete rotation symmetry. They can split out into four, and test that. Those tests pass, but they pass for a sphere. There was one instance where, 'Yes, it passed for a sphere but it doesn't guarantee that it works', so we tested it against a known solution scattering off a cube. But then we said, 'Hang on a minute, yes the fields are the right amplitude but the phases are different', because the sphere has this unique geometry and symmetry in rotation, and mirror symmetry in any plane, basically. So some of these sign errors can be revealed only if you use anything other than a sphere so we use cube and other more complex objects with known analytical results. That's how we usually test them, and of course ultimately — more importantly- experimental results. I used to work with a number of experimentalists in our group. They had their results and we did our best to fit the model to it and we had some predictive results as well which they used to design these new devices and conducting experiments and either confirm or refute out theory — our model.

Simulationists then have the advantage that they are placed at the crossroads of theory and experiment, and yet are neither and draw on specific tools, practices, knowledge, traditions and epistemic styles to produce knowledge. As in the case of the models/theories they use, they constitute families of practice that have their own professional identities and forms of closure. By having illustrated how the lower lozenges of the horseshoe diagram exhibit this fragmentation I have laid down the terrain for next chapter, where I will explore how despite this fragmentation of practices and the parallel fragmentation of languages and thought styles physics still remains a solidly interlinked discipline.

CHAPTER 3

Bridging the gap between contiguous micro-cultures

Common reason should perswade, that much reading and long practise in every Art makes men expert. Per Contrarium I conclude, you that haue neither read nor practised, must needs be egregiously ignorant.

—Henrie Chettle, from 'Kind-hartes dreame' (1592)

3.1 The fragmentation of high theory and pure experiment

In previous sections I have argued that differences in theoretical styles lead to fragmentation between communities that are side-by-side in the horseshoe, even communities that are working on a same direct research topic. In communities that are further away from each other, such as high-theory and pure experiment, the differences are even more radical because physics is different to many other natural sciences, like molecular biology or astronomy, in that there is a radical divisions between the high-theoretical and the pure empirical extremes.[1] Thus, one often finds that when a theoretician says

[1]As an ex-molecular biologist now-theoretician confided

> I was a sort of semi…I both did experimental work as I quite rightly thought you had to that, to go into the field, but I was meant to be a sort of semi-theoretical biologist. I consider it to be a bogus profession. Almost nothing came out of it. In biology…when I say it's a bogus subject…there isn't theory. I mean, there's no theory that makes any…it begins

that his theory is 'empirically adequate' or 'fits experiment', the experiments are not performed by the theoretician but by a professionally independent group, in contrast to many other sciences where those that produce the theory and those that produce the experiment are usually in close collaboration. This is made evident by the way many theoreticians admit they are perfectly able to work without ever consulting ongoing experimental research. Even when their theory is 'empirically-minded', many times experiment enters only very indirectly in a high-theoretician's work. When questioned about the role of experiment in his own theoretical work, Berry explained:

> Reyes: You've made quite a few theoretical predictions of phenomena. Do you have any direct contact with a laboratory where you can say, 'do this, don't do this'?
>
> Berry: No, no, no... with this conical refraction, astonishingly, and actually *for the first time*, I had some contact with an [experimental group] in Dundee. I've just recently encountered them. [...] And I'm predicting all kinds of things, like if you put crystals in series what would you get, and so on. So there's something where I'm directly involved, but it's unusual for me. I don't normally do that. There are different types of theorists, and some people work very close with the experimentalists. It's fun to do that, but I tend not to.

Berry is not a theoretician who works in an esoteric field of physics to which there is no easy empirical access. He in fact specializes in optics, one of the most empirically focused and well-probed fields of physics (he is proud to mention how he even once managed to publish peer-reviewed experimental papers using a table-top physics kit). Berry, as any other optics expert, has access to and is familiar with a plethora of empirical

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> to make sense when, and in fact I sort of left molecular biology just when it was becoming once again to become a theoretician of a different kind the kind that does computer science and tries to organise all the data.

Molecular biology 'theory' is a field that comes and goes as experiment requires it, in contrast to physics where theory can exist independently from the needs of the experimental side. Moreover, molecular biologists seem to be comfortable with the supposition that even theoreticians ought to have ample lab experience, an attitude rarely seen in theoretical physics circles. Immunologist Medawar (1969, p. 57) wrote that "most scientists cannot be classified as either experimentalists or theorists, because most of us are both...", a statement that while plausible in the biological sciences would be false in physics, as the number of people that work in both theory and experiment simultaneously is negligible.

knowledge created around the world during the last two centuries or so, and yet has never performed the majority of the experiments that concern his own theory. Berry is not an exception to the rule. Another high-theoretician evidenced his own work as being divorced from direct experimental contact:

> Reyes: Many of the theoreticians that I've talked to have told me that when you're working in pure theory and then— for whatever reason— you actually want to go and understand an experiment, it's extremely hard.
>
> Tong: Yeah! *I've never done it. Never, no.* Understanding an experiment means understanding the quirks of the beam at LHC and you know exactly what all the layers of the detector are seeing, blah, blah, blah, but really knowing what the flaws in the detector are, no, no. There are people that do. There are theorists that do. But you know, the whole thing is this continuum, where there are hardcore theoreticians at one end who only work on string theory, all the way through to the guy with the spanner, tightening up the nuts and bolts. There's thankfully big overlaps between each section but yeah, I've never chatted to the guy with the spanner. (emphasis added)

3.2 Difficulties in migrating from pure-theory to phenomenology

Another theorist who started out as a high-theoretician but has now 'migrated' to more phenomenological approaches confided:

> Romero: The opinion of many theoretical physicists is… actually, it's a kind of satire, something between dark humour and caricature and also with a hint of truthfulness… is that the further away you are from experiment the better things are; the idea of rigour. I myself grew up a bit with that idea, and with the years I changed, I turned around completely. In my opinion that is not physics; it's mathematics. It's something that's on the edge of mathematics. I think that doing physics is to have in mind a real problem from 'out there' as I like to refer to it.

Romero nowadays works closely with experimenters attempting to model the phenomena that they encounter in the lab. One would suppose that if a phenomenally oriented theoretician had the capacity to deal with experiment directly it would be one like Romero. And yet, when asked if this contact with his collaborators' experimental culture had increased his ability to access experimental data more directly, his answer was negative:

> Romero: There's a part of [experimental procedures] that's very obscure. It's the actual assembly of the experiment, and everything that you need around it. [...] They tell me things like "here's where we switch on the magnetic field, and then we're going to send a radiofrequency pulse and then we'll send a pulse of light and take a photo". I understand what a radiofrequency pulse is, I know what it is. I understand the numbers. I understand the final results, and a few details. But if you ask me where the pulse originated from I have not the slightest idea! Lasers, yes. Coils, well, I can see them. But the assembly part is very obscure.

Even a phenomenal theoretician has a hard time accessing the instrumental side of experiment. Indeed, phenomenal theoreticians don't gain 'broad' phenomenal knowledge, but usually only manage to understand the phenomenology of experiments they have direct contact with:

> Reyes: You work with one experimental group, but do you ever have to read work from other experimental groups? Do you ever have to contrast with things that come from experimenters with whom you don't deal directly?
>
> Romero: It's practically impossible. It's practically impossible unless you're friends with them and they give you the data directly. In these our times, it's impossible to verify directly anything of what's published. You see a graph and they tell you, "I'm graphing this against this". It's something worth commenting upon, as I was also involved in this issue. I was involved in this stuff about light that travels faster than light. We never had our own experiments, and it involved reading other people's experimental graphs. It turned out that in all experiments, there ought to be a requirement of an additional graph that contains the non-analyzed raw data

> [laughter]. What is it that you measured? You looked at an oscilloscope? You measured voltage. Then from that you gave me a time measurement? How did you manage that? How did you do that? "Well, because of this formula that..." Well, things turn sour there because at that stage they've already introduced a theoretical filter. That's typical. They obtain graphics where you really wonder how they've managed to obtain the results. And, now that I deal more with experimenters, you ask them about it and it turns out they actually didn't measure that. [...]Unless you have contact with an experimental group that shows you the raw data, and tell you what they measured [...]. Then they do data massaging. Data massaging to do this and this with this well-known formula, and the end result is quite lost. Sometimes you believe them, when the results are very well established. But when there is controversy, or it's a new effect...

Other phenomenologically oriented theoreticians similarly confided that a full understanding of the experimental culture was not within their grasp. Even Volke whose work comprises the last link of theory immediately above fully experimental work, the interpretation of experimental data, testified to only partial 'fluency' in experimental material culture:

> Volke: An experimenter in acoustics who does acoustical levitation approached me, and he thought, "optical trapping...acoustic levitation...we could work together". And it was really great. We did a wonderful collaboration. But there my proposals were like mapping my experience in optics. *As far as working on the experiments, well, only like a robot!* He would tell me to push some button and I would push it. He'd say 'raise that and lower that' and I would raise it and lower it. But he was the one that did all the experimentation part and designed the whole experiment. Once we started seeing results, then I could tell him if something was happening because maybe we had to use a bigger disc. In the interpretation and in proposing things to measure, yes, I could do it. *There I was playing the part of theoretician, essentially.* (emphasis added)

3.3 Differences in epistemic distance

Theoretician like Romero who start off as high-theoreticians can choose to bring themselves closer to experiment, but it is not a easy task, and it can take years of dedication to migrate from being a pure-theoretician to an empirically-oriented one. Romero described how in his early years he started out as a theoretician 'distant' from experiment, and then made an effort to transform himself from that kind of 'abstract' theoretician to one with more experimental links:

> Romero: In your work you make an effort, an enormous effort, to see if what you are saying has anything to do with what is out there, with an experiment. [...] Anyway, I think there are two kinds of us. I do feel more of the pragmatic kind, although if you look at the everyday stuff I do it's just as abstract as the people who do mathematical physics. [laughs]

Romero's clearly stressed that to include experiment in a theoretician's work requires massive amount of work if one starts from the pure theoretical side:

> Romero: Once [...] we needed to design a light detector. I had no idea how to do it! If you give me a month I'm sure I could manage, but then along comes a guy with a little machine, a circuit that only needs batteries, and everything is done. That experimental part, when they talk about the laser being unstable...I have no idea what the hell they're talking about. Instability? Then they explain and...well, there's a lot that's obscure, very obscure. However, in general, the more you speak to them the better you're at it. You start saying, "hey yes, this is where the laser comes out". Of course, it has to be tuned into the transition I want to make, and here's the cell and here's the detector, which is a *black box*. (emphasis added)

3.4 Languages in theory... in theory

Anyone who has ever tried to learn a foreign language understands the cultural shock associated with having studied a language— possibly for years— and then finding oneself at last in the country where that language is the mother tongue. Even when one may know the correct grammar and syntax of the language, sometimes even better than the natives, one usually fails to understand a lot of what is being said. However, nothing can compare to full immersion in an alien culture for raising a foreign speaker's linguistic skills. The equivalent of full linguistic immersion was the pathway followed by Romero and Volke, who were very explicit about the necessity of setting up personal, face-to-face communication in order to break down the linguistic barriers:

> Romero: Electronics in particular, I've only very rudimentary knowledge of it. So if you tell me that you get a signal and then you convert it from analogue to digital...it really irritates me. *It irritates me because I can say nothing about it.* [...] The more I talk to them the deeper my knowledge about the experiment and the experimental results. Nowadays I can actually see naked experimental results without any analysis and know what it is they did. But that's the whole of it. The part that I participate in the most is when I tell them, "Look, why don't you try this new thing" and they'll just reply, "No, that's way too hard". "Why?" "Because you can't do that to the coils". "Well, ok". *Then you keep on talking.*
>
> Romero, like other theoreticians who interact with experimenters directly, was emphatic on the necessity of constant, prolonged, face-to-face interaction in order to achieve mutual understanding between experimenters and theoreticians:
>
> Romero: With the Brazilian experimentalists it's no longer a thing of them giving me data and me adjusting curves. *I do talk to them.* But it's still a lot of me telling them to measure something and them measuring it and then we talk, and look over the results. *Every time we talk more about their technical setbacks*, why this laser or that laser is used. And sometimes it goes the other way round too. Due to my liking of laser-cooling and related things— not in the case of the principal researcher, but rather with the postdocs and the student— it turns out that sometimes I've understood things better than them. They knew that they had to treat the lasers a certain way and such, and there were some curves there with which they fit things. And I deduced these curves. Well, a Nobel prize winner deduced them, but I found the papers and grew to understand them very well, and I

> noticed one day while talking to them that they did not, that I had caught them in a misunderstanding. (emphasis added)

Another theoretician who migrated from high-theory to experimental analysis also explained how the process was carried out by 'jumping in with both feet' into the new context:

> Reyes. When you started doing this numerical analysis work having been trained as a pure theoretician, how did you go about that transition? I suppose it required different skills.
>
> Fairhurst : Yes, it does. What happened is I finished a postdoc and applied for another postdoc in a group which had theorists and also these guys doing gravitational wave searches. I was basically applying to work with the theory guys and I got offered a job from the analysis people because the gravitational search field was booming. [...] I just jumped in with both feet. They said, 'Start in some simple project,' like you might do as a beginning grad student, and away you go. [...] I moved to this group and it's what they did everyday. I was meeting with a professor who worked on this. I worked very closely with a final year PhD student and we got along very well. [...] I'd got to him everyday, and I'd go, 'what's going on? What does this mean?' He was writing his thesis so I read large parts of it, read a lot of papers. It was a lot of writing code so I just sat down and tried and learned how to do it. [...] There was no grand plan. It was a mess, trial-and-error. [...] And being able to walk down the hall to people...it took me a week to solve a problem that guys down the hall could've done in twenty minutes, but it was worth it because the second one I could solve in a day, and so on. But they were down the hall and so if I got really frustrated I could go to them. I think that's very important.

These two examples show that migration between the lozenges of the horseshoe diagram can be carried out in the same way as migration from an ordinary culture to another culture can be accomplished in real life: by full linguistic immersion into a new culture. One would expect that 'related' or 'close' cultures require less immersion than very 'distant' ones, both in the wider world and in theoretical physics; as a Latin

Americans one finds it relatively easy to speak to and relate to other Latin Americans, but much less so with many European cultures. Theoretical physics works similarly if modelled according to the horseshoe diagram: cultures close together would find communication fundamentally easier than with faraway ones. This is partly because closer cultures have similar characteristics, but also because cultures that are close together have more opportunities to interact directly, and therefore be partly immersed in each other's social worlds.

We can see that in these examples, there are two families of theoreticians: those who choose to practice their theory 'far' from experiment (high-theoreticians), and those who choose to get close to and understand experiment 'phenomenologists'. The rest of this chapter will be devoted to setting up a theoretical approach to expertise and expert languages to describe how the interaction and transfer of knowledge is carried out in the latter case — generally, where the interaction between dissimilar cultures or forms-of-life is 'forced' to interact.

3.5 Collins & Evans' framework of tacit knowledge

If one takes the disunity thesis seriously by considering that that science is fragmented in a non-trivial way into autonomous micro-cultures or heterogeneous linguistic domains as has been argued happens in physics, one is faced with the task of explaining how collaborations, communication and transmission of knowledge are carried out between these different micro-cultures. I will introduce Collins & Evans SEE (Studies in Expertise and Experience) framework to give an answer to the problem of communication, and I will argue that 'close' micro-cultures retain their autonomy as fields of practice, yet communicate by maintaining ties that rely on 'interactional expertise', the ability to speak the language of a social domain proficiently without actually becoming an 'active' member of that domain.

In Collins & Evans' framework, interactional expertise is established through linguistic immersion in another culture and it is based mainly on the acquisition of large amounts of tacit knowledge from the 'host' culture. I have shown how theoreticians like Romero and Volke who force themselves into interaction with an initially 'alien' experimental culture do just this: full linguistic immersion with the 'parent' culture. Collins & Evans posit that full immersion is necessary not only to gain bits of explicit language, but more crucially, to understand the *tacit dimension* of the culture the foreigner wants to interact with. Tacit knowledge has been widely discussed in the academic literature starting with work by Polanyi (1958, 1966), and has been influential in numerous sociological studies of science.[2] To give a brief working definition that stems directly from Polanyi's work, tacit knowledge is knowledge that is not explicit: it is knowledge that has not been verbalised or encoded in any linguistic form.

Tacit knowledge is intimately tied to the problem of communication. Collins has evidenced in numerous case studies how the existence of tacit knowledge in experimental physics defines the boundaries of 'core-sets', the sets of legitimate experts that constitute a field of specialisation. Collins has shown that because tacit knowledge is an intrinsic part of any esoteric knowledge domain, the transmission of knowledge from one field of expertise to 'laymen' outside it (or to experts in other esoteric domains) is impeded by the inability to transmit the tacit knowledge on which full comprehension of the expertise depends. In Collins (1974, 1985), analyses of experimental physics replication has shown that mismatches in tacit knowledge arise when scientists belong to different fields of specialisation because tacit knowledge is highly localised and context dependent. An expert in an esoteric field of experimentation may not be able to communicate effectively enough with another expert in the same field that is working in a different laboratory, to the point that the second expert may be unable to replicate some of the first's experimental results. Collins argues that the reason behind this is that there is a component of tacit knowledge in scientific practice that can only be carried out through physical immersion in the situation where the tacit knowledge is being applied.[3] In this chapter I will introduce the tacit knowledge framework posited in Collins (2010), which includes a much more sophisticated typology of tacit knowledge than was present in previous works. Importantly, Collins argues that tacit knowledge comes in at least three main types, and these offer a much more sophisticated analytical tool than is found in the literature.

[2]See for example Collins (1974, 1985, 2001), Pinch (1981), MacKenzie & Spinardi (1995), Kennefick (2000), Schmidt Horning (2004) and Doing (2004, 2011).

[3]Nielsen (2011) offers a very interesting critique of positions that try to delimit esoteric domains by appealing to tacit knowledge, arguing that tacit knowledge has become the new 'ghost in the machine' for the definition of social groups, and is particularly critical of using this strategy to 'obscure' the social bonds that hold professions together. Nielsen nevertheless does not give an account of what the alternative explanation could look like.

3.6 Types of tacit knowledge

As Tsoukas (2003) argues, although tacit knowledge has been the object of much philosophical work and its study has influenced areas distant from philosophy such as management studies, most of the research shows minimal conceptual evolution beyond what Polanyi himself wrote. Since Polanyi, for example, tacit knowledge has been classified into two basic types, 'know-how' and 'know-that'. In the first case, tacit knowledge includes the classic case of bicycle riding ('I know how to do X', although I cannot explain it) while the second case typically includes knowledge that has been left unsaid. As Tsoukas notes, the second case ignores a central proposition from Polanyi: there is knowledge that may be not only unstated, but unstatable. This leads to the question of what is the source of this inability to state what someone knows, a question that has received careful examination in the work of Collins (2010), where he also traces the 'unstatability hypothesis' to the work of Wittgenstein (1953). Collins (2010, ch. 4-6) begins elucidating the problem by noting that one must think not only in terms of whether knowledge can be unstated or not, but also of why it remains unstated. He proposes three categories of tacit knowledge in order to better understand the problem:

1. Relational tacit knowledge, which is knowledge that has the status of 'tacit' or non-explicated merely by circumstance. It can be knowledge that has been deliberately been kept secret, or knowledge that has not been made explicit because the knower does not realise the importance of making it explicit. An example of the first case would be to withhold an ingredient in a food recipe when handing it to someone else, an ingredient that would prove crucial to achieving exactly the same taste as the original dish and which is non-evident. In the second case, one may not include a tip on how to perform a particularly difficult step in a recipe simply because one is not aware that the information is not evident to whoever the recipe is meant for. For example, when making meringue with an electric whisk it is crucial that the blades are ultra-clean or the eggs won't 'sponge'. A good way to do it is to clean the blades with lemon juice or a small bit of cream of tartar, which may or may not be mentioned in a recipe for making a Sachertorte, which requires meringue (according to grandmother Galindo who obtained the recipe from a Viennese chef decades ago). A seasoned chef is probably aware of

this, but may not put it in writing when handing the Sachertorte recipe to a less experienced person, simply assuming that it is 'a well known fact'. Relational tacit knowledge is the lowest type of tacit knowledge in the 'tacitness' scale because with enough effort or desire for transparency, it could be made explicit.

2. Somatic tacit knowledge has been central to the topic since Polanyi's famous example of bike riding: no amount of explicit amount of bike-balancing instructions will prepare an aspiring rider to accomplish the feat; the novice must simply try and try again to ride successfully, and after a few falls will (most likely) accomplish balance. Because of the Polanyi connection to physical body action, somatic tacit knowledge has been the hallmark of all other types of tacit knowledge; Collins' new typology goes beyond this initial definition. While recognising the importance of somatic tacit knowledge, he classifies it only in a mid-range category within the 'tacitness' scale. In theory one could build a machine that could achieve bike-balancing performance; without going into the argument, Collins supports the idea that when tacit knowledge can be mimicked by a machine, it is in principle explicable. "We ask the standard question once more: can all somatic knowledge be made explicit? The answer is 'yes' if here 'explicit' means 'expressed scientific understanding of causal sequences'."[4] Along with bike riding, Collins presents chess-playing computers as an important example of how somatic tacit knowledge can be embedded into a machine; the somatic comes into play when one realises that these machines are intended to mimic an essential part of the human body: the brain. This point will become important further on because of the role that mental processes play in theoretical physics as opposed to physical acts. Thus we might simply say that somatic tacit knowledge is tacit knowledge that can be gained only through actual attempts at implementation of successful praxis, be it related to the soma or to the psyche.

[4]The full argument is set out in Collins & Kusch (1998). Collins (2010, ch. 5) recognises that "there is nothing philosophically profound about somatic tacit knowledge, and its appearance of mystery is only present because of the tension of the tacit with the explicit: if we did not feel pulled towards saying what we do, and if we did not make the mistake of thinking this central to the understanding of knowledge, we would find nothing strange about our brains' and bodies' abilities to do the things we call tacit. And that is why too much concentration on the body as the seat of the tacit takes one away from a proper understanding of the idea."

3. Collective tacit knowledge is the most sociological and tacit of tacit knowledges, because it can only be gained through full immersion into the social and cultural context where the knowledge is relevant. Collins places the 'location' of this tacit knowledge not on the 'individual', but in the 'collectivity'. At best, the individual can 'tap into' a reservoir of collective tacit knowledge, but the individual cannot transmit it in full to another individual who has not tapped into the same reservoir. Collective tacit knowledge can be transmitted to an individual who becomes socialised into a collectivity, but it cannot be exhaustively transmitted between individuals.

Notice that collective knowledge in general need not be tacit. In fact, this is seen by remembering a central point of the early sociology of science: knowledge in science does not become knowledge until it passes the filters of the collective. Once a localised bit of knowledge becomes 'a fact', say, when an experimental result is published and becomes acknowledged as one of the papers that should be canonically cited by theoreticians, the result will probably be disseminated far and wide. Authoritative texts or handbooks that are widely accessible will probably cite it, thus ensuring that practically anyone may 'tap' into this knowledge. Thus the knowledge becomes collective, and explicit.

3.7 Interactional expertise and Collins & Evans model of expertise

The nature of expertise in esoteric domains, its foundation on tacit knowledge, and the access to expert 'languages' by laypeople and experts from other domains has been analysed in depth by Collins & Evans (2007). One of the most significant conceptual developments of this theory is the introduction of the category of 'interactional expert' to expertise studies. Interactional expertise is defined within Collins & Evans' framework as "expertise in the language of a specialism in the absence of expertise in its practice."[5] Collins & Evans consider interactional expertise is exhibited by demonstrating mastery of the language of a domain by people who do not necessarily contribute

[5] Collins & Evans (2007, p. 28).

to the development of the expertise domain.[6] According to Collins and Evans, one of the aims of introducing interactional expertise is to fill a conceptual gap between the two traditional ways of treating expertise in a language domain. Collins & Evans name these the 'Informal View' — which says that to master a language requires full immersion in a form-of-life — and the 'Formal View' — which claims that all that is needed to master a language is the acquisition of sufficient amounts of propositional knowledge. One of basic tenets of Collins & Evans' model is that any and all forms-of-life incorporate sufficient amounts of tacit knowledge to render the Formal View untenable. That is, mastery of a language associated to any form-of-life is intrinsically embedded with tacit knowledge, which is not prone to communication or transmission without contact with the 'parent' culture.

The existence of interactional expertise allows the possibility of bridging the gap between two genuinely different forms-of-life or cultures because interactional experts are, by definition, proficient in the language and form-of-life of a group outside their own, by establishing 'parasitic' language relationships with other cultures or socio-linguistic groups. Collins & Evans claim that one can master the language of a form of life without physical immersion in the form-of-life corresponding to that language by upholding prolonged and continuous linguistic immersion in another culture's talk, the so-called 'minimal embodiment' thesis. A central point of Collins & Evans' model is that the acquisition of interactional expertise is crucially carried out through socialisation and language enculturation processes, which are the only possible ways for the transmission of tacit knowledge to occur. Socialisation leads to the acquisition of tacit knowledge, which is an indispensable component of interactional expertise. However, Collins (2010, p. 137) explains in reference to the importance of tacit knowledge and interactional expertise that "drawing on the tacit knowledge of the collectivity through language alone is often not the most efficient way to do it. Engaging in physical activity with other people tends to create more opportunities for conversation than engaging in talk alone, so even if the sole means of transmission was the word, the transmission would be enhanced by physical activity." Although interactional expertise can be acquired in principle exclusively through talk and without physical immersion in a do-

[6]In SEE jargon, full-blown experts that possess the full skills of an esoteric domain are said to have *contributory expertise*. Contributory experts have both contributory expertise and interactional expertise.

main, physical immersion speeds up the process.

In the previous chapter, the contact between close cultures, such as the 'first-principles' and 'data-fitter' cultures, automatically allows constant interaction to occur, and therefore interactional expertise between these cultures arises naturally. In contrast, physics cultures that have little contact with each other and whose social networks do not overlap do not develop interactional expertise ties. High-theoreticians who migrate to a distant phenomenological culture must then carry out enculturation into the new social group. Of course, tacit knowledge is not the sole component of enculturation. The acquisition of a 'basic vocabulary' of explicit knowledge is necessary for any activity that involves other human beings, be it commercial exchange, simple banter, or academic collaboration. But the main barrier to overcome by an aspiring interactional expert who wishes to communicate in significant ways with an expert within that expert's own form-of-life is gaining tacit knowledge. This is, to stress the point, akin to learning a new language and wanting to practice it proficiently in a foreign country not as an alien foreign speaker, but 'as a native'. It is not enough to know the correct way of constructing sentences, or avoiding spelling or syntactic mistakes, or having a large vocabulary which are all explicit knowledge skills. Although these skills permit basic communication to occur, they are not the defining features of a native speaker.

3.8 Delimiting esoteric expertises

In order to understand how expertise domains can be delimited, and how tacit knowledge plays a part in this, one can begin by considering the signals whereby expertise is exhibited:

1. Knowing something.
2. Knowing how to do something.
3. Being able to do something.
4. Being able to talk about doing something.

In any social group there is a wide range of things that people know, know how to do, and do well. For example, living in the United Kingdom successfully (in a sociological sense) implies a number of things. It may be something as trivial as knowing that one must drive on the left side of the street, or something as subtle as knowing that one should never skip a queue wherever there is a queue (or for that matter, knowing the meaning of the word 'queue', since the word is unusual for most North Americans). This 'ubiquitous' knowledge can even lead to extremely different ways of applying institutionalised rules: bribery is an accepted and often times required part of Mexican everyday life that would be heavily punished in most British situations. Similarly, drunkenness in Mexican streets is severely prosecuted, when in Britain it is tolerated in many public places. Collins & Evans refers to this sort of 'self-evident' knowledge as *ubiquitous expertise*. One is 'an expert' in these matters simply in virtue of belonging to a particular social group, or being enculturated into a group.

Collins & Evans recognise that by virtue of growing up within a society, one is already 'an expert' (one possesses, at the very least, the tacit knowledge pertaining to that society which a foreigner would not have). In Collins & Evans model one must presuppose that there is such a thing as an expertise 'universe' in order to start any sociological analysis of esoteric or specialist forms of expertise. Thus, proper bribing etiquette is not particularly esoteric if the 'expertise universe' is defined as all Mexico, but it is not ubiquitous at all if the UK population is the universe-set. The sociologist can in fact limit the 'universe' by defining what 'ubiquitousness' is for each context. A sociologist could start their analysis taking not the culture of Britain as ubiquitous, but limiting oneself only to Cardiff. 'Ubiquitous' would then mean the sort of thing that a Cardiff local would likely know.

For the purposes of this work, the ubiquitous universe is not the wider world that Collins & Evans originally contemplate, but is much more restricted: it is the 'universe culture' of research physics and of the professional physicist. I will borrow loosely from a definition concerning "the things that any physicist should know" given by one of my interviewees: the knowledge that is generally acquired by the middle stages of graduate work, either in experiment or theory. In reference to the amount of knowledge that one can presuppose an audience of physicists possesses and needn't be explained when giving a talk, Tong (2011) gives the following heuristic pointers:

> There's no hard and fast rule which determines the amount of knowledge you can assume of your audience. It depends very much on who you're speaking to and you should try to get a good idea of this when preparing

the talk. But a good rule of thumb is that anything taught at an advanced Masters level — say the level of Part III in Cambridge— can be assumed to be common knowledge.

3.9 The ubiquitous universe-set and lower levels of expertise

Collins & Evans' connection with Collins' tacit knowledge scheme is most noticeable when one begins to analyse specialist esoteric domains and particularly how knowledge from these 'spills out' into the wider world. Although autonomous expert domains are by definition 'closed' to the outside world, there is a flow of knowledge from them to the outside, as clearly occurs in science and it various connections to 'the public'. Although the wider public has very limited knowledge of the scientific world as seen 'from within', scientific 'facts' are part of many non-scientists' cultural baggage: it is 'a known scientific fact' that man is genetically related to other apes, or that the Earth moves around the Sun, both facts being familiar to laypeople with minimal scientific literacy. Making a simile with another esoteric domain, it is also common knowledge that white wine goes better with light meats while red wine tastes better with red meats, or that Mexican food is quite hot. Knowing this does not imply that one is in any way a gourmet specialist, even though it does imply possession of a piece of knowledge related to an esoteric domain. Again, what is specialist or esoteric and what is ubiquitous depends on the sociological universe that is chosen. If the universe-set is the wider world of British society, then the above bits of knowledge are ubiquitous. In contrast, any physicist possessing an undergraduate degree— even an experimenter— is expected to know how to derive the wave equation for light propagation using Maxwell's equations in vacuum. It is ubiquitous knowledge for the physicists-layman but not for the 'general' layman.

Once the universe-set is chosen, one can start looking at how esoteric domains arise there. Thus, the wider world of British culture exhibits all sorts of esoteric fields of expertise: literary circles, professional academies, sports stars, etc. These domains are populated by individuals who possess skills that are beyond the reach of the rest of the wider social group, and that define the crème-de-la-crème of these esoteric domains; these are named *contributory experts*. Nevertheless, Collins & Evans recognise that individuals may still posses 'lower' levels of expertise or knowledge about these domains, even though they do not contribute towards the evolution of the esoteric domain itself. There are three types of knowledge about specialist domains within the universe-set that 'outsiders' to these specialist domains can acquire. In order of increasing sophistication these types of knowledge are:

1. Beer-mat knowledge. The enunciation of this kind of knowledge is parrot-like and there need be no other skill implied than to enunciate the 'facts', as in a pub quiz.

2. Popular understanding. There is a mild comprehension of the mechanism that gives rise to the knowledge that the knower can in articulate. Popular science books inspire the terminology, in most of which the author tries to teach the reader a bit more than 'just the facts' and provides more background aimed at a better comprehension without going into a technical discussion as an esoteric expert would have with another expert. The knower can then give a rational reconstruction of the mechanisms of knowledge production, with a bare-bones description of the mechanism whereby the knowledge came to be.

3. Primary source knowledge. In this case one has actually read and understood, located or is able to cite the sources of knowledge, and work out their basic principles as stated in the primary texts of an esoteric field.

The problem of communication in physics can now be set up using Collins & Evans' mosdel. Essentially, the problem involves understanding how in the expertise universe-set that has been chosen (physics as a whole) the different esoteric domains (the lozenges of the horseshoe diagram) are inter-linked, as shown in Figure 3.1.

3.10 The importance of interaction

In the following sections I will concentrate on elaborating on how collaboration arises between epistemically closer micro-cultures in physics collaborations where the micro-cultures are forced to interact. Collins & Evans posit that in order for true comprehension to occur, there has to be prolonged linguistic immersion in the 'foreign' culture or

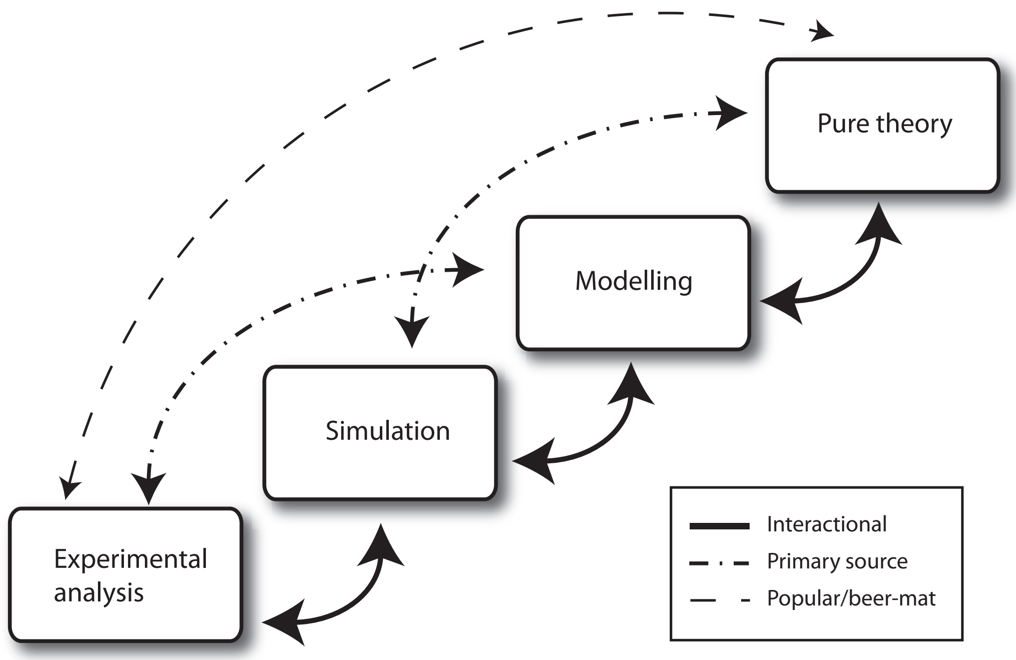


Figure 3.1: Physics communicating using Collins & Evans' model. Only a portion of the 'unfolded' horseshoe is shown, with the chain continuing at each end using the same mechanisms. The interactions at the edges of the diagram also rely on the establishment of sociological trust, which will be treated in depth in Chapter 4.

expertise, unlike the case of isolated high-theoreticians that have no such exposure, and thus that cannot apprehend tacit knowledge or development of interactional expertise. Consistent with the Collins & Evans' model, theoreticians that were interviewed who are in collaborative *projects* with experimental groups emphasised the importance of personal and prolonged linguistic interaction:

> Horgan: You have conferences or things like that or smaller meetings. So we had a meeting this weekend, and Matt next door has gone to Durham, and so he's taking some of the preliminary results up there and he will discuss with these people, they will go away and then of course they can e-mail back. So you can then set up a dialogue where they don't quite understand exactly what we did, you see. And we don't understand what they want to do. So you have to understand their physics. They sometimes are sceptical, and we're sometimes sceptical because you think, 'You didn't do this. You didn't do that. How does this work? I don't understand that.' And then either that's true and they change or you change, or it was a misunderstanding and so very gradually you can pull their understanding into your thing but that's because you communicate [...].

When asked how long the establishment of cooperation usually takes, Horgan made it clear that it was a time consuming process of *years* of duration:

> Reyes: How long does [the] communication setup take?
>
> Horgan: Well, sometimes it can take years. Normally...it depends if the calculations are large. In this kind of lattice QCD a computer calculation can take months. Partly writing the code, debugging it, but also just the processing. So in this particular project I'm talking about, through a two-year project you get these numbers. Now there will be a sort of post-project interaction with people. And maybe it doesn't come to anything in the end. Maybe it was too hard. It's important to have a relationship with the people so that you're not just talking to a machine through a machine. So you don't know such and such a person...you communicate with them, you meet them at some workshop, and then maybe they come over for two weeks and so on. And then you have a long term...or sometimes

> you don't, but know you know them. You can talk to them. You can ask them, 'I don't understand this, can you tell me about that?' and so on. So yes, it can take a few, you know six months only if you get the initial thing going but it could take...it could have this effect. Your group just gets bigger, at least your local group where you can walk next door, but there's all these people like the people in this picture who are in Ohio and Dallas and...you name it.

3.11 Collaboration in small- and mid- scale physics

Theoreticians like Tong or Berry tend to work in isolation from experimental physics, and high-theoreticians tend to be the 'lone wolves' of physics. One of the more mathematically oriented theoreticians interviewed even commented:

> No real theorist ever works in a group. You never say a 'team' of researchers, and I immediately...when somebody says this— not everybody, but far and large— they're not thinking clearly.

This is a rather idealised view for theory as a whole, as some collaborations in modern physics do require significant theoretical input beyond what single individuals can provide. For example, teams of researchers are increasingly common as theoretical work in particle physics becomes more and more complex. Particle physics led projects such as the massive experiments that form part of the European Organisation for Nuclear Research (CERN, from the original Conseil Européen pour la Recherche Nucléaire) require the coordinated efforts not only of hundreds of experimenters and technicians on the site, but also of theoreticians working in collaborations in CERN headquarters and smaller 'teams' around the world. This same pattern is followed in many other areas of modern experimental collaborations that require theoretical input:

> Mondragón: I'm [...] interested in dark matter, on both direct and direct detection of dark matter, and how it relates to these models beyond the Standard Model, because with new cosmological measurements — you can no longer ignore the precision of astrophysical and cosmological measurements when you write up models beyond the Standard Model — and

> we have worked with a group of very interested people on a criterion for entropy. There we have people that do general relativity, me and another guy doing particle physics, people who do more of astronomy, and this project we have kept on going. What we are trying to do is find independent criteria for particles: abundance, astrophysical processes, etc. and to contrast them with concrete suppositions of possible models or extensions and see if these are compatible. This has proven very interesting and it's a large working group because you need people from many fields.

Horgan is another theoretician working in collaborative work that involves particle physics simulations:

> Horgan: This is the webpage for our particles physics collaboration, HPQCD, that's High Precision Quantum Chromodynamics. [...] All these people do simulation, and I do calculation [...] So there's a whole QCD thing there, and that relates to experiment, and it's meant to help the experimenters see if there are any anomalies in their experiments that could be 'beyond the Standard Model' physics.
>
> Reyes: When you say you give this information to experimenters...you produce a number, and then you give them that and what do they do with it?
>
> Horgan: Well, I mean, it takes a long time. We haven't done this yet. [...] There's an experiment called LCHb which is looking at the physics of b-quarks. They will measure the decays of b-mesons which are mesons made of a b-quark and a light quark like an anti-up or something. [...] So it tends to be one ingredient in a much bigger attack on the calculation, and so in the end 'the best' number will come out, not necessarily straight from us, but from us in collaboration, or we'll write a paper, or somebody else will use it and say, "ah, this group in Cambridge has done the best number calculation of this and now we can put this into this model over here and that reduces the systematic errors on that," and eventually somewhere down the line we'll pop out the best number for the decay rate. [...] So yes, in the end the knockdown thing is that you give the experimenters a number, and they see if the number agrees.

Horgan's 'collaboration' involves only theoretical calculations, not an orchestrated effort with experiment. Horgan described the interaction with experiment as forming "a chain" where his group is only one link in the path to experiment, which suggests a picture similar to the horseshoe diagram where each link in the chain works independently from the others. Horgan identified four links also similar to the diagram: a group above his that did 'analytic calculations', his which supplied QCD parameters to a group that uses them to make the actual predictions, which are then handed down to the group that actually carries out the experiments:

> Reyes: So you produce your number and you hand it to whoever connects in the chain.
>
> Horgan: Yes, yes.
>
> Reyes: Who would that be?
>
> Horgan: It might be...I don't hand it directly, right? You publish the paper. The paper goes out. The title obviously attracts the other people. So they might be the experimenters directly who have their own pet theorists, but it might be other people who have a parametrisation for the decay rate which takes into account things we can't calculate but in the middle sits this number we can calculate. I mean, *I can't tell you their names.* I can think of people that might do that. [...] Sometimes somebody has an idea but there's a hole in the calculation, and they can't do these calculations, so it's not important for them to finish this idea. But then somebody publishes a paper who calculated this thing, and suddenly now you can do your own thing and use that, and then your idea becomes much, much better and that happens you know. We had an idea in this group some years back to actually do some calculations but using the computer, but we wanted to compare it with an analytic calculation. [...] The point is that these other guys did a calculation that was completely independent of what we wanted and for a completely different reason and we had an idea, "ah, wouldn't it be nice to do this," and we can do some things with this method, but then putting the two together actually gives you a much better result. *So it's important to just seed the world with this knowledge in chunks, and it gets picked out. That's kind of how that works.* (emphasis added)

What implication does this have for the communication problem? Horgan's account provides a highly fragmented view of his field of physics, so that even within collaboration 'chains' the basic method is to work within one's own speciality and 'seed' the world through publication. Horgan stressed that once the arduous task of setting up the computational collaboration are finalised, particular results are taken up by groups who he did not know well enough to be able to name them on the spot. Again, one gets a picture of physics where direct interaction— when and if it is needed— is set up by processes of prolonged dialogue. Once a result is produced, the groups can publish the result, in the hope that someone will take up this finalised knowledge.

3.12 Interactional ambassadors in LIGO

If Collins & Evans framework is to be posited as a general model of communication in theoretical physics, it should not be restricted only to particular interactions between individuals, or small groups like Horgan's where there is a 'natural' tendency to set up fragmented workgroups, but should also be able to describe physics at large and in more complex settings. An example that can be compared to Romero's person-to-person collaboration and Horgan's small-scale collaboration and that has been the focus of decades long work by Collins (2004, 2010) is LIGO (Laser Interferometer Gravitational-Wave Observatory), a massive project for detecting gravitational waves that involves hundreds of scientists around the world. One would expect that in large-scale collaborations such as LIGO that *require* the coordination of physicists from all over the horseshoe diagram (unlike in small-scale collaborations like Horgan's where once the group's work is done one simply 'seed' the world with one's result), communications between the micro-cultures of physics would be much tighter than outside them. But as will be shown, LIGO is also fragmented into expertise domains that retain high degrees of autonomy that rely on SEE type mechanisms to achieve coordination and knowledge transfer.

S. Fairhurst leads a group in LIGO which carries out numerical analysis using the raw data generated at the experimental site. Within the horseshoe diagram, his group is directly next to the experimental group; the interaction could hardly be more direct. As such, he is as close to experiment as any theoretician can get in terms of his objectives. But not only that, for as part of LIGO there is an institutional drive to coordinate work that is absent in small-scale physics. Nevertheless, according to Fairhurst the closely-interacting groups have *not* established an inter-group language to understand each other better, or as permanent interaction as groups, despite having been collaborating for years. In fact, Fairhurst does not feel enough rapport with the 'on site' experimenters to call them up directly in order to discuss some problems with the data:

> Reyes: How does the actual communication go about? Do you call up [the experimenters at the site]? I was curious as to whether you just picked up the phone and...
>
> Fairhurst: Some people do. I don't know the guys at the site well enough to just pick up the phone and say, 'this is killing us', but I know the person I would talk to who is my expert and there's a chain...I don't know how it would get to them!

When asked about this 'expert', Fairhurst explained there was a student in his group who had been sent specifically to develop skills as a 'liaison' so that his groups could have a contact point with the experimenters on the site. The method to achieve this was for the student to be physically present at the site, so as to gain 'a foot in each camp' and be able to 'translate' for the data analysis team:

> Fairhurst: You need a few people with a foot in each camp who can almost translate, but there are a lot of people who just don't care to make that effort. I've done a bit, but you know one of my PhD students went to the site for four months and this was great for us because he came back and we'd hear something about what the detector did. And he's been there and helped work on it and he'd say, 'Yeah, that means this.'

This setting up of 'ambassador' or 'translator' based interaction has been posited in previous SEE-related work such as Collins (2011) and Ribeiro (2007b), in models of communications relying on the concept of interactional expertise. Collins has classified these 'ambassadors' as possessing 'specialist interactional expertise', to stress that they have no practical and only linguistic immersion in the domain that they translate from. Collins (2011, p. 287) confirms the existence of these 'interactional ambassadors' or 'special interactional experts' in gravitational wave physics, and Collins finds that in LIGO

> "groups of physicists do not speak each other's practice languages. The solution is to delegate particular individuals belonging to the GW physics-practice to learn some astronomy practice language, to gain interactional expertise, and to form bridges with different kinds of astronomer [...] Each delegate has to become a special interactional expert with respect to the community to which he or she is to build a bridge. The delegated individuals, in so far as they succeed, can then answer technical questions and queries from GW physicists on behalf of, say, x-ray astronomers, without always referring back to those astronomers — this is how one detail of the technical cooperation between these middle-level practices is made possible."

Collins' findings were echoed by Fairhurst himself:

> Fairhurst: We have our weekly meeting about what we analysed the last week by telecom. We'd get our story together and try to poke in on this data. Sometimes we will come to them. If my student was at the site he'd just come and pass it on. Then there were e-mail lists and things, but I think generally these list things kind of don't work so much. To do something like that you need a point-to-point contact between the two groups, right? And that could be someone who's visiting somewhere, or it could just be a relationship that's grown up. So we work out what we want to say, and then whoever is sort of our liaison goes and talks to the person at the other side. Maybe they go to their teleconference and they summarise for the other people something. *In reality it's only a handful of people who are really at the interface.* (emphasis added)

3.13 Interactional expertise and management skills

In LIGO, group coordination is carried out using the same sort of mechanisms as in Horgan's and Romero's case, that is, by establishing interactional expert links through prolonged interaction. In Romero's case, where the theoretical side is handled exclusively by he himself, the interaction is necessarily direct. In Horgan's case, a few select members establish these bridges, but the group being small this is still a significant part of the collaborating population. In LIGO the same mechanism is followed, but the scale of the project being at a completely different scale, the interactional expertise dimension and the establishment of interactional expertise through personal immersion is much more visible.

Nevertheless, LIGO is different to small-scale physics in that there is an overarching coordination driving the day-to-day work. In order to achieve this, there need to be individuals that can somehow see the project 'as a whole', in much the same way as a good manager at a factory needs to have a bird's eye view of the entire assembly line processes that lead from the worker's individual skills to a final product that receives multiple small-scale inputs. B. S. Sathyaprakash, who is part of the 'theoretical' data analysis group but has been involved in LIGO since its inception, commented on his need to acquire such a wide-angle vision:

> Sathyaprakash: I have been interacting with [all] of these communities. I think it depends to some extent on how deeply you want to get involved in various aspects. What happened to me is that I entered the field in the early stages where most of the people involved that I was talking to were experimentalists. There were not many theoreticians. There was not data, so therefore there were no data analysts. [...] I had to talk to these guys. I had to understand the detector first and foremost. [...] You can say that I am a theoretical experimentalist in that sense. The difficulty arises when you don't know the language, and that can happen. I do interact with all these people, but at a level where I need to understand how I can pattern the science behind it, not much more than that. If you try to do their job, then it's not good. You have to do your job. You try talk to them and extract as much information as possible, in trying to understand how the science that we are doing is going to be impacted. In some sense all that you might need to worry about is... 'oh, I will just worry about the sensitivity. Tell me, what is the sensitivity of your instrument?' But that's not enough, because they might have different choices. So you need to probe, and tell them that, 'look, if you try to gain a little bit here in this frequency region I might be able to do this science, and try to probe a little bit of the technology involved. And try to get a feel for it. [...] You try to

understand what they are trying to do.

Sathyaprakash's role within the LIGO collaboration includes senior managerial duties, which has thus led him to develop interactional skills in order to communicate with the four larger-scale communities that make up LIGO (experimenters, data-analysts, theoretical data analysts, and experimental-prototype theoreticians). Sathyaprakash requires the ability to interact with each of the teams in their own terms. Collins & Sanders (2007) have analysed the skills that managers require in cases where this happens, having concluded that it is through interactional expertise that they are able to carry out their work. Although Sathyaprakash is unlikely to have gained full interactional expertise in order to comprehend the aspects of the entire LIGO collaboration's work (an inordinately large amount of immersion and acquisition of tacit knowledge would be necessary otherwise) he has developed enough specialist expertise to become a 'high-level' liaison between the groups. As in day to day interactions, the importance of personal interaction is mentioned as the way to deal with communication barriers, and as SEE argues, this immersion in the language of an esoteric domain in turn leads to interactional capacity.

3.14 Breaking down tacit barriers in LIGO

Relational tacit knowledge is reported to be a major hindrance to inter-group collaboration in LIGO, pointing to the generalised establishment of localised 'languages' in the collaboration that make cross-lozenge communications difficult. Relational tacit knowledge arises which facilitates in-group communication, but hinders contact with outsiders. The way to overcome the relational barrier is, of course, personal contact:

> Sathyaprakash: People very soon will start using jargon, jargon which is not fundamental physics. But the jargon is needed, because every time you don't want to say a very, very long sentence to define what something is in terms of fundamental things. Over the course of their training— specially younger colleagues— they don't know that this is not fundamental physics, that it is something very specific to their field. Very soon there is a tendency to get lost in jargon. That creates a language barrier. How do we avoid that? One idea is to sit face to face with people and ask them,

> 'explain this to me. What do you mean by this?' You can do it this way, or you can try to ignore parts that are not important.

Faihurst is also very aware of the growth of 'jargon' within specialties, but unlike laid more stress on how it facilitates in-group communication:

> Fairhurst: It comes back to this 'do we speak different languages?' Yes. We do. I expect it's the same with the other people you've interviewed, but in our field we use more acronyms than you can imagine. Everything is an acronym, because it saves time. And so if you're in my little clique you know the acronyms we use. We use them everyday, and that's fine. And the experiment people use their own. You have to put in a little bit of legwork to understand the main ones or they won't even talk to you, right? And if they use some more obscure ones you can kind of call them. But yes, speaking the same language, understanding the basics. Yeah, I think generally we assume people know more than they do. It's very rare that you assume people know less than they do. And also we assume we know more than we do.

This is clear evidence of how mismatches in tacit knowledge create real-time difficulties even amongst groups that are tackling closely related empirical work, but with one in the experimental and the other in the theoretical terrain. Even though Fairhurst's group and the people at the site have huge overlaps in the pieces of LIGO that they develop and study, this is still not enough to create a communal language that is unambiguously understood by both groups.

3.15 Complexity and specialisation

The complexity of the chains of knowledge is linked to the high degree of professional specialisation that has become intrinsic to modern theoretical physics. For a single individual to become an expert in every part of the chain seems highly unlikely or even impossible. The story of Enrico Fermi as the last of physic's polymaths has become standard lore in the history of physics, particularly that of the 'Golden Age' of quantum mechanics:

> Tong: It's usually said that Fermi the last guy to do this [work within the whole spectrum of physics]. I don't know if that's a true statement. Certainly I don't know people that do it. I know of very impressive people that work on string theory, the geometry of extra dimensions, and at the same time do nuts and bolts work of top quark analysis from the data from LHC, so they get their hands on the raw data and try to sift through and understand what's top quarks and what's not. That guy is a fairly extreme example because it seems that— to me at least— his two bits of work don't overlap, but to go all the way through, I don't think so.

Technically, it is not impossible for a theoretician to also work in a laboratory (one theoretician that is now in charge of running an experimental optics laboratory and actually setting up experiments was interviewed for this work). But while the Fermi story may not be completely accurate, it would be feasible to argue that Fermi was perhaps the last physicist to do research in both theory and experiment at the level for which he became legendary.

Specialisation thus naturally occurs in any collaborative work, even when wholly theoretical. Wray (2005, p. 153), commenting on D. de S. Price's work, highlights the growing complexity of published papers in modern science, noting that specialisation occurs "because there is a limit to how much people can read each scientist can attend to only a finite and rather small portion of the continuously growing body of scientific literature." He identifies Price's work as constituting a second stage of STS work on scientific specialisation, which concentrated on how specialisation occurs as a scientific field grows, becomes crowded, and the younger generations tend to seek new niches for their professional development. Law (1973) has shown how specialisation occurs even in small communities that are otherwise homogeneous in their background research commitments by studying the diversification of techniques in British crystallography and the emergence of the new field of protein crystallography, and how these communities eventually grew to become autonomous fields of specialisation. Collins (2011) has linked the existence of division of labour within large scale collaborations to the emergence of a common interactional domain that allows the autonomous practices to function as a whole, with linguistic socialisation functioning as the integrative element between disjoint groups.

In small collaborations one individual may cover each of the horseshoe categories, or it may happen that single individuals may be competent in several categories simultaneously (as in Romero's case where he is basically covering many of the phenomenological categories at the same time). In complex collaborations between theoretical and experimental physics, the links in the chain become more closed in on themselves as fields of specialist expertise. As complexity and specialisation grows, the sociological location of individuals within the diagram becomes more focused.

3.16 Comparing Collins & Evans' approach to Galison's model of communication

If two foreigners whose mother tongues are completely different and who do not share a common language are forced by circumstance to cooperate and achieve a coordinated goal, how could they manage to do this? Galison (1997) has presented this linguistic puzzle as analogy to the problem of communication between the micro-cultures of physics, along with a highly influential answer in STS, which will be considered here in detail and juxtaposed to the Collins & Evans approach, as it has also been analysed in depth by previous SEE-influenced authors.

As one of the most influential authors to tackle the problem of communication in science directly, for Galison experimenters and theoreticians are not two sorts of physicist differentiated by efforts concentrated on different problems, but can be better understood as two different cultures embedded in the larger culture of physics. Galison thus recognises that in this sense, physics is a disunified science. Based on historical work on experimental traditions in early 20th century physics Galison found that along with these two traditional cultures, one could define a third culture which was of equal importance to the other two— technology— thereby increasing the complications for the problem of communication. Galison showed that the development of experimental physics was not synchronised with either theoretical or technological developments, and therefore that the emergence of 'revolutions', 'paradigm shifts', etc. in one of the cultures cannot be mapped one-to-one to those in the others, further supporting the view of a fragmented culture of physics; theory, experiment and technology ought to be treated as autonomous micro-cultural entities, and conceptual mismatch between them is as real as the linguistic incommensurability between two groups that speak different languages.

Galison's historical accounts include experimenters, theoreticians and technologists found to be working on common projects and goals which required input from all three micro-cultures. In order to explain how communication does happen, as it is seen to happen in such collaborations, Galison put forth the idea of 'trading zones', linguistic spaces — possibly but not necessarily associated with physical spaces — where hybrid proto-languages develop that take elements from both parent languages ('pidgins' and 'creoles', in increasing order of sophistication). The hybridisation begins with simple exchanges of words to which both micro-cultures give common meaning, after which the trading zone language can gain in complexity so that if the trading zone is sustained for long enough it is possible for a new autonomous language to develop. Taking his cue from linguistic studies describing such cultural clashes, Galison saw the coordination of the three micro-cultures of twentieth century particle physics and their developments as cases of the emergence of trading zones.

Galison (1996, p. 153) has exploited the metaphor in other historical studies to highlight the role of trading zones. In his analysis of Monte Carlo simulations in nuclear physics, Galison for example states that "in the heat of the moment, a kind of pidgin language emerged in which procedures were abstracted from their broader signification. Everyone came to learn how to create and assess pseudorandom numbers. [...] Everyone learned the techniques of variance reduction. [...] By the 1960's what had been a pidgin had become a full-blown creole: the language of a self-supporting sub-culture with enough structure and interest to support a research life without being an annex of another discipline, without needing translation into a 'mother tongue." A few lines later Galison points out that "of course not everyone shared all the skills of this new 'trading zone.' Some focused on the game-theoretical aspect; others, more on variance reduction or convergence problems." Galison clearly showed that Monte Carlo developed into a technique that grew out of the localised context in which it was developed and offers evidence that it indeed became an autonomous area of expertise, as did all of computational physics. But as far as the intermediate hybridisation process is concerned, he only affirms that the motley crew of professionals involved in the development of Monte Carlo "could and did find common cause" without any evidential support. In fact, Galison points that in these interactions, "individuals [...] could alternate between problem domains without difficulties." This is not far from Kuhn's observation that scientists can and indeed do switch between paradigms when looking at different problem domains, a picture which Galison explicitly wants to reject with the creation of trading zones as 'intermediate' linguistic zones between the independent micro-cultures.

Galison's model is one of scientific communication between autonomous micro-cultures in general, and so one can examine whether interaction between scientific micro-cultures are generally based on the establishment of trading zones. Going back to the linguistic metaphor, if this were the case it would mean that when two foreigners interact, the only means they have of promoting collaboration or communication is to establish pidgins, creoles and trading zones. However, although we know that creolisation happens in certain contexts, it is not the only way in which communication happens between individuals from different cultures in general. One can for example use the services of a translator, or what is perhaps more common, one of the parties involved might learn the language of the other person's culture. Indeed, one need not go that far if one allows that 'interactions' may not necessarily be limited to close partnerships or personal contact. One could, for example, simply read the English translation of a Spanish speaker's biography to gain insight into that person's life even if one does not understand Spanish, and thereby gain some sort of insight into that person's culture; although this is hardly interaction, it does imply transmission of knowledge, which is definitely one dimension of scientific interaction.

3.17 An analysis of the Galison-type trading zone model

The most severe limitation of Galison's model is that it assumes that all relations between micro-cultures are of a very particular kind, one where the two parties involved have the same footing in terms of, for example, resources or power. Critiques of this default position have been put forward by Collins et al. (2007) where trading zones are classified along two axes: the relative power between the groups, and the degree of the homogeneity of the trading zone. Galison's position in the Monte Carlo case is identified as a maximum balance of power, maximum heterogeneity class of 'boundary object' trading zone (classified as a type of fractionated trading zone), where although the people involved all 'talk' about the same object, they do so from within their partitioned expertises with no culture dominating the other.[7] In the case of full creolisation, the trading zone is located in the maximum homogeneity, maximum balance class (referred to as an inter-language trading zone), since a 'new' culture develops that has its own autonomous elements of self-identity independent from the parent culture.

Collins et at also consider that in the case of fractionated trading zones there is the possibility that instead of choosing to construct a boundary object the interaction may be shaped on the establishment of interactional expertise, which in linguistic terms would be equivalent to the full immersion of the parent cultures into the language and culture of the other. The end result is that some of the members of the parent cultures become 'interpreters' of the other language and culture to the members of their own culture. Ribeiro (2007b,a) has extended this idea to include cases of exchanges where the interaction is carried out by external interpreters in technically challenging settings where the members of the parent cultures interact minimally with each other. Ribeiro in fact argues that in some cases the language barrier can be an aid to communication for interpreters, as they can choose to ignore pieces of conversation that would be considered offensive were they translated because of culturally offensive content that is unknown as such to the speaker. In interactional expertise-mediated interactions, although the power is balanced in that no group dominates the discourse or the cultural resource of the other, there is minimal interaction between groups, and only few individual of one group become 'ambassadors' to the other. Ribeiro's contribution is relevant to the problem of communication in physics as it adds another dimension to it, posing the question of whether all exchanges of information need be carried out between fully interacting cultures in order to be significant, or if indeed full interaction between the cultures is always the ideal situation to be aimed for. Nevertheless, the case described in Ribeiro (2007a, p. 562) is one where "two radically different language groups can be brought into a productive relationship with one another through the mediating role of an interpreter who acts as a buffer between the cultures".

The central thesis in this work is that within physics, interactional expertise acts as the main bridge between the dissimilar forms-of-life that make up the horseshoe diagram. This does not mean that Collins & Evans' model rules out the establishment of Galison-type trading zones, but it does relegate them to at most a complementary position relative to the establishment of interactional expertise. It is in fact possible that

[7]The concept 'boundary object 'was first introduced in Star & Griesemer (1989).

interactional expertise and trading zones work side by side in a number of instances. For example, Fairhurst himself has been instrumental in developing programs within the LIGO collaboration that exhibit the properties of a trading zone:

> Sathyaprakash: Even within the theoretical area there might be difficulties amongst different levels, to talk to, for example, numerical relativists, numerical simulations of black holes. That's one area where we had a lot of difficulty understanding their language and effort was put it. Stephen [Fairhurst] was one of the leaders in starting a group called NINJA which helped create a platform in exchanging ideas. Not just ideas! Also to set up a language, a common language between these two. It requires a lot of effort.

NINJA (Numerical INJection Analysis) is described in the group's wiki as follows:

> The goal of the NINJA project is *to bring the numerical relativity and data analysis communities together to pursue projects of common interest* in the areas of gravitational-wave detection, astrophysics and astronomy. (emphasis added)[8]

From Sathyaprakash's description, NINJA can be seen to be an example of an emerging Galison-type trading zone ("a common language"). Also, the group has decided to focus on specific topics that signal the appearance of boundary objects, in this case a 'topic' with which all groups in the collaboration can deal with directly, 'the merger phase of binary black hole (BBH) coalescence'.[9] Trading zones and interactional expertise mechanisms do not rule each other out and can work in parallel for the construction of specific projects or the attainment of particular goals, but they must be 'manufactured' for this purpose, and at least in the NINJA case do not seem like the 'natural' pathway to communication found in everyday physics practice.

[8] https://www.ninja-project.org/doku.php
[9] Aylott, B. et al (2009).

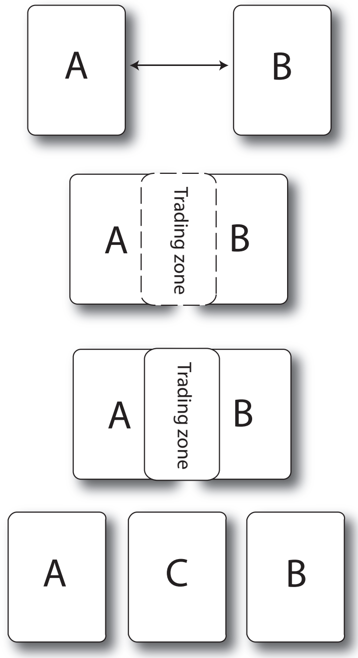


Figure 3.2: Trading zones that evolve over time beyond the 'creole' stage lead to an increase in complexity, which *complicates* the problem of communication.

3.18 A brief critique of trading zones and linguistic specialisation

I wish to present one more argument as to why the trading zone model is insufficient to explain collaborative work in science. The full dynamics of Galison-type trading zones develop in the following manner, schematised in Figure 3.2.

1. Two communities, A and B, are forced to interact.
2. In order to do so, A and B develop a pidgin, or basic hybrid language, let us call it (A&&B).
3. If interaction is prolonged, a more sophisticated creole hybrid language develops, let us call it A&B.
4. Given enough interaction, a new language— a full-blown language— develops, let us call it C.

5. The end result of prolonged interaction is then, given that the previous step is reached, that one begins with two languages A, B, and ends with the three languages A, B, C.

The end result of the prolonged-interaction trading zone is then to increase the disunity of practices, not to diminish it! Thus, unless one adds the supposition that trading zones never evolve beyond creoles, the trading zone metaphor does not solve the problem of communication, but in fact makes it even more problematic. In the end, one must still explain how after prolonged interaction, giving rise to a more complex situation, the three autonomous languages A, B, C interact. If one posits that this is again done through the establishment of trading zones, the end result is that given enough interaction, one is left with six total languages, and so on ad infinitum. SEE and trading zones are in thus at odds in the sense that prolonged interaction enables communication for the former, but hinders it in the case of the latter. Thus trading zones are not a solution to the general problem of communication, although they may of course be mechanisms that facilitate communication in very specific contexts such as the NINJA project.

3.19 Collins & Evans' model as an answer to the problem of communication

I began this chapter by showing that there are two kinds of physicists, the high-theoretical variety for whom experiment only indirectly enters their work and that have only very minimal overlaps with the social world of experiment, and phenomenologically-oriented theoreticians who either by choice or circumstance interact closely with experimental groups. I then analysed three different cases of physics collaborating with experimenters: person-to-person collaborations between individual theoreticians and experimental groups, small inter-group collaborations where the groups are still fairly autonomous but the efforts directed towards phenomenology, and 'Big Science' collaborations where all science is subsumed into a single underlying project. In all these instances, although the overarching form of the collaboration is significantly different, interactional expertise is seen to work at all levels of communication, and linguistic immersion into another native culture is seen to be the primary mechanism of learning to speak 'the other's' language, as Collins & Evans dictate should happen. In the following chapter I will deal with theoretical physics that interacts' with experiment via Collins & Evans lower categories of knowledge transfer, that is, through beer-mat, popular understanding, and primary source knowledge, where interactional expertise is not needed as in the above cases.

CHAPTER 4

Mid- and long-range interactions between micro-cultures

Y trust unto here godenys,
she wolde not mysdoo;
That y wyst ful wel, y-wys,
for ofte y have y-founde hit soo.

— Anonymous, from 'As I Lay Upon a Night', 15th century polyphonic carol

4.1 Knowledge exchange between non-interacting micro-cultures

The flow of knowledge from empirical to theoretical physics sometimes happens without there being direct interaction between the micro-cultures. This chapter will concentrate on the types of knowledge exchanged between lozenges in the horseshoe diagram that are not in immediate contact and where there is no natural development of interactional expertise due to a lack of shared social spaces. I will show that in these cases, the flow of information is based on the existence of trust between experimenters and theoreticians, and that the underlying mechanisms for this to happen can only be understood from a sociological perspective on the how scientific knowledge is sanctioned and supported. I will illustrate these aspects of theoretical knowledge-exchange by examining the role that trust plays in the way that mathematical and pure theory relate to experiment in physics. This will also serve to challenge a putative *sine qua non* of scientific practice: the Baconian precept of science being directly based on 'facts', these being presented as unproblematic objective knowledge.[1] I will show that rather than based on facts, theoretical practice is actually based on external accounts of facts, which evidences the necessity of sociological trust to bind physics' overall structure.

4.2 Trust and autonomy

In the previous chapter I illustrated how high-theoreticians are most of the time sociologically 'distant' from experimental production, and how this isolates them from the world of experiment (see section 3.1). Although this is a form of division of labour, with some physicists specialising in producing theory and others in doing experiment, there is also a partition of epistemic *power*. Experiment and theory become closed fields of expertise to 'outsiders', who are technically incompetent at proficiently producing this knowledge, and thus illegitimate actors to criticise it's production. In the words of Bourdieu (1975, p. 23),

> "in a highly autonomous scientific field, a particular producer cannot expect recognition of the value of his products ("reputation", "prestige", "authority", "competence", etc.) from anyone except other producers, who, being his competitors too, are those least inclined to grant recognition without discussion and scrutiny. This is true de facto: only scientists involved in the area have the means of symbolically appropriating his work and assessing its merits. And it is also true de jure: the scientist who appeals to an authority outside the field cannot fail to incur discredit."

Additionally, as esoteric expertises become closed circles of knowledge production, autonomy also implies *legitimacy* when facing the outside world. What is manufactured

[1]Harré (1970) refers to this as 'The Mythology of Deductivism' and identifies three Great Myths: The Myth of Events as Prime Objects of Knowledge, The Myth of the Vehicles of Thought and The Myth of Deductive Systems.

by autonomous micro-cultures is presented to the outside world as 'the' truth, and remains the most trustworthy account unless an outsider is willing to penetrate into the specialists' domain in *their terms*. This brings about a situation in which knowledge about the physical world, when it enters high-theoretical practice, does so indirectly through the accounts of experimenters whom the theoretician has no links to and may have never met personally. And so for theoreticians to maintain the idea of physics as an *empirical* science they must either relinquish authority to experimenters in matters of empirical 'truth', or migrate from theory and become experimental collaborators themselves as described in detail the previous chapter, through the acquisition of interactional expertise in the form-of-life of experimenters.

The converse of the above is equally true; theoretical expertise is an esoteric, autonomous field of specialisation in relation to experiment. When physicists decide to specialise in experiment, they are also relinquishing the possibility of becoming active, participating members of the theoretical community, which has its own practices of technical argumentation, indoctrination and legitimisation. Experiments in modern physics consume so much in the way of time and resources that those who create the experimental data often have to devote themselves so fully to this task that even the labour of interpreting the data may have to be left to others. The task of developing the superstructure that gives coherence to experimental results is taken away from the experimenters who created it. In opposition to the experimental handbooks that simply state 'accepted' experimental parameters without further ado, one can find handbooks of mathematical functions and formulas of mathematical physics that simply state 'the results' without any justification for them.

4.3 You need a bus load of faith to get by

Theoreticians often need to appeal to 'canonical' experimental results and tables in their work. Established experimental results such as those found on popular material-property handbooks are not upheld by one or two people, but by a large number of highly trained experts, teams and laboratories often working independently from each other. To question one of these experimental result would require either a divergent result of one's own (which a theoretician would be incapable of producing), or 'to go against the social grain' by questioning communally sustained knowledge. There are cases where this happens: scientific controversies. But controversies are not the mainstay of scientific activity. In most of their everyday work, theoreticians are content to use experimental results that have been validated by a community to which they have no direct access or which — as has been showed in previous chapters — it takes great effort to migrate into. High-theoreticians generally accept that this trust in experiments is a natural part of physics:

> Tong: At some point I have to take on faith what experimenters tell me. And so I know that there are important questions that need to be answered: the cosmological constant, dark matter, the spectrum of cosmic microwave background radiation or fluctuations you can see, problems in fractional quantum Hall effect or high temperature superconductivity. I've never done of those experiments, and I don't understand most of the experiments, but you know I have faith in these problems that need answering. Because you know if I go away and I start digging so I do understand the next layer of experiment. [...] I'd call it trust, but trust based on lots of evidence. And trust that I can test it at any time. [...] Certainly I don't understand the way LHC [CERN's Large Hadron Collider] works. But I could. I could sit down and spend three years of my life figuring this out.
>
> Reyes: Do you know anybody who has?
>
> Tong: Oh yes, I know theoreticians who understand it. I'm sure they didn't put a fuse together, but yeah, I know...the people who work at LHC full time. They could tell you what all the quirks were, what could be going wrong, what to worry about.

This last idea that theoretical physicists can 'in principle' always test experiment at any time is quite common. As another senior theoretician put it,

> De la Peña: Experiment is a fundamental guide. Physics is still a science with experimental foundation. Not necessarily experimental in the sense of the beginning of the twentieth century, like in the German tradition where if you did not make experiments you were useless; where experimental physics was the only kind that made any sense. Nowadays it is quite

> clear that you can do excellent theoretical physics without any knowledge of even how to tighten a screw (is it clockwise or counter-clockwise? I don't remember!) It can be done perfectly well. A capable theoretician can acquire profound and solid physical intuition while being completely detached from experiment.

He then added,

> De la Peña: Nevertheless one must be aware of how the [experimental] results were arrived at; one must have a clear idea of the limitations of the experiments so that one can appreciate to what degree what one is doing is really well-founded. Theoretical physicists don't usually know that, but that is part of the complications in the field.

This belief in the *possibility* of reproducing experimental results is part of the lore of theoretical physics. Despite this common appeal to the 'openness' of experimental results, in practice, the theoretician does *not* have access to the production of experiment unless he is willing to sacrifice his theoretical career for it. For a theoretician to appreciate the reach, limitation, origins and the other inner particularities of modern experimental work would in most cases likely prove practically impossible.[2] I will call this 'myth' virtual empiricism: the *trust* that theoreticians must rely on to take up experimental results, tied to the *belief* that a theoretician should have the capacity to fully understand the experiments that his work is anchored to— either directly, or 'through' a colleague to whom the theoreticians has access.[3] This myth asserts that the ability to understand all scientific knowledge is one of science's intrinsic characteristics, even when in practice it is through trust in 'the mechanisms of science' that this comes about.

According to the myth of virtual empiricism science is an open book ready for anyone willing enough to read it, but from a sociological perspective virtual empiricism implies trust not in the individuals that produce knowledge, but on the 'institutions' of science. Here 'institution' is to be understood in a wide sense, as given to it by Malinowski (1944, p. 47, 52) where he considers an institutional system as "the social

[2]As Collins (1974, 1985, 2004) has shown, the reach of experiments can be uncertain to experimenters themselves in their *own* area of expertise.

[3]I thank Professor Collins for his help in coining the term 'virtual empiricism'. However, as often happens with academic matters, the term has some antecedents which only became known to me in the later stages of the writing process; see Kitcher (1995).

scheme of organised life", or "an organised systems of purposeful activities." Giddens (1990) has carried out analyses of trust working as an institutional phenomenon and posits the emergence of this kind of trust as one of the most important characteristics of modern societies at large. Giddens (1990, 27-28) account of trust in expert systems offers a theoretical understanding of the mechanisms behind virtual empiricism, discussing the example how when one enters the upper levels of a house, one has 'faith' in what the architects and builders have done, even though "we know very little about the codes of knowledge used by the architect and the builder". There is faith in the individual's competence (how could I know after all, that either one is competent if I am not an architect or a builder myself), but there is also trust "in the authenticity of the expert knowledge which they apply — something which I cannot usually check exhaustively myself." Sztompka (1999, p. 13) similarly notes that "large segments of the contemporary social world have become opaque for their members. [...] More often than not we have to act in the dark, as if facing a huge black box, on the proper functioning of which our needs and interests increasingly depend. Trust becomes an indispensable strategy to deal with the opaqueness of our social environment. Without trust we would be unable to act." As with most sociological theorists, Sztompka explains the emergence of this opaqueness or 'black-boxing' as due to the increasing complexity of today's social world. Faced with this complexity, the individual is forced to resort to trust expert systems in order to operate in the wider social settings.

But virtual empiricism *also* carries along the mythos that trust is not a necessity but a practical choice, one that could be dispensed if the individual were given enough resources.[4] Nevertheless, the theoretical verification process of experimental results, the phenomenological domain, is sociologically 'black-boxed' by trust. The idea of trust at the collective scale has an important role in social studies of science. Shapin (1994) is the primary reference in science and technology studies, presenting a historical analysis of how trust in empirical science was established within the 17th century context of English science through the work of Robert Boyle and other members of the Royal Society and their 'gentlemen of science' inter-personal trust, which gave way to the establishment of institutionalised trust in empirical physics.

[4]If one analyses the description of the 'norms' that make up the scientific *ethos* as set out in Merton (1942), one can understand why the accessibility to the mechanisms of knowledge-production must be believed to be open to all: 'universalism' and communism', taken not as normative statements but as doxastic attitudes, underlie the mythos of the virtual empiricism.

Hence, because of autonomy, the task of deciding whether a particular experimental result merits recognition can only be settled within an experimental community itself and high-theoreticians must rely on trust— or perhaps more specifically on the suspension of doubt— on experimental results. This phenomenon is well known to social studies of science, with the existence of deep case studies of how 'core-sets' of scientific experts are constituted in experimental physics, the core-set being defined as the reduced group of experts that can legitimately contribute to an esoteric debate. Collins (1985) and Pinch (1986) have carried out detailed studies of experimental controversies, where the importance of core-sets becomes more visible than in normal science since these discussions tend to centre around delimiting who is a relevant expert and who is not in order to settle the controversy. Pinch (1986, p. 214) discusses how 'black-boxing' scientific instrumentation allows it to become widespread outside the context within which it is created, and how "black-boxed instruments are the carriers of social relations." Likewise, the black-boxing of experimental or theoretical results allows for them to become as much 'off-the-shelf' knowledge, as Geiger counters or oscilloscopes are 'off-the-shelf' technological devices. In this way, black-boxing allows experimental results or theoretical 'devices' to become 'tried and tested' knowledge. MacKenzie (1993, p. 372) has proposed a model of uncertainty referred to as the 'certainty trough', to model different attitudes towards 'established' technology, which can be extended to general discussions of certainty and trust in esoteric domains. According to MacKenzie there are three levels of certainty/ uncertainty depending on the 'distance' between the core-set of an esoteric domain and those outside the domain. Inside the core-set, 'uncertainty' is high because the core-set members are aware of the 'human' (one might say sociological) dimensions of the knowledge produced. On the borders of the core-set, the 'users' of the technology/ knowledge suspend doubt on the esoteric domain's products. This is in fact the domain of virtual empiricism and lower-level knowledge. Finally those wholly outside of the user/consumer domain establish a final level, one of highest uncertainty, as they have no sociological connections to the knowledge/technology, or they follow alternative sources of it.

4.4 Laboratories as producers of inscriptions: beer-mat knowedge

Laboratories are thus socially black-boxed spaces to high-theoreticians, churning out experimental results through mechanisms that are obscure to outsider theoreticians. One can think literally of a lab as box with concrete walls producing 'results' in printed form for the theoreticians to plug into their work. In their classic ethnographic study of laboratory science, Latour & Woolgar (1979) use the following metaphor for a molecular biology lab: a laboratory can be represented as a producer of 'literary inscriptions'. As one departs from the realm of experimental physics practice and tries to identify which parts of experimental work enter high-theoretical work, it turns out to be more than a useful metaphor: high-theoreticians using experimental knowledge are indeed often times using 'experimental results' in the form of numbers, data-tables— in short, inscriptions in the most literal sense.

Latour's metaphor of laboratories as inscription-producing devices can easily be restated in Collins & Evans' model through the horseshoe diagram. When micro-cultures exhibit large epistemic 'distance' between them — when there is no social bonding that permits interactional expertise to arise— the principal means of transmission of knowledge is through inscription-style beer mat knowledge. 'Numbers' and 'parameters' in tables and handbooks are simply bits of data which are completely detached from the context within which they are produced. If a physicist has the need to enter the numerical value for the speed of light in a calculation to produce a numerical result, or the rest mass of the top quark, or the charge of the electron, this can be easily solved by going to an appropriate table, without the physicist having any need to understand anything about the experiment that gives rise to the result. One can easily imagine a physicist-only pub quiz with questions like, "what is the value of the speed of sound in paraffin?" The answer, "1,940 m/s", would constitute empirical beer-mat knowledge.

Experimental inscriptions are used in all areas of physics. Experimental and theoretical physicists alike often use handbooks of material properties in their work when they are describing 'real' phenomena: these books are usually nothing but long lists of the measured numerical values of numerous experiments which have become of standard usage. Perhaps the best-known handbook is the massive *Review of Particle Physics (RPP)* published by the Particle Data Group, a team of physicists at the Lawrence Berkley National Laboratory. "The Review", as its authors proudly state, "has been called the bible of particle physics. Over the years, it has been cited in 30,000 papers." *The Review* is a 1,340 page volume containing the Particle Data Group's "determinations of the best values for the masses [and other parameters]" of all particles that have been experimentally detected.[5] Although nowhere near as massive as *The Review*, each field has its own similar authoritative data tables, experimental parameter references and review papers, so that the physicist can just look up the necessary parameters when doing computations. In its most elementary form, these kind of tables can be seen in any introductory physics textbook as appendices listing the more important 'measured constants' such as the speed of light, sound, refraction indices, etc. which enable students to solve the more 'realistic' problems.

Latour and Woolgar's metaphor works perfectly well at this level: long after the laboratory machines have been turned off, what will remain for others to use are the inscriptions produced therein. Although Latour and Woolgar's metaphor of labs as producers of inscriptions may have been mostly a methodological proposition, in the end one can see that in printouts such as the *RPP* or in material property reviews it is not such a far-fetched idea. For many theoreticians, experiment enters theory only through numbers; widely established and trusted numbers to be put into calculations as free parameters or input values. After all, part of the advantages of standardisation that projects such as the *RPP* offer to the non-experimentalist (or to an experimentalist who is not an expert in a particular field) is the possibility of suspension of doubt, of fully being able to trust that the tables of data available therein are given 'facts'.

Mathematics can also be used as beer-mat knowledge. Just as there are empirical data tables, there are for example encyclopaedias of 'special functions' where physicists can look up how to 'apply' a certain technique or how to write down the solution of a well-known equation. These books do not require that the physicist know how to derive the solution or the method, but simply state what the answer is. In pure mathematics, a theoretician can also point to a particular theorem that says that a certain physical equation has or lacks a solution, without needing to derive it, or understand where it

[5]Introduction by M. Barnett, head of the Particle Data Group: `http://pdg.lbl.gov/2010/html/what_is_pdg.html`

comes from. Likewise, software like *Mathematica* allows theoreticians to harness the power of black-boxed calculation software without the need of fully comprehending the mechanisms that give rise to computational results.

4.5 Understanding Latour's inscriptions within a general model of expertise

A particular problem arises with Latour and Woolgar's account — or at least at odds with the model of communication presented here — in that there seems to be a generalisation of inscriptions as *the* mechanism for transmission of knowledge in science. Latour and Woolgar downplay the fact that inscriptions, or beer-mat knowledge— is only *one* of a number of types of knowledge that can be exchanged between scientists, as they constantly make reference to how established knowledge is the point of contact between different 'lab cultures'; Latour & Woolgar (1979, p. 66) write that "the inscriptions devices, skills, and machines which are now current have often featured in the past literature of *another field*. [...] The apparatus and craft skills present in one field thus embody *the end results of debate or controversy in some other field* and make these results available within the walls of the laboratory (emphasis added)."

Contrary to this position, Collins & Evans recognise that the transmission of beer-mat knowledge is only part of the story. Moreover, the present work argues that underlying the establishment of 'facts' there is a sociological process of black-boxing through sociological *trust*, a mechanism which Latour and Woolgar never allude to, preferring the term 'reification'— which seems to carry with it a lot of rhetorical possibilities, but little sociological content. In fact, analysing theoretical physics up close shows that in collaborative settings theoreticians communicate through a collection of strategies which Latour and Woolgar's account does not give sufficient justice to. Whether this is due to Latour and Woolgar having centred their case study in the biological sciences and not in the physical ones may partly explain the difference, as perhaps the dominant form of knowledge transfer in biology is of beer-mat type. Nevertheless, it is highly unlikely that the transmission of knowledge in biology is exempt from the other types of interactions considered by Collins & Evans.

It is also possible that the tension between Latour's approach (both in the cited work and in later developments) and Collins' explanatory framework is due to deep methodological disagreements on how to study science, society and cultures at large. Although in Latour & Woolgar (1979, p. 39) the authors openly avow an anthropological method of "participant observation", the understanding of the term is very distant to the method of "participant comprehension" as understood by Collins (see Chapter 0 of this work). Latour & Woolgar (1979, Postcript, p. 278) for example insists that their methodology follows Schutz's prescription of the observer becoming 'the stranger' inside the place of observation, so that the authors choose to "portray the laboratory as seen through the eyes of a total newcomer. The notion of anthropological strangeness is used to depict the activities of the laboratory as those of a remote culture and to thus explore the way in which an ordered account of the laboratory life can be generated *without recourse to the explanatory concepts of the inhabitants themselves*". Thus while Collins maintains a version of sociology where the description of the scientific form-of-life begins by gaining an insider's understanding, Latour and Woolgar promote a view where the sociological description begins with an explicit non-understanding of an observed 'alien tribe' whose form-of-life the authors are openly *wanting* to destabilise. In other methodological issues there is much in common between these authors, so that in fact this apparently innocuous — but in reality deep — methodological difference may have become lost among other debates. [6]

4.6 Popular understanding of experiments

One would expect that theoreticians closer to experimenters develop 'higher' forms of knowledge, which includes rational reconstructions of experiments and a mild comprehension of the experimental process that is absent from beer-mat knowledge. 'Popular understanding' can be understood as a form of rational reconstructions. A central purpose of scientific popular understanding literature is for a wider audience to 'under-

[6] Collins' approach may seem to have a foot in each side of the sociology/anthropology divide, thus contrasting with Latour and Woolgars quasi-positivist project — at least as stated in *Laboratory Life*. From a similar theoretical position, Bourdieu — himself deeply influenced by Wittgenstein and considerations of tacit knowledge — has similarly noted how underlying a lot of modern sociology there is a "mistaken" theoretical presupposition that the researcher can indeed treat social phenomena as external to his own theoretical schemata; see Lamaison (1986). For a general analysis of the influence of the tacit knowledge concept and Wittgenstein on Bourdieu's oeuvre, see Gerrans (2005).

stand' a piece of science better. Popular science books aim not just to convey facts, but to present a sketch of how facts are produced, the 'official' history behind their production, and the rational reconstruction behind a scientific fact. Mellor (2003, p. 512) points out the role of popular science 'expository books' which are "structured around the exposition of a particular scientific discipline. This may include a narrative about the history of the discipline, but the emphasis is on a particular subject or theme and its logical consistency or empirical basis, rather than on a particular story."

An example of 'popular' understanding of experiment by theoreticians is knowledge about the Lorentz force, an important phenomenon used in many physics experiments whereby a charged particle moving in a magnetic field experiences a force perpendicular to both the field and the particle motion. Studying it is part of any undergraduate electromagnetism course. It is used, for example, in cyclotron accelerators that make charged particles move faster and faster using a combination of two electrodes and a magnetic field. The Lorentz force 'bends' the particle's trajectory, and keeps the accelerated particles inside the machine. The equations to describe the motion can be easily understood by anyone with a 'minimal' physics background. Any physicist can understand the general mechanism that makes a cyclotron work, which is in essence the Lorentz force, but of course this does not imply that any physicist can actually go and build one or operate one without spending a non-trivial amount of time attempting to do it. Any theoretician knows similar 'basic principles' behind plenty of modern experiments, and in many cases would be able to understand a general description of the ones that are unfamiliar in a 'popular' talk by an experimenter.

'Popular understanding' of mathematics can come about, for example, by browsing through the proof of a theorem, even if the full fundamental proof is not worked out step by step. A superficial read over a proof, or the 'bare bones' of a proof as is offered in many theoretical textbooks gives some insight into the mathematical nature of a theorem or method, even if mathematical-level rigour is not provided. An important means to acquire knowledge beyond the beer-mat level, one that is found in all physics institutions, is the organisation of seminars and colloquia:

> Romero: One thing that I think is crucial to both theoreticians and experimentalists, is going to seminars. Seminars open you up. It's a funny thing, when you go to a seminar for the first time because you understand

> nothing. Absolutely nothing! And as years go by you look back and you say, "hey, this is the same thing which that guy talked about a long time ago." For example, I was never an expert, but I grew to know quite well, on the surface, the topic of superconductivity at high temperatures because it was discovered at the time when I was a PhD student, in 1986. I was doing something else, but there were so many seminars about it and I went to so many seminars about it; and I knew about superconductors type one, two and three; or what Anderson had said about it; or about RGVB, the Resonating Valence Bond Approximation; about tight binding. And only from going to seminars, without sitting down to read even a single paper! Of course, that does not make you an expert, nor does it enable you to work on that topic. It's baggage that you acquire.

Seminar talks also make reference to the classic or foundational publications in the field, as well as current developments, and usually end with an 'open problems in the field' slide to point out what the community defines as interesting (legitimate!) research lines. A time for questions is usually allotted at the end of the talk, when the audience has a brief chance to interact with the speaker (one may also use this time to massage the speaker's ego, or to break into an open argument with the speaker; Q&A time can sometimes be the most exciting part of the seminar to witness a good sparring round between colleagues).

Seminars function as popular introduction to esoteric topics, but they are also grounds for meeting and interacting with members of other groups, thereby also imparting small amounts of tacit knowledge to attendants. Thus, although he never became 'an expert', in being able to 'understand' a lot of talk about superconductors after having attended many seminars on the topic Romero is exhibiting medium-range levels of interactional expertise in the field.

4.7 Primary source knowledge

Given sufficient familiarity with an experimental topic, theoreticians may take up and consult experimental publications. Yet most experimental publications only approximately reproduce what an experiment was really like when being performed. Reading an experimental paper probably gives insight into some of the technical aspects of an experiment complimentary to that a seminar would, but the insight is limited to a polished account of the main points that the author wants to highlight. Just as a published theoretical paper is a refined exposition of the results of a lot of work carried out on paper, blackboard and computers, experimental publications are refined versions of months or even years spent at the workbench or lab. Again, this is due to the fact that in order to understand a piece of physics— theoretical or experimental— surpasses what can be transmitted in terms of explicit knowledge, and in a short session. Otherwise, all that theoreticians would need to do to 'connect' with experimenters or 'understand' their work would be to read their publications, or attend their talks. Nevertheless, I have shown that high-theoreticians tend to not to use these publications directly, even in cases like Romero's where the interaction has been prolonged. It is still however possible that theoreticians can consult experimental primary sources directly after long exposure to phenomenological work.

4.8 Asymmetries between theory and experiment

Although experiment and theory share epistemic power, it should also be pointed out that the trust necessary to support a theoretical result and an experimental result to the outside world is asymmetrically distributed. The amount of trust that a physicist is willing to put in another physicist from a different domain is important when one considers the asymmetry between the time and effort it takes to develop and carry out experimental and theoretical research programmes. Whereas serious theoretical work is relatively easy to 'set up' for— as many of the interviewees noted— a lot of theoretical work involves simply playing around with ideas, concepts and equations, experimental work generally requires much more temporal and material investment in definitive research directions. Theoretical research programs because of their non-materiality are much more flexible than experiment. One often finds that theoreticians involve themselves in several research lines at the same time, even when these are usually connected by some underlying themes,

> Tong: It's impossible for experimenters to be that 'flighty' [as us theoreticians] just because for a particle physics experiment, the run-up time

> is about twenty-five years but even for a condensed matter experiment it takes a couple of years to get things up-and-running. You can't just switch and change that easily. So people working in those groups are just tied to doing that. Probably if you get more towards the mathematical side…it's easy for me. I just throw a bunch of papers on my desk, print out a bunch more and just sit there with pen and paper and just start playing. So that's why I suspect the more flighty theoretical physicists are on the more theoretical side. […]

…in contrast with the material 'investment' that experimentation often requires,

> Volke: Sometimes problems come up which lead to delays. Here [in the lab] sometimes the time between the request for a piece of equipment and its delivery can be a couple of months or up to a year. You have to adapt to those changes and always have a plan B. In experiment, you always have to have a plan B. You have to think about it, or you will have to start thinking about it when an unpredicted event appears. [As an experimenter] you begin to work slightly different. On one hand there's teamwork, and having to rely on other people. On the other hand, adapting yourself…it can happen when you're working in theoretical problem that you have a clear idea of where you want to go, that you begin down a road and it wasn't the correct one and you have to take another one. You also need a plan B there. But as far as methodology goes you probably find fewer surprises.

This has strong implications concerning the amount of trust that is invested in theoretical and experimental results. If an experimenter is aware that a research program will very likely cover his entire professional lifetime, this translates into a very serious investment of personal resources, and of the necessity for a lot more stability of trust relationships. One would not be likely to spend twenty years setting up an experiment to discover or probe a certain physical phenomena that is highly unlikely to exist.

Pinch (1986) carried out a study on solar neutrino detection that includes an analysis of the interaction between Davis' experimental project and Bahcall's theoretical (phenomenological) analysis of Davis' 'anomalous' results. Pinch describes how Davis' "openness and his willingness to consider all criticisms [were] mentioned by most respondents as one of the main reasons why they believe this experiment is good", despite the fact that the results were at some points heavily criticised and 'in contradiction' with Bahcall's predictions. A decrease in the community's belief and trust in Davis' results would have entailed the failure of a project with high investments— in terms of time, money and reputation— in which several high-profile physicist (experimental and theoretical) had stakes.

In contrast, Bahcall was allowed significantly more flexibility. Bahcall's initial predictions were in conflict with Davis' results, and for a while he was severly disheartened by the mismatch between his predictions and Davis' experiments. Nevertheless, after the initial disappointment Bahcall's position evolved from an effort to find mechanisms so as to make the mismatch less significant, to a later position where the mismatch was fully embraced as a pointer to the existence of 'new physics'. Although Bahcalls 'flexibility' was attacked by a handful of theoreticians, the community at large did not shun this strategy.

Although both Davis and Bahcall relied on their good reputation and standing within their communities, the need to establish the firmness and stability of the results was important only in the experimental context, which Davis did very successfully by the close links he had established with the astrophysical community with which he was in constant dialogue, but without ever challenging their authority in their own domain. Davis subjected his experiment to a number of 'calibration' tests suggested by the astrophysics community which although "largely a waste of time in terms of his immediate experimental goals", Pinch (1986, p. 174) describes as having served "an important ritual function in satisfying the nuclear astrophysicists, and thereby boosting the credibility of his experiment."

4.9 Trust as the substratum of long distance interactions between theoretical and empirical physics

I have argued in this chapter that for physics that is epistemically 'far' from experiment, the main sociological mechanism for knowledge exchange is the establishment of trust, the idea having been complemented by Collins & Evan's classification of the 'depth' which this knowledge can achieve. I have also argued that at its most extreme distance, knowledge exchanges happen in the form of 'inscriptions' which Latour describes. However, these sociological mechanisms are hidden behind the veil of virtual empiricism so that in most cases physicists are only partially aware that the empirical content of their theories are in most cases very indirectly sourced. I have concentrated on the way that theoreticians take up experimental work in this chapter, but an analysis of how theory interacts with its other pole of influence, mathematics, will be carried out in the next chapter where trust-based mechanisms will also be shown to be at play to bridge the epistemic divide between theory and 'pure' mathematics.

CHAPTER 5

Trust and proofs in mathematics and high-theory

Faith,
Faith is an island in the setting sun,
But proof, yes,
Proof is the bottom line for everyone.

— P. Simon, from 'Proof'

Proof — n. Evidence having a shade more of plausibility than of unlikelihood. The testimony of two credible witnesses as opposed to that of only one.

— A. Bierce, 'The Devil's Dictionary'

5.1 Trust and mathematical proofs

The previous chapter has shown that when theoreticians that are epistemically far from experiment use experimental knowledge, they do so as inscriptions and rational reconstructions that are manufactured by laboratories, which to theoreticians are cultural, social and linguistic 'black-boxes'. Nevertheless, it was also argued that pure theoreticians occasionally migrate to physics that is closer to experiment, and this process has been described as a long and arduous task that requires prolonged exposure and dialogue with laboratory scientists. The greatest barrier to overcome in understanding a foreign culture is the transfer of tacit knowledge. This chapter will set out a definition and typology of tacit knowledge that will then prove useful in giving clarity to the problem of knowledge transfer when theoreticians choose to reach out to another 'micro-culture' or expertise domain directly — pure mathematics — which is as autonomous from theoretical physics as is experiment, and to analyse how specific types of tacit knowledge impinge on mathematics and physics.

Epistemic autonomy is intimately connected to the definition of expertises and expert languages in esoteric domains, and to the tacit knowledge components that make up specialist expertise. To master the language of an esoteric expertise means to master the tacit knowledge of the domain, not just the explicit dimension. The importance of tacit knowledge in mathematical proofs and theorems is particularly interesting, because it is often claimed that mathematical derivations are one of the few instances of science where one is likely to find a minimal levels of tacit knowledge. I will show that although certain parts of mathematical culture do indeed aim to minimise certain types of tacit knowledge, this cannot be generalised to all practices of professional mathematicians (and even less to that of theoretical physicists), and that tacit knowledge is inextricable from even the most formalist mathematics.

5.2 Relational tacit knowledge in theoretical physics

Mathematics is said to be 'the language' of physics, and it is a primary form of representation of theoretical work. There are many advantages to presenting theoretical results in a mathematical form: concreteness, exactness, etc. Being a 'universal' language, mathematical equations allow a result to be presented in a way that is more accessible to more individuals than resorting to ordinary languages. Or so the story goes. In fact, reading a mathematical or theoretical paper is not straightforward at all, even for an expert with an excellent mathematical background. In fact, mathematics can sometimes obscure a paper. The reason for this is that even the most highly mathematical papers make use of tacit knowledge.

Most physicists skim trough the majority of theoretical or mathematically oriented papers they come across, and only devote their time to understanding and analysing the steps of only a few select readings of particular importance. Step-by-step analysis of the deductive steps in a theoretical paper is far from simple and can become a painstaking process, even when it is taken for granted that the deduction follows the most primitive rules of mathematical logic:

> Mitchison: I certainly read extremely carefully about one paper a month, I would say, to the extent that I actually go through everything in it; tracking down references, working out the calculations myself and finding them not quite right and so on. So I think there is some body of things that people really read extremely carefully when it is close to what they're doing and it's sort of exciting in some way. [...] I presume that's the absolutely standard pattern of physicists.

Theoreticians often point out that when theoretical papers are written without clearly stating many of the intermediate steps in the deductive chains of reasoning it is out of practical considerations. However, practicality can, paradoxically, lead to major obfuscations in the original argument:

> Mitchison: I think that papers in mathematics for instance are constructed out of lies almost. I think people do a terrible thing which is that they hide the origin of ideas. So it happens again and again... you're reading a colleague's paper and you really can't get anywhere, and then you go and you say, "look, I'm having trouble with this. Can you explain it?" and he'll say "no, you should really think of it like this: that's a Fourier coefficient or something" and then in two seconds you've got it! And the way he's written the paper to conform to this idiotic notion of a reasoned argument has hidden it. And people actually strive to hide the truth through papers.

What we see in these observations is that mathematical physics papers 'assume' vast amounts of relational tacit knowledge. When the author is one's colleague sitting in an office down the corridor, the relational tacit knowledge can quickly be made explicit. Usually, this is not the case, and one is left to try to work out the missing information, a task that can eventually consume a very long time. Sometimes, seminars or study groups may be organised exclusively for a research group to probe particular papers and work out the missing information. A typical exercise that is often left for aspiring theoretical researchers in the earliest stages of their careers by their tutors is to take home a particularly relevant or paper in the field of expertise at hand, and to try to 'work out' the argument step by step:

> Mondragón: The first thing [to do with a new student] is to get a general overall picture and acquire certain basic knowledge so that we can communicate. I set them to work on more particular things before they actually get to know everything in the field, because if you wait for them to get the large picture then by the time they do everything has already moved forward and you will never be independent. You obviously have to read and solve problems from textbooks, but the best way is to have to face a problem on your own, even if it is a small one, and to work on it yourself. After basic knowledge is acquired, I make them solve something that they have never done in their life by themselves, like to reproduce a calculation: a research level calculation in a published article. I tell them to prove to me whether a certain publication is right or wrong. Or if I have a new but not extremely difficult problem I ask them to find the solution. I think that there is something like a phase transition, from the moment that they are passively solving textbook problems as student to the moment when they have to think and use their own research tools and to look in books and use the knowledge at hand. Or to try to figure out what it is they are missing to solve a problem, say, not knowing group theory; then they have to learn group theory. Up to now, it has been a good strategy. I don't give them finished problems where they only have to do a small calculation and run a computer program. One student once asked me, "Why do this problem if you already know how to handle it?" Well, I told him that I had a program that could do it, and it could only be a matter of him pushing a button, but he would learn nothing. I told him after he had solved it we could check with my program if what he had done was correct. But he had to do it from scratch, with whatever tools he had at hand; Mathematica, FORTRAN, whatever he could do it with. A lot of people panic at that point, but in general there is a positive change of attitude, at the beginning because it is their first time in a situation like this.

The technique of making the student face a real problem has two functions: to force the student to hone skills in relational tacit identification and elucidation, and to introduce the student to the particular relational tacit knowledge of the specialty field in which the student has chosen to do research. The student is in fact being taught two things: to correctly make the explicit-to-tacit and tacit-to-explicit translations without supervision, and for this to be recognised as 'the way the field works'. Students who manage to become proficient experts and eventually publish in the field will 'naturally' recognise the relational tacit knowledge that need not be stated for other insiders. It will be eliminated and black-boxed, just as a master chef hides the basic methodology in a written recipe. The student through immersion picks up which is the knowledge that can *legitimately* be kept tacit. The more relational tacit knowledge available to a practitioner, the better performance one should expect. This phenomenon of tacit 'introjection' is eloquently described by Field medallist mathematician Thurston (1994, p. 167):

> Mathematics in some sense has a common language: a language of symbols, technical definitions, computations, and logic. This language efficiently conveys some, but not all, modes of mathematical thinking. Mathematicians learn to translate certain things almost unconsciously from one mental mode to the other, so that some statements quickly become clear. Different mathematicians study papers in different ways, but when I read a mathematical paper in a field in which I'm conversant, I concentrate on *the thoughts that are between the lines.* I might look over several paragraphs or strings of equations and think to myself "Oh yeah, they're putting in enough rigmarole to carry such-and-such idea." When the idea is clear, the formal setup is usually unnecessary and redundant—I often feel that I could write it out myself more easily than figuring out what the authors actually wrote. It's like a new toaster that comes with a 16-page manual. If you already understand toasters and if the toaster looks like previous toasters you've encountered, you might just plug it in and see if it works, rather than first reading all the details in the manual.
>
> People familiar with ways of doing things in a subfield recognise various patterns of statements or formulas as idioms or circumlocution for certain concepts or mental images. But to people not already familiar with what's

> going on the same patterns are not very illuminating; they are often even misleading. *The language is not alive except to those who use it.* (emphasis added)

The difficulty inherent to successfully learning a new technique or of solving open problem has been referred to as 'hardness' in studies of veterinary surgical procedures in previous STS work by Pinch et al. (1996, 1997). Pinch et al propose that the skill learning process is optimally carried out using the 'enculturation' model, where the process of learning is carried out during the performance of the relevant actions where the skills are involved. This is juxtaposed to an 'algorithmic' model, where the skill transfer is carried out through the acquisition of declarative knowledge. Pinch et al argue that the enculturation model is preferred over the algorithmic one because skilled practice comprises mostly the application of tacit and not procedural knowledge. Although the importance of tacit knowledge is stressed (for example, by introducing a surgical version of the experimenter's regress for surgical procedures), there is an incomplete differentiation between the three types of tacit knowledge involved in hardness; this work being previous to Collins (2010), somatic tacit knowledge— as an individual skill— and relational tacit knowledge— as unstated but expressible knowledge— are mentioned, but not clearly differentiated as two distinct classes of knowledge.

Pinch et al suggest that in studying skill transfer mechanisms, hardness itself is an estimable quantity (estimable by a proficient expert), and could be used as an indicator for a new skill-learner to identify the success or failure of skill-learning or replication processes. In this sense, hardness is a 'second-level' explicit indicator about the acquisition of tacit knowledge. An example is a laboratory attempting to replicate the experiment of an independent lab working solely from the published literature. A low indicator of hardness would mean a technique expected to be replicable in a relatively short time, while high levels of hardness would signify that an experiment can be expected to take a long time to replicate. A hardness indicator would not make the acquisition easier, but could be a guide for the replicating lab to know if they are more or less working in the right direction.

In theoretical physics, a sense of the hardness of a particular problem is a handy skill in trying to recognise whether a particular pathway or an initial solution will lead somewhere significant or not. Berry in fact uses hardness to assess how to approach the tutoring of new graduate students

> Berry: You quickly realise whether somebody is somebody who will have a broad view and find problems within the area that I proposed or am working on, which is why they come to me in the first place, or whether it's somebody who is very focused and needs to have their hand held and to be told 'Now do this. Now do this.' There are different kinds, different sorts. So I give them some little problem, where I know the answer or I know there will be an answer which I deliberately don't work out. I want to see if they can do it.

As Berry explained, this is not an easily defined skill, but more like a 'feeling' that one gets after much experience, and which he himself uses to gauge whether his solution to particular problems are headed in a good direction.

> Reyes: When you're working on a problem... I suppose it's a hard question to answer... from when you first see the problem to when you're actually thinking you're on the right path, is there a point where you realise 'Yes! I think I'm on the right path!'?
>
> Berry: Yes, there is. That point comes at different times for different problems. Some of them I don't understand right away, I put them away, come back years later. Sometimes things come very quickly, and really, they come in a day sometimes; sometimes years. And here is a concept which I think is useful. I should write it, I mean to do so, which is the following. It concerns exactly this realisation. What is the elementary particle of sudden understanding, the 'clariton'? This concept is very useful. Everybody knows immediately what it means. It's a sudden, 'Ah! I know!' Unfortunately, there are also anti-claritons that come the next day and annihilate the one you had yesterday. So one's intellectual life is a succession of clariton/anti-clariton events. One hopes there's an odd number, so in the end you have at least a clariton. Oh, there are little ones, claritinos, just little tiny ones that give you pleasure when you write a technical paper. Nobody will ever be as excited as you, but it gives you great pleasure, just like a carpenter who made a cupboard and he knows that somewhere

> in the cupboard is a drawer with absolutely perfect joints. Nobody except him knows, but it smoothly works, the cupboard drawer; the private pleasure. Then there's the bigger pleasures, that everyone understands. So there are different magnitudes of claritons. Then there are ambi-claritons; you're not quite sure whether something sudden that you realise is good or bad news. And that happens too.

5.3 The role of rigour in pure mathematics

If mathematical and theoretical papers are loaded with tacit knowledge, then what is their advantage? In order to understand this, one must understand the role of 'rigour' in modern mathematics, or the idea that if a mathematical result is proven by mathematicians, then it is as close to 'hardcore clear truth' as anyone can get. Modern mathematical practice is constituted around the idea that one should be able to logically support all mathematical statements, theorems and results. In order to achieve this, mathematicians do not only posit mathematical statements, but must accompany them with formal proofs. Proofs— logical proofs, many if not most of them based on set theoretic constructions developed in the past two centuries— are an important part of modern mathematics since the early 19th century.[1] In fact, proving that a certain mathematical relation holds true is held by many to be what mathematical practice is mostly about nowadays. Mathematical proofs have an air of transcendent truth that permeates the popular view of how truth, proof, universality and mathematics are all interlinked. A typical example is encapsulated in the following quote from a popular science webpage

> In mathematics you can't just say that something is true; you have to prove it. Mathematical proofs have to be rigorous. This means that they have to hold true regardless of what test you may apply to them. If they don't, they aren't proofs at all.[2]

[1] Euler was famous for having discovered many deep theorems in pure mathematics which were provided with either no proof, or with proofs that would not pass modern mathematical standards. See Polya (1978) for a discussion of Euler's theorem on polyhedra, a case that was also analysed in depth by Lakatos (1976).

[2] "Basic methods of mathematical proof", BBC's h2g2 webpage, available at: `http://www.bbc.co.uk/dna/h2g2/A387470`.

However, things are not as quite as simple as that. The importance of proofs in mathematics changes across time and cultures, and even for contemporary mathematicians there is a wide spectrum of opinion as to the importance of proofs in practical mathematics, the reliability of published proofs, and even of whether proofs are the most important part of mathematical truth or not.[3] Because in theoretical physics mathematics plays such an important role, it is useful to illustrate the role of proofs in mathematics and to then contrast it to that in physics to understand how mathematics enters theoretical practice. This discussion will also serve to illustrate how mathematical proofs, the epitome of explicit truth if there ever was one, cannot escape from the influence of the tacit any more than any other human construction can.

In the early 1990s an interesting discussion was carried out in the mathematics community concerning the usage of 'rigorous proofs' in mathematical papers in which many of the most important living mathematicians were involved. The discussion started after the appearance of a paper by mathematicians Jaffe & Quinn (1993) that proposed the segregation, in practice and in formal teaching, of mathematics into two areas: "theoretical mathematics" and "formal mathematics". Jaffe and Quinn (J&Q hereafter) began their article by writing

> Modern mathematics is nearly characterised by the use of rigorous proofs. This practice, the result of literally thousands of years of refinement, has brought to mathematics a clarity and reliability unmatched by any other science. But it also makes mathematics slow and difficult; it is arguably the most disciplined of human intellectual activities.
>
> Groups and individuals within the mathematics community have from time to time tried being less compulsive about details of arguments. The results have been mixed, and they have occasionally been disastrous. Yet today in certain areas there is again a trend toward basing mathematics on intuitive reasoning without proof.[4]

J&Q went on to describe what they see are the two stages of mathematical production, the 'theoretical' phase in which "intuitive insights are developed, conjectures are

[3]See Kleiner (1991) for a concise historical review of changing perspectives on proofs within mathematics.

[4]Jaffe & Quinn (1993, p. 1)

made, and speculative outlines of justifications are suggested", and the 'rigorous phase', in which "the conjectures and speculations are corrected; they are made reliable by proving them." The choice of name for the 'theoretical' phase was chosen to show an innovative insight into the practice of both mathematics and physics, for J&Q note that[5]

> The initial stages of mathematical discovery— namely, the intuitive and conjectural work, like theoretical work in the sciences— involves speculations on the nature of reality beyond established knowledge. Thus we borrow our name "theoretical" from this use in physics.
>
> Theoretical work requires correction, refinement, and validation through experiment or proof. Thus we claim that the role of rigorous proof in mathematics is functionally analogous to the role of experiment in the natural sciences. This thesis may be unfamiliar but after reflection should be clear at least to mathematicians. Proofs serve two main purposes. First, *proofs provide a way to ensure the reliability of mathematical claims, just as laboratory verification provides a check in other sciences*. Second, the act of finding a proof often yields, as a by-product, new insights and unexpected new data, just as does work in the laboratory. (emphasis added)

J&Q also noted that

> Mathematicians may have even better experimental access to mathematical reality than the laboratory sciences have to physical reality. This is the point of modelling: a physical phenomenon is approximated by a mathematical model; then the model is studied precisely because it is more accessible. This accessibility also has had consequences for mathematics on a social level. Mathematics is much more finely subdivided into subdisciplines than physics, because the methods have permitted a deeper penetration into the subject matter.

The rest of the paper argues two points. The first is that mathematics' relationship with theoretical physics has generally been synergetic for both disciplines, particularly

[5] Jaffe & Quinn (1993, p. 2)

in the way that theoretical physics has invited progress in 'theoretical' mathematics, mentioning field theory and E. Witten's superstrings as important examples. But secondly, J&Q note that "these physicists are still working in the speculative and intuitive mode of theoretical physics. Many have neither training for nor interest in rigor. They are doing theoretical mathematics." This is ironic, for in the physics community string theorists are often accused of being too mathematical and far too rigorous. J&Q's paper goes on to work out the distinction between the speculative and hypothetical practice of 'theory' and the confirmatory validation process supplied by 'rigour'. J&Q note that even amongst mathematicians, there are those who are more theory-oriented, sometimes exasperating to a great degree many of their fellow 'rigourists'.[6]

J&Q finally warned mathematicians about how the level of rigour in even the most mathematically-oriented theoretical physics paper is rarely comparable in its rigour to that found in any typical mathematics paper. Because of the rise of the 'rigour' school in mathematics during the past two centuries, no 'theoretical' proposal, no matter how brilliant, can be considered finalised or complete unless it passes rigorous filters. J&Q ended their article by severely warning the mathematical community against bringing up new generations of mathematicians solely in the theoretical school, for "most students who try to dive directly into the heady world of theory without such a [disciplined and rigorous] background are unsuccessful. Failure to distinguish between the two types of activity can lead students to try to emulate the more glamorous and less disciplined aspects and to end up unable to do more than manipulate jargon."[7]

5.4 Responses to Jaffe and Quinn

Thurston (1994, p. 164) replied to J&Q's paper arguing that the importance of rigour in mathematics is real but overstated by J&Q, and is only one of many resources in the development of mathematical though and technique:

> Intuition, association, metaphor. People have amazing facilities for sensing something without knowing where it comes from (intuition); for sensing that some phenomenon or situation or object is like something else

[6]Jaffe & Quinn (1993, p. 4)
[7]Jaffe & Quinn (1993, p. 9)

> (association); and for building and testing connections and comparisons, holding two things in mind at the same time (metaphor). These facilities are quite important for mathematics. Personally, I put a lot of effort into "listening" to my intuitions and associations, and building them into metaphors and connections. This involves a kind of simultaneous quieting and focusing of my mind. *Words, logic, and detailed pictures rattling around can inhibit intuitions and associations.* (emphasis added)[8]

Thurston's account opposes that of J&Q's by defending an intuitionist perception of mathematical practice. J&Q were worried about the possibility of allowing 'theoretical' mathematics too much leeway. The opening line of the essay abstract asks, "Is speculative mathematics dangerous? Thurston claimed that J&Q's account of mathematical practice had more in common with a definition-theorem-proof caricature of mathematics than with actual mathematical practice, a point also highlighted by Berry

> Berry: You will find mathematicians who say that unless you write Theorem-Proof-Corollary-Lemma you are not doing mathematics. Well, then to annoy them I quote René Thom, a very great mathematician, Fields medallist, 'Any imbecile can prove theorems.' I don't believe it, but you know, quoting him, one of the most creative mathematicians of the 20th century. So he said mathematics is not about proving theorems; it's about understanding. Feynman said, 'A great deal more is known than has been proved.' That's deliberately provocative because they have a narrow, particular view of knowledge. It's not where I choose to be.

J&Q's paper created quite a stir in the mathematics community, prompting replies from many mathematicians. Atiyah presented a response that generally agreed with J&Q's account, but criticised it for presenting "a sanitised view of mathematics which condemns the subject to an arthritic old age."[9] Atiyah argues that there are many instances of mathematicians that have empowered great advances in modern mathematics, mainly the oft-quoted cases of Witten, Euler and Ramanujan; he also mentions that Jaffe's version could be influenced too much by his work theoretical physics background, "Jaffe represents the school of mathematical physicists who view their role as

[8] Thurston (1994, p. 65)
[9] Atiyah et al. (1994, p. 178)

providing rigorous proofs for the doubtful practices of physicists. This is a commendable objective with a distinguished history. However, it rarely excites physicists who are exploring the front line of their subject. What mathematicians can rigorously prove is rarely a hot topic in physics." Borel also objected to J&Q's essay, which he saw as an unnecessary exercise of "pundits who issue prescriptions or guidelines for presumably less enlightened mortals" noting that Weil had readily advocated for the utility of intuitive mathematics without much criticism; Borel was confident that the proofs obviously required by mathematics always come by their own accord, for mathematics is self-correcting in that respect.[10] On the other hand, Chaitan wholeheartedly embraced J&Q's proposal.

Mandelbrot replied that "the main reason why I find the JQ prescription appalling is because it would bring havoc into living branches of science. Philip Anderson describes mathematical rigor as 'irrelevant and impossible'. I would soften the blow by calling it 'besides the point and usually distracting, even where possible.' "[11] Mandelbrot also discussed the case of Paul Lévy and Henri Poincaré, both of whom were known in the stuffy and ultra-rigorous French mathematics community of the 19th century as being 'incurable' in terms of their shunning of formal proofs. He mentions how Hermite and Picard "shunned Poincaré, prevented him from teaching mathematics, and made him teach mathematical physics, then astronomy." Although theoretical physics was probably the big winner in Poincaré's relegation to mathematical physics, Mandelbrot makes the important point that a too rigorous approach to mathematics can have the same kind of destructive effect that Jaffe warns intuitionism can cause. It also highlights how demands of rigour can change across time and space sometimes very quickly; Mandelbrot complains how the Bourbaki seminar, initially started as a private joke with a disregard for orthodox mathematical practices, soon turned into a rigid promoter of a rigorist credo. Thus, the degree of rigour that the mathematical community demands from its members is far from being a simple and immutable criterion, and is a clear sign of how sociological factors can actively affect the production of mathematical knowledge. As noted in the response by Thom, "Since the collapse of Hilbert's program and the advent of Godel's theorem, we know that rigor can be no more than a local and sociological criterion. It is true that such practical criteria may frequently be 'ordered'

[10] Atiyah et al. (1994, p. 180)
[11] Atiyah et al. (1994, p. 194)

according to abstract logical requirements, but it is by no means certain that these sociological contexts can be completely ordered, even asymptotically."[12]

Of the many responses to J&Q's provocative essay, the response is mixed enough to suppose that, as Mac Lane summarised, "the ways of doing mathematics can vary sharply, as in this case between the fields of algebra and geometry, while at the end there was full agreement on the final goal: theorems with proofs. Thus, differently oriented mathematicians have sharply different ways of thought, but also common standards as to the result", agreeing with both Thurston and J&Q in that "throughout mathematics, inspiration, insight, and the hard work of completing proofs are all necessary."[13] It seems that while mathematicians recognise the important contribution of non-rigorous and intuition-led mathematics, there is also a unanimous acknowledgement of the importance of rigorous proofs, with variations from person to person on which is the more important. However, for the purposes of this work the most interesting conclusion is that there appears to be a generalised agreement amongst mathematicians that intuition plays a primordial and vital role in the development of all types of mathematics, and that both in considerations regarding the discipline as a whole and in their personal work, intuition is a powerful mover of ideas and results.

5.5 When mathematicians do not trust

Laughlin (1997, p.22) notes that rejections of intuition such as J&Q's can be understood if we take into account "the historical association of intuition with metaphysics and religious knowledge," both of which "appealed to private, esoteric, and ineffable knowledge that, however productive of personal wisdom, was seen by scientists as inaccessible to public scrutiny." This was a result, according to Laughlin, mainly of the positivist movement and its absolute rejection of metaphysics, for "somewhere in the project of formulating the positivist project the intuitional baby was thrown out with the metaphysical bath water." We can still therefore find philosophers such as Bunge (1962, 29) stating that "sensible intuition and geometrical intuition, or the capacity for spatial representation or visual imagination, have very few defenders in mathematics nowadays, because it has been shown once and for all that they are as deceptive logically

[12]Atiyah et al. (1994, p. 203)
[13]Atiyah et al. (1994, p. 191)

as they are fertile heuristically and didactically. Therefore, what is usually called mathematical intuitionism does not rely on sensible intuition," a statement at odds with both Thurston's and J&Q's critics' replies. Bunge also states that "the products of intuition are rough to the point of uselessness; they must be elucidated, developed, complicated. The intuitive 'lightning', the hunch, may be interesting in the mind of an expert if it is cleansed and inserted into a theory or at least a body of grounded beliefs. This is how our intuitions gain in clarity and scope. By being formulated into formulated concepts and propositions, they can be worked out, analysed and logically tied to further conceptual constructions. Fruitful intuitions are those that are incorporated in body of rational knowledge and thereby cease being intuitions." But as Fischbein (1987, p. 175) points out, "it is clear that Bunge is projecting features of an elaborated theory onto the process of elaboration itself."

Perhaps even more surprising is that the existence of proofs does not necessarily lead to an increased belief in mathematical theorems. MacKenzie (2001) has analysed the historical development of computer-aided proofs, paying particular attention to the mechanised (computer-aided) proof of the Four Colour Theorem, and how many mathematicians initially rejected this proof despite the fact the mechanisation of proof is nowadays common in mainstream mathematics.[14] MacKenzie (2001, p. 102) cites mathematician W. Tutte, "the feeling is that the Four Colour Theorem ought not to have been provable like that, 'by brute force'...I have wavered between belief and disbelief in Shimamoto's proof, but have never liked it", and also F. Bonsall stating in 1982 that "we cannot possibly achieve what I regard as the essential element of a proof— our own personal understanding— if part of the argument is hidden inside a black box," further down demanding to avoid wasting funds on "pseudo mathematics with computers." In a semi-popular exposition, Gonthier (2008, p. 1382) writes that the 1976 proof of the theorem "had a hint of defeat: they'd had a computer do the proof for them!"

[14]The theorem says that for any map, the countries can be coloured by any of four colours without any two countries with common borders sharing the same colour. If the theorem is true, map-makers are guaranteed that there is at least one way to colour a map using four colours without neighbouring countries sharing colours, although it does not say how to colour any specific map to achieve this.

5.6 Formal proofs and trust: the Four Colour Theorem

Though its results may be plausible, mathematicians seem wary of computer-assisted proof. Gonthier (2008), referring to a revised 1995 version of the Four Colour proof, writes that despite mathematical controversy having died, "there was something still amiss: both proofs combined a textual argument, which could reasonably be checked by inspection, with computer code that could not. Worse, the empirical evidence provided by running code several times with the same input is weak, as it is blind to the most common form of computer 'error': programmer error. Gonthier himself has published a 'computer-checked proof' of the theorem different in strategy to the original one, in which "every single logical step" is made explicit. Gonthier's proof therefore relies on what he calls 'general arguments' and not on examination of a set of particular cases (Gonthier writes that in this sense, his is a 'meta-proof' and not just a mere proof; meta-proofs allow mathematicians to talk about why an argument is valid, and not just to show that it is valid for all cases).

Hales (2008, p. 1371) writes that "traditional mathematical proofs are made to make them easily understood by mathematicians. Routine logical steps are omitted. An enormous amount of context is assumed on the part of the reader. Proofs, especially in topology and geometry, rely on intuitive arguments in situations where a trained mathematician would be capable of translating those intuitive arguments into a more rigorous argument." Compared to these intuitive arguments, a formal proof is "a proof in which every logical inference has been checked all the way back to the fundamental axioms of mathematics. All the intermediate logical steps are supplied, without exception. No appeal is made to intuition, even if the translation from intuition to logic is routine. Thus, a formal proof is less intuitive, and yet less susceptible to logical errors." Hales (ibid.) highlights that this is a painstaking process; "A. Matthias has calculated that to expand the definition of the number '1' in terms of [Bourbaki's formal system] primitives requires over 4 trillion symbols."

If ultra-formal proofs and meta-proofs are so ridiculously complicated, and if they don't afford any sort of 'understanding', why go through all the trouble of implementing them? Wiedijk (2008, p. 1408)) answers that in writing formal proofs one is "ensuring a reliability that is orders of a magnitude larger than if one had just used human minds." The 'formalist' computer proof makers like Gonthier propose that computers make better proof-checkers because one must necessarily take the formalisation seriously when the actual programming is carried out.

MacKenzie (2001, ch. 4) argues that the Four Colour proof became acceptable not because the proof was correct (nobody could have 'checked' it, except a computer-brained human!) but because people considered that the arguments were 'sufficiently' good. The J&Q and the Four Colour theorem episodes highlight one of sociology of science's most important reflections on science: truth is never 'self-evident', it is socially consolidated and always has elements of conventional agreement. Even in mathematical logic, with its particular notion of truth through proofs, truth does not escape construction and convention. MacKenzie (2001, p. 147) writes that "for some [...] to put one's trust in the results of computer analysis was to violate the essence of mathematics as an activity in which human, personal understanding is central. To others [...] using a computer was no different than to use pencil and paper, which is universally accepted in mathematics." In the end, one again faces questions of trust, and where trust is implicitly and explicitly placed.

5.7 The tortoise and Achilles

One can take the tacit agreements necessary for the application of logic literally to another 'level', as Carroll (1895) showed in his dialogue between the Tortoise and Achilles. In the dialogue, the Tortoise shows that the proper application of logic to the most trivial syllogisms necessitates the tacit acceptance of rules that must remain unchallenged, and usually remain unstated. For example, consider the statements A="all apples are red", and B="this object is an apple." It follows— trivially one could say— that statement Z="this object is red" is true. Carroll (or the Tortoise) asks us to consider the statement "D=if A is true and B is true then Z is true," and ask if it is possible to deduce the truth of D from A, B, and Z, otherwise this in itself implying the tacit acceptance of the 'meta-rule' that trivial syllogisms are themselves proper rules. Carroll shows that in order to practice logic then, the basic rules of logic must remain accepted as conventions, or otherwise an infinite regress occurs which cannot be broken by appealing to higher order theories (meta-theories). The point is explored even further in Hofstadter (1979) in connection with Gödel's incompleteness theorems and Artificial Intelligence.

It is thus plausible that between mathematics and formal logic there is a sociological connection analogous to that between experimental physics and theoretical physics, and the one between theoretical physics and mathematics. Alas, it is a connection that will not be pursued here, but that this is so can be glimpsed by analysing the attitude that mathematicians have towards hardcore set-logical proofs as described by Harrison (2008, p. 1399), which itself seems to echo the attitude of physicist towards 'rigorous' mathematical proofs:

> Since mathematics is supposed to be an exact science and, at least in its modern incarnation, one with a formal foundation, this situation seems thoroughly lamentable. It is hard to resist the conclusion that we should be taking the idea of formal foundations at face value and actually formalising our proofs. Yet is also easy to see why mathematicians have been reluctant to do so. Formal proof is regarded as far too tedious and painstaking. Arguably formalised mathematics may be more error-prone than the usual informal kind, as formal manipulations become more complicated and the underlying intuition begins to get lost.

Thurston (1994, p. 165) also meditated upon the role of logical deductive arguments (rigorous proofs, or what he calls the "popular caricature" of definition- theorem -proof mathematics), making a similar point as to how logic becomes a 'black-boxed' set of rules for pure mathematicians in the proof-construction work (delegating this work to logicians):

> Mathematicians apparently don't generally rely on the formal rules of deduction as they are thinking. Rather, they hold a fair bit of logical structure of a proof in their heads, breaking proofs into intermediate results so that they don't have to hold too much logic at once. In fact, it is common for excellent mathematicians not even to know the standard formal usage of quantifiers (for all and there exists), yet all mathematicians certainly perform the reasoning that they encode.

When it comes to the practice of mathematics, it appears that it is simply too destructive to go into a logic-minded-Tortoise way of thinking, just as like in physics it is unproductive as a physicist to think like a mathematician.[15]

[15]Sociological analyses of practice in pure logic and in the elaboration of mathematical proofs from a logician's perspective can be found in MacKenzie (1999), Rosental (2003)).

5.8 Rigour in physics

Theoretical physicists have a different relationship with mathematical 'objects' than do mathematicians. In contrast to mathematicians, theoretical physicist usually have no problem using 'rules of thumb' or intuitive and proof-less results. Theoreticians, unlike mathematicians, are less concerned with being able to provide or understand the rigorous elements of mathematical statements, but rather take them up if they can be used in productive ways. When mathematical rigour concerning mathematical objects is needed, theoreticians will often consult a mathematical colleague directly. Theoreticians see mathematics as a tool; a tool in which one can trust thanks to the work of professional mathematicians. Thus rigour is of little direct importance to physicists when carrying out their craft,

> Berry: I remember in the early days when I was involved with singularity theory I got to know Chris Bezzina, who was a really great mathematician. He was the person who brought catastrophe theory to a wider public. [...]. We talked a lot about what I did, making concrete use and understanding problems in optics using this new bag of tools; that was a new thing that came in, singularity theory. Then he said, 'I've got this very good student, and maybe you'd like to take him on as a graduate student.' And then I discovered that this guy learned mathematics and theorems, but he didn't know what a Bessel function was! [16] I had to explain that this is useless to me. So there are different kinds of expertise. We talked about theorems yesterday. I myself in my work don't have the ability to prove a theorem to the rigorous degree that the mathematician uses. I quoted Thom, 'imbeciles can prove theorems.' Not really true [laughs]. It's not what I do. It's not useful for me to have somebody around me who does. I don't need that. It's not the level at which I'm working. If there was a genuine uncertainty about something which really required a mathematician— and there are occasions as these— then I would find it profitable, worthwhile to consult someone who knows that culture, but mostly not, certainly not

[16] Bessel functions are mathematical functions that are solutions to a general type of mathematical equation and that arise in many areas of physics, particularly when problems are posed in terms of cylindrical coordinates.

> with a student who doesn't know a simple hands-on calculating tool that I use.

or as Mitchison (who is originally an applied mathematician, and a theoretician only in the later stages of his career) explained,

> Mitchison: I tend to stay with theoreticians. I don't talk to mathematicians unless I'm really stuck and I know there's something, and then I have friends who are mathematicians, and one very, very brilliant mathematician who very kindly phrases things in a language I can understand. But mostly I just talk to my colleagues.

The ability to use mathematical tools without having to rigorously work out their usage— the task of 'rigorous' mathematics— is another illustration of the trust which physicists rely on, once mathematics has been 'black-boxed' and the derivation of results been made tacit. A theoretician can be confident that the tools he is using have inherited the epistemic strength of proved mathematics because others have done the work, or at least because the theoretician believes that someone has done it. As Berry noted, there is not even the need to personally know someone who has carried out the proof, except in extraordinary cases. Just as experimental knowledge flows from experiment via virtual empiricism, mathematics is supported in physics by 'virtual rigour', if one may abuse the phrasing from the previous chapter. Again, this does not mean that the world of pure mathematics is impregnable to theoretical physicists, but only that mechanism of institutional trust is established as a way to make the division of labour possible. Many theoreticians enjoy going through the proofs of the tools they use, even if they do not carry out work in pure mathematics. [17]

5.9 Levels of understanding

One must make a distinction between rigour in supporting a mathematical tool to be used, and the actual argument that uses the tool to derive a 'physical' result, although

[17] For the role of mathematical proofs as mechanisms for the suspension of doubt in physics and the black-boxing of mathematical results through proofs, see Pinch (1977) and his discussion of the von Neumann-Bohm debate concerning quantum physics hidden-variables proofs.

it is not simple to demarcate one 'level' from the other in practice. Trust works at both levels, but with different objectives: a theoretician may trust his mathematical tools just like a car driver can trust the car manufacturer to have constructed a vehicle that is safe to use; a theoretician may also trust the results of a paper that manipulates and transforms equations that rely on these tools themselves. In the latter case, trust is established on both the tools and those who use them, like one trusts other drivers on the road to stop and let one safely cross at a crossroads when they are stopped by a red light.[18] In the first case, one trusts mathematicians/car manufacturers whose world is practically inaccessible, while in the second one trusts an author's/ car-drivers actions who are accessible, though the full access may require too much effort. Collins (2007) has identified a hierarchy of mathematical understanding in theoretical physics that classifies the way in which theoreticians enter through different 'levels' of understanding:

1. Authority based understanding: "judgement on the basis of whether a mathematical argument has been published in a peer-reviewed journal or similar source without any understanding other than the reputation of the author."

2. Familiarity based understanding: "acceptance, or otherwise, on the basis of the fact that the claim makes sense or otherwise within the familiar and consensual conceptual structure of the scientific domain. The mathematical exposition does not have to be read."

3. Impressionistic understanding: "the mathematics is read to gain an impressionistic sense of the argument but it is not followed step-by-step."

4. Checking the proof: "any level of reading that does involve following the proof step-by-step."

5. Innovatory understanding: "doing brand new proofs of the same sort where you do not already know the outcome."

[18]In Mexico City, for example, this is not the case, and one must be *very* careful when crossing many intersections even if one has a green light. This situation is equivalent to reading a paper by another theoretician that has a bad reputation of making mistakes in derivations or calculations: this gives rise to an 'abnormal' attitude of suspicion.

6. Domain-specific ownership: "«ownership» of the mathematical area in the manner of, say, a physicist, or whichever domain scientist is using the mathematics."

7. Mathematical ownership: "«ownership» of the mathematical area in the manner of a mathematician."

According to Collins' query among a small sample of theoreticians, an average of 60% of the papers they read are at most to level 3, while 85% of their reading is done up to level 4. Collins supposes that levels 1 to 4 are the domain of most physicists, while the upper levels are more akin to mathematical practice.

The adjective 'rigorous' is also applied differently depending on whether one is inside the physics or the mathematical community, and in each of these communities the purpose of rigour is quite different. Physics-rigour is related to offering theoreticians the possibility of playing around with mathematical models without worrying too much about whether these models are directly linked to physical reality or not, because they are supported by 'good' mathematics and therefore they may in the end provide descriptions of possible physical scenarios. The fact that these models have been 'proven' to constitute legitimate mathematical objects makes them immediately interesting.

Mathematicians seem willing to overlook physicist's 'sloppiness' when the 'theoretical' developments may turn out to inspire future mathematically rigorous results. On the other hand, inside the mathematical community, the process of legitimisation in itself, the application of rigour, is an important objective in itself. There are also intermediary categories: theoreticians who care little for the actual physical applicability of their results in a physically realisable system ('mathematical physicists') or mathematicians who work out techniques for applying mathematical results to any system, without paying much attention to the proof of novel mathematical statements ('applied mathematicians').

Physicists can get away with keeping a lot of 'rigorous' background knowledge tacit in their publications, while mathematical papers are different in this respect. Harrison (2008, p. 1395) explains that "formal proof is a proof written in a precise artificial language that admits only a fixed repertoire of stylised steps. This formal language is usually designed so that there is a purely mechanical process by which the correctness of a proof in the language can be verified." Harrison also states that the use of proofs is "to improve the actual precision, explicitness, and reliability of mathematics." But this difference between the level of rigour should not be taken to mean that there is nothing tacit in a mathematical paper, but only that the collectively tacit is eliminated in mathematics as much as possible, at least relative to the allowances that seem common to theoretical physics, but this does not eliminate the relational-tacit fully.[19] Harrison (2008, p. 1398) admits that despite the struggle for clarity, "mathematicians seldom make set-theoretic axioms explicit in their work, except for those whose results depend on more 'exotic' hypotheses. And there is little use of formal proof, or even formal logical notation, in everyday mathematics; Dijkstra has remarked that 'as far as the mathematical community is concerned George Boole has lived in vain'. Inasmuch as the logical symbols are used [...] they usually play the role of ad hoc abbreviations without an associated battery of manipulative techniques."

If we take the responses to J&Q as representative of mathematical practice, then surprisingly it seems that creating innovative mathematics is not so strongly associated with working out proofs. Although it is very likely that proofs would play a much bigger role in mathematics than in physics, the dominance of hard-core logic in mathematical practice is not as high as is commonly supposed, particularly when it comes to pioneering work. Collins concludes his mathematical-level article noting that even though there exist atypical cases (like refereeing) where the mathematics in physics can become central, "the necessity of mathematics under circumstances like these does not show that every physicist has to be a mathematician, only that some physicists have to be mathematicians." We could rephrase for mathematics saying that even though there are cases in mathematics that demand logic and rigour, this does not show that every mathematician has to be a logician, only that some mathematicians have to be logicians. The thing to note is that intuition in its many guises is the vanguard of mathematical creation, and logical proof comes only a posteriori. This should to force us to consider that, because of (and not despite of) the importance of mathematics in theoretical physics, tacit knowledge should play a leading role in the account of both mathematical and theoretical physics practice.

[19]The degree of work that writing a mathematical paper involves can have deep effects on the configuration of mathematical work; for example, it can be seen to deeply influence the rate of publication for mathematicians. Grossman (2002) has shown that around 60% of mathematicians have only published between one and two papers in their lifetime; although he admits that this includes mathematicians at any stage of their career, the numbers is sufficiently low to clash with physics practice, where one article per year is still a rather low number. Physicists also tend to publish more in collaborative work, and more as one shifts from the purely theoretical, to the phenomenological and then to the empirical.

Part II

Tacit knowledge in theoretical physics

CHAPTER 6

Relational and somatic tacit knowledge in physics and mathematics

6.1 Intuition in theoretical physics

The discussion surrounding J&Q's paper highlights the importance that intuitive and informal processes play in pure mathematics for working mathematicians. J&Q's position regarding the 'dangers' of intuitively oriented mathematics is not unusual, but as many of the responses to the paper highlight, it seems a position that simply does not do credit to the way mathematics is produced, but rather to the way that it is rationally reconstructed once the mathematical idea is developed.

As far as theoretical physics goes, intuition seems to play exactly the same fundamental heuristic role as it does in mathematics, the difference in physics being that logical proofs are both much less stringent and much less common in the general practice of the field. Although formality may be slacker, theoretical physics still relies on mathematical and logical arguments to present theoretical ideas. In most of my interviews I confronted physicist with the question of whether there are additional major heuristic guides in theoretical physics besides experimental knowledge and mathematical reasoning. The answer in all cases was the same: a third major heuristic is 'physical' intuition. A typical example is the following exchange:

> Reyes: Mathematics, for theoretical physicists, is a tool, right?

> Mondragón: Yes.
>
> Reyes: On the other hand you have experiments, which is a guideline, which is restrictive. Is there something to theoretical physics beyond these two dimensions?
>
> Mondragón: Yes of course. There is intuition, and a certain form of...good taste. [...] Yes, of course. You have your tools. You need a good toolbox or you'll find you can't solve your problems. And what you want to describe are physical phenomena; things you can observe. For example, "Why are there three generations [of quarks] and why are their masses so different?" There, you definitely need a strong intuitive and creative component. If you have it and you don't have the tools, it's like having musical taste but not having the ability to play the piano. It's not good for much. But if you only have the tools and you don't have that feeling, that intuition, you'll be reduced to doing very technical things. There is a creative component, not unlike writing poetry [laughs]. When you see the great physicists you ask yourself, "How on Earth did they come up with that? How did he imagine that the solution was to plug in a scalar?" or something like that; flashes of imagination. There is also a certain aesthetic sensibility. Sometimes you see a theory and you say, 'They might describe many things, but the physics is horrible.' You just look at them and it seems ugly! They plugged in this scalar there, and it just looks baroque, with lots of...things. And then others you look at and you just say, 'This has to be right. If the experiment is not exactly coincident it has to be very close.' It's beauty...in the equations, or in the formulation. It may have to do with the symmetries that are somehow built into our brains and that one recognises.

The response followed the same pattern every time. Intuition along with experiment and mathematics is one of the key practical elements of theoretical physics. For some, it is in fact a theoretician's level of intuitive understands which marks him as a true physicist:

> Romero: A good physicist— I'm not talking about a genius, just a regular good guy— is the one that builds his own intuition. One learns it partly

> through his supervisors, during his postdoc, with your colleagues when you are still young, but with the years you develop your own, in your own style.

A key question is then to ask what it is that 'physical intuition' is composed of. Being such a crucial component of a theoretician's toolbox, at the same level as 'experiment' and 'mathematics', it is remarkable, though perhaps not surprising, that 'physical intuition and 'physical insight' prove extraordinarily hard, or actually impossible, to define:

> Reyes: I find some trouble understanding this [intuition]. It's relatively easy to understand what a mathematical 'argument' is...
>
> De la Peña: Yes, that's right.
>
> Reyes: It's relatively easy; it's centred on a deductive procedure...
>
> De la Peña: Yes, and the other one is a physical intuition. Yes, it's a physical intuition. People will want to slap me; I know any philosopher would want to slap me, 'what do you mean with an intuition?' It's an educated intuition. It's an intuition that one acquires with time. It doesn't emerge spontaneously. It emerges from having gone deeper into the problem, in the physics of the problem. The hard part is to create such an intuition. That's why problems in physics are complicated.

De la Peña, who specialises on a highly unorthodox type of quantum theory known as Stochastic Quantum Electrodynamics, may be argued to not be representative of the theoretical status quo. In fact, he is fully aware that his views lie outside the orthodoxy, but that it is his engagement in debates regarding a universally acknowledged importance of intuition in physics that has allowed him to still dialogue with his theoretical colleagues *despite* these differences:

> De la Peña: Look here, why are the problems of quantum mechanics so complicated? Well, precisely because there is no such thing as a sufficiently deep quantum intuition. It's lacking, completely. One does not know

how to dive in. Any argument that is mathematical, if it works, it's considered good. The historical construction of quantum mechanics was accomplished by finding mathematical descriptions that allowed the depiction of the physical situation. But there was little knowledge of what the physical situation was which was being described. There was no intuition of that physical situation. That was the problem. That is the problem that I devote myself to try to understand, to try to find that missing construction, and consequently that profound intuition of the quantum world that is what would allow us to advance along other lines.

Maybe the word intuition is inappropriate, because rather it is a cultivated thing. In theory, intuition is not something one cultivates! Einstein handled this concept; he spoke of intuition. He said, "There's something in me that does not allow me to think that the world is not deterministic. God does not play dice". It's a physical perception. It's what I would defend. God either plays dice or does not. The conviction that it does not is what moves us; what moved him and what moves me. That of physicist nowadays is, "Who knows if he does. What do I care?"

Mathematics accounts the phenomena. Mathematics is the apparatus that gives account. That is only a description; it is not comprehension. Science is needed to understand the world. Understanding is something much deeper than that. It's not enough to know how to describe things. It's not enough to be able to calculate some little number. One has to understand the 'why' behind the number. It's false — in my opinion — that science's aim is to calculate the number. The objective of science is to understand the little number

Mitchison similarly failed to verbalise intuition:

Mitchison: Sometimes you can play [physics and mathematics] off against each other, the kind of physical intuition and feeling for how things work so aligned to mathematics that, you know, a particle is a representation, or you simply move between different chunks of mathematics in every new world. ...Intuition is...something that I still haven't got [laughs]. Intuition

> is, something which you get...so, which some people seem to have...intuition is...I guess...it's yeah...[very long silence] it's very, very difficult to describe! [...] It's a certain kind of picture of how things; whether it's curved space-time or molecules vibrating in a lattice. It's just having a picture, as Feynman said, which would lead him to where the mistake in the equation was. I think that that kind of picture is what counts as physical intuition.
>
> Reyes: In a way this idea of physical intuition is at odds with the idea of rationality, you know, theoretical physics as...
>
> Mitchison: Well, only with a very narrow idea of rationality! I mean, *rationality is not what doing mathematics or logic is. It's something you do retrospectively to check it out.* And so how you get the things, how you get the material that you then apply rationality to, is through physical intuition, and I don't see that as being contrary to it. (emphasis added)

Curiously, for some theoreticians, it seems that the inexpressibility of 'physical intuition' is not only unremarkable, but is also a quintessential part of its nature:

> Reyes: One thing that [people I've interviewed] have mentioned many times is physical intuition. That is the part that nobody has given me an answer to: what is physical intuition?
>
> Berry: Well, why do you expect an answer? *The very word intuition implies that you're not going to get a very clear...there isn't going to be an algorithm for it. You know, there isn't. That is what intuition means.* Of course, you get it— if you're lucky enough to get it— by having studied a lot of problems and seeing connections between them. That's where the intuition is. You see that something is like something else you did, except for some crucial aspect. This locates it in the world of doable, understandable phenomena. But intuition is...intuition means that you're so familiar with similar situations that the connections come almost unconsciously. That's what intuition is. I think I hadn't thought about it, or hadn't thought how to describe it. But I think it's that.
>
> Reyes: So it's based on experience?
>
> Berry: Yes, of course. (emphasis added)

There are thus several important points associated with 'physical intuition', which should be clearly set out:

1. It's one of the three major heuristics of theoretical practice, alongside experiment and mathematics.

2. It's inexpressible in an algorithmic form.

3. It is acquired with time, both by exposure to relevant problems, and by attempting solutions to these problems.

4. It is learnt through direct practical immersion; it is passed on to young physicists through close supervision of attempting solutions to real research problems, in the style of an artisan apprenticeship.

5. There is a creative component, and it is what in real-world research circumstances leads physical practice, as opposed to the mathematically rigorous reconstructions, which are retrospective exercises.

6. There is an aesthetic component, which may be closely linked to intuition being based on the possibility to visualise physical phenomena, but which in others may hinder the 'spontaneity' of intuition.

7. Physics that is not led by intuition is usually reduced to very technical work, although physics with only intuition and no mathematics is "like having good musical taste but not having the ability to play the piano".

Intuition as unstated knowledge, is some sort of tacit knowledge, the crucial question being what kind. It cannot be relational tacit knowledge, because it does not hinge on things being left out, but rather on things being *inexpressible*. It is not located in the collective, but is rather in the individual, as it is 'taught' and acquired through attempts at successful performance; although physics can be performed without intuition leading the way, theoreticians typically see this as physics that is too 'mechanistic'. The only type of tacit knowledge left from Collins' typology is somatic tacit knowledge, which in fact exhibits *exactly the same characteristics* as physical intuition as outlined above, except that the 'soma' (from the Greek word for 'body') is to be replaced by the 'mind'. 'Physical intuition' is the somatic tacit knowledge dimension of theoretical physics. One problem that could be conceived when linking these two concepts concerns the connection of somatic tacit knowledge with the body. Collins does mention chess-playing as a form of somatic tacit knowledge, and chess-playing can be conceived in a purely abstract form (think of the reduction that is made of chess games to a set of written steps when it is described in a newspaper sports column; the list of moves is actually the chess game), and so this does not seem like a particularly relevant objection. After all, *the brain is part of the body.*

Polanyi (1958, p. 59) supported this extension of the somatic to the mental realm, as he argued that the somatic-tacit mastery of tools and instruments should not be reduced to the physical plane, for "while we rely on a tool or a probe, these are not handled as external objects. [...] We pour ourselves out into them and assimilate them as parts of our own existence. We accept them existentially by dwelling in them. [...] Hammers and probes can be replaced by intellectual tools; think of any interpretative framework and particularly of the formalism of the exact sciences." Polanyi calls this alertness or awareness of the tools or probes that sit in one's hand 'subsidiary awareness' for "they are not watched in themselves; we watch something else while keeping intensely aware of them" in opposition with 'focal awareness' which is a full, conscious attention towards an object. Thus, in hammering a nail, we have only subsidiary awareness of the hammer as a hammer, and a good hammerer will literally feel the hammer as an extension of his hand, while there will be focal awareness of the nail as an external object. Likewise, one would expect that when wielding their mental 'tools' theoreticians would have only subsidiary awareness of this object. They would be able to use them proficiently, but without being able to put the process into an algorithmic form. This is physical intuition. The continuous reference of mathematics as a tool throughout the interviews is relevant here.

Polanyi stressed how true and full mastery of an art or a technique necessitates that the 'logic' behind the mastery remain indescribable, and gives numerous examples of activities where focal attention on the action itself renders it literally impracticable (e.g. piano playing, or bicycle riding). That 'the physics' of bike riding or the technique behind a pianist's 'touch' can still be explained post facto is only possible because these are physical objects which can be probed independently of the process. However, when we deal with intellectual 'tools' such as mathematical reasoning no such manipulation is possible, and the best we can do is either logically reconstruct the argument or make no attempt at an explanation. If we trust the ample mathematical and theoretical physics testimony presented here, then it is a fact that in many cases the solution to problems in these domains does not involve what is retroactively logically reconstructed. One must accept that a fully somatic tacit mechanism is involved in physics practice, one that may never be fully grasped through anything other than immersion into real theoretical practice. As Berry explained concerning the process of thinking about doing science related to how one actually does the science:

> We tend to be doers. It depends on how reflective a human being you are. [...] In fact, you've got to be a bit careful because if you spend too much time in the 'why', you stop doing it. That's true of all human activity. If you're walking and you think consciously about putting one foot in front of the other you'd stumble over, and if you're a musician and you're playing something on the piano if you try to think 'where's this finger going to go' then you can't do it anymore. To some extent, reflection can inhibit activity. You've got to be a bit careful.

As Polanyi (1958, p. 53) notes, "an art which cannot be specified in detail is an art which cannot be transmitted by prescription, for no prescription for it exists. It can be passed on only by example from master to apprentice. This restricts the range of diffusion to that of personal contacts, and we find accordingly that craftsmanship tends to survive in closely circumscribed local traditions." There is no reason to expect that mathematics or physics and the mastery of their tools are any different, and thus one would expect that the acquisition of successful physical intuition can only be gained through contact with a master craftsman in these trades, through full immersion into the world of real research with close supervision; as in fact it actually happens. One is reminded of Wittengenstein famous closing aphorism from the *Tractatus*: "What we cannot speak about, we must pass over in silence."[1]

Polanyi's work is clearly set out in the phenomenological tradition within philosophy. The analysis of the hammer is actually one of Heidegger's best-known phenomenological analyses, that of tools and tool-usage. Polanyi echoes Heidegger (1927, p. 98)

[1]Wittgenstein (1922, §7).

when the latter writes that when using tools such as hammering with a hammer, an entity like the hammer

> "is not grasped thematically as an occurring Thing, nor is the equipment-structure known as such even in the using. The hammering does not simply have knowledge about the hammer's character as equipment, but it has appropriated this equipment in a way which could not possibly be more suitable. [...] The more we stare a the hammer-Thing, and the more we seize hold of it and use it, the more primordial does our relationship to it become, and the more unveiledly it is encountered as that which it is— as equipment. The hammering itself uncovers the specific 'manipulability' [Handlichkeit] of the hammer. The kind of Being which equipment possesses— in which it manifests itself in its own right— we call 'readiness-to-hand' [Zuhandenheit]. [...] 'Practical' behaviour is not 'atheoretical' in the sense of 'sightlessness'. The way which it differs from theoretical behaviour does not lie simply in the fact that in theoretical behaviour one observes, while in practical behaviour one acts [gehandelt wird], and that action must employ theoretical cognition if it is not to remain blind; for the fact that observation is a kind of concern is just as primordial as the fact that action has its own kind of sight."

Polanyi's 'subsidiary awareness' is thus akin to the hammerer's perception as Zuhandenheit, which Heidegger contrast with the attitude of 'presence-at-hand' (Vorhandenheit) that is 'theoretical' in that it is concerned with observable facts about the object. The tension between these two attitudes in both Polanyi's and Heidegger's accounts can be seen to mirror the tension between the processes that bring about the explicit and somatic tacit knowledge about the usage of a tool. Polanyi however goes beyond material tools, and extends this to intellectual tools, which are of course hallmarks of theoretical practice, pointing out that the mastery of a tool involves deep readiness-to-hand knowledge, and little presence-at-hand knowledge.

Dreyfus (1991, section 4.I.B) has discussed this passage and related it to the analysis of the blind man's cane by Wittgenstein, Polanyi and Merleau-Ponty, where the comparison is between the blind man describing what the cane's 'properties' are ("light, smooth, about three feet long"), and the moment when the blind man actually uses the cane, and loses awareness of the cane itself and feels it become an extension of his own body through which he can actually feel the external world.[2] Because in Merleau-Ponty (1945, p. 143) Gestalt psychology tints the dialectical elements of analysis, the classic tension between subject and object is transformed to that between the foreground consciousness of the body, and of the background that constitutes the world within which the body moves.[3] In the blind man's case, "The blind man's stick has ceased to be an object for him, and is no longer perceived for itself [...] In the exploration of things, the length of the stick does not enter expressly as a middle term: the blind man is rather aware of it through the position of objects than of the position of objects through it. [...]To get used to a hat, a car or a stick is to be transplanted into them, or conversely, to incorporate them into the bulk of our own body." Dreyfus & Dreyfus (1980) have proposed that somatic tacit knowledge of this kind, being completely unstatable, does not share the features of 'true' knowledge, and choose the term 'intuition' for it. However, as Collins (2010, p. 106-113) argues, that somatic knowledge is unstatable is only an accidental feature of the way we as humans have integrated it into our being; if a machine can reproduce the behaviour, even if it is through a different process, then a relationship can be traced between the unstated human version and the stated machine version that undermines the separation of the somatic from 'real' knowledge.

6.2 Somatic tacit knowledge and collective knowledge

A tension arises here between intuition as somatic tacit knowledge, and the sociological definition of knowledge discussed in Chapter 1, where it was claimed that, sociologically, personal knowledge is of no consequence to sociology. Certainly, one would be forced to admit that somatic tacit knowledge as such has no apparent sociological dimensions, it being of an individual nature, but there are several reasons for supposing that even at this deep personal level the influence of sociological factors is deep. In the training of young theoreticians, although the acquisition of intuitive skills is a personal affair of practical immersion, this immersion is ideally guided by a recognised expert who directs the novice in the right direction of performance. A notable result of this training method is that the novice does not just learn to solve problems, but acquires

[2] See Polanyi (1966, p. 12-16), Wittgenstein (1953, § 626) and Merleau-Ponty (1945, p. 143).
[3] See the *Editor's Introduction* in Merleau-Ponty, Maurice & Baldwin T. (ed.) (2003).

his own style of doing so which echoes the teacher's predilections:

> Romero: The way [teaching at research level] is done is like this. You talk to your student about a particular problem. Typically when you are starting out you have a tutor who talks to you about a problem you don't understand, and sometimes even he does not understand it! Maybe he half understands it. And he leads you towards a point where you can try to solve it. But typically, he will show you things that he himself knows how to do. He knows how to do it because he's tried it out for other things, and he advises you to also try out the same thing. Now that I'm on the teaching side, I know that one does it with the hope that the student will turn out to be a real jewel and that he'll be able to do something that you yourself weren't able to do treading the same roads that you yourself once took. *It really is like being an artisan's apprenticeship.* Although maybe even artisans are more professional than us since they're more systematic. It's an artisan learning process. (emphasis added)

Thus a theoretician will generally try to tackle a theoretical problem with the 'tools' he is most adept at, and these are naturally those that were learnt in his early research career. As Romero also mentioned, it is not altogether rare that despite knowing plenty of methods, theoreticians will fight to frame the problem so that it can be tackled with the tools they are most proficient in:

> Romero: If you learnt for example a lot about Laplace transforms in your PhD…we had a postdoc mate whom we made a lot of fun about. We'd joke and say, "hey, ask this guy how to solve a problem and he'll tell you to first do the Laplace transform." You'd ask him and really, he'd say, "what if you take the Laplace transform first?" He was such a good problem solver. He'd arrange the problem so that he'd be able to take the Laplace transform and he'd then have the equation. Others, they choose the Green's function approach…everything is Green's functions! For other everything is some other [mathematical] method.
>
> Everyone learns particular tools, at a very early age I think, typically when your mind is still fresh. You base yourself on those tools, and you reduce

> all problems to a usage of those tools. [Some theoreticians] reduce every problem to the same thing. [A] reduces every problem to a transfer matrix problem, or a dispersion matrix. [B] reduces everything to a transfer matrix. They reduce every problem to obtain the same equation so that they can then do what they know how to do.
>
> One tackles problems that can be handled through the things that you learnt know how to do; not necessarily problems that one thinks are important. This is typically how one carries on. Technique is learnt at an early age, during your PhD or your postdoc, or the first years of your professional career when you are open to learn new things, new techniques.

The content of a theoretician's mathematical tool bag is thus shaped by his early career, though Romero did not mention whether this is a conscious phenomenon or not. It may well be that this combination of a 'natural' way for a theoretician to grasp a problem tinged— a theoretical 'style'— marked by the tradition that the theoretician grew up in has mixed elements of both the somatic and the collective tacit. Berry likewise reflected on how the content of his mathematical 'bag of tricks' was deeply influenced by work with his PhD supervisor:

> Berry: I like asymptotics and divergent series; I've worked a lot on them. So the tools, I like the tools; using the tools. There's that, and it's an area where I can do something; there's a fair amount of space [...] It's hard to be specific why you like something; why you do something. Maybe historically I was in it too. My PhD supervisor who just died— Bob Dingle in St. Andrews— he made utterly seminal contributions to understanding divergent series. [...].
>
> Underlying a lot of what we do are tools. And a lot of tools have to do with divergent series. One of my favourite tricks is the Poisson summation formula. That's a formula for evaluating sums. And it relates a sum to another sum where the quantities in the second sum are the Fourier transforms of the ones in the first sum. I encountered it in a paper decades ago and then I very quickly realised— but don't ask me how because I don't know— that this is a very general notion and it replaces a sum over quantum numbers (one sum) by a sum over topologically different classical paths (that's

> the other sum) and this is a very valuable duality and I've applied it to a half a dozen different problems over many years, and it keeps being useful and it gives a huge amount of insight. So that's another trick. So I don't know where they come from. Asymptotics I learnt from my PhD supervisor Dingle, I mentioned that. He sensitised me to asymptotics.

Thus, although the development of the tools is a personal process, the guidance of a novice's tool selection is determined by his senior mentor, the research group he is immersed in, and the traditional tools which are mastered within this tradition. There are a few important historical studies that have concentrated on the development and dispersion of such theoretical 'tools' and the traditions and institutions which these spark, the most detailed being Kaiser's on Feynman diagrams, with smaller work on the dispersion of General Relativity.[4] As Kaiser (2005b) sums up the corpus of his studies on Feynman diagrams,

> As physicists recognised at the time, much more than published research articles or pedagogical texts was required to spread [Feynman] diagrams around. Personal mentoring and the postdocs' peripatetic appointments were the key. Very similar transfer mechanisms spread the diagrams to young theorists in Great Britain and Japan, while the hardening of the Cold War choked off the diagrams' spread to physicists in the Soviet Union. Only with the return of face-to-face workshops between American and Soviet physicists in the mid-1950s, under the "Atoms for Peace" initiatives, did Soviet physicists begin to use Feynman diagrams at anything resembling the pace in other countries. [...]Thus it remains impossible to separate the research practices from the means by which various scientific practitioners were trained.

[4]See Kaiser (2005a) and Kaiser (2006), respectively.

6.3 Revisiting Planck's solution to the black body problem: the importance of physical intuition

Physicists, like mathematicians, insist that it is intuition that most often leads the way for theoretical practice. But is 'intuitive' theoretical physics then the only option? It seems that while intuitive physics is the preferred method, it is not the only successful method. Revisiting the episode of Planck's solution to the black-body problem, it is interesting to reexamine to search why Planck did not approve of a mere data-fitting approach (see Section 2.7, p.56). Planck (1920) admitted that his initial answer to the blackbody problem was little more than an ad hoc move:

> Even if the radiation formula should prove itself to be absolutely accurate, it would still only have, within the significance of *a happily chosen interpolation formula*, a strictly limited value. For this reason, I busied myself, from then on, that is, from the day of its establishment, with the task of elucidating a true physical character for the formula, and this problem led me automatically to a consideration of the connection between entropy and probability, that is, Boltzmann's trend of ideas; until after some weeks of the most strenuous work of my life, light came into the darkness, and a new undreamed-of perspective opened up before me. (emphasis added)

The formulation of the Planck distribution initially presented at a seminar was thus empty of 'physical' content. It was led by no other argument than the need for data-fitting, and a limited set of criteria concerning the form the function had to take. Although intuition for the development of the theoretical functional criteria Planck had to adhere to did arise through 'physical arguments', the actual solution to the problem did not. Although Planck did not discuss this any further, it seems Planck played around with equations of very simple kind and saw how these fitted the data best while still keeping within the physical constraints imposed by thermodynamics and the Wien law. What Planck accomplished seems not to be too different to what Collins describes a chess-computer does: apply a bit of heuristic rules and a lot of combinatorial brute force to find the optimal answer, as opposed to a chess grand master who uses a heuristic which is not amenable to full explication. In fact, this strategy of data fitting is not uncommon in a lot of modern physics, and computers in fact do it better than any human being. Even when Planck later derived a truly physical explanation for the seminal 1901 paper, what is clear is that physics of the highest impact may be at times led by non-intuitive means. The situation is similar in some computer-aided mathematical proofs such as the four colour theorem that was already mentioned, where a 'brute force' approach led to the examination of numerous cases that are not practically soluble, yet do constitute a logical but non-intuitive proof of the theorem.

Collins (2010) explains that if an activity in which tacit knowledge is evidently present can be successfully replicated by a machine, then even if the machine follows a different pathway, this knowledge is at the somatic, and not at the collective level. What I have argued with the Planck example is that this may indeed be possible in some theoretical cases, when what is wanted is an equation to describe a set of data, and that this can also be a useful heuristic tool for theoretical research, even if initially the equation is 'empty' of intuitive or 'deep' content. Thus as far as the end results go (producing an equation that fits the data) it can be possible in principle to substitute intuitive arguments with purely 'mechanical' ones. This is a second reason to view physical intuition as somatic tacit knowledge in theoretical physics, if Collins argument is taken at face value.

The Planck brute-force argument can be observed in pure mathematics, in the discussion surrounding computer-assisted proofs discussed in previous sections. What if Planck had been able to use, say, the program Mathematica to fit his data and find his function? The result would have possibly been the same! Yet while the establishment of 'physical' arguments for Planck's formula only took a matter of years, the 'axiomatization' of quantum mechanics did not come about until several decades later; yet in the course of a few years Planck's brute force formula had already revolutionised the physics landscape. In contrast, while mathematicians nowadays are wary of computerised 'brute force' proofs, even if they are useful in actually proving a hypothesis, it seems to matter significantly whether the intuition is there or not. While rigour and intuition both have a place in mathematics and in theoretical physics, comparing episodes such as Planck's and the Four Colour Theorem illustrates J&Q' point that rigour plays very different roles in these discipline. This is an important sociological difference that sets theoretical physics apart from mathematics independently of the 'objects' that they may study.

Trying to pin down the nature of theoretical physics practice, or of mathematics by reducing them to only intuitive components or only formalist components is a tasked doomed to failure. Neither physics nor mathematics is restricted in any sort of way by the idiosyncrasies of individuals' methods. Nor can one say that 'good' mathematics or physics are restricted by particular methods, formalist or intuitive. On this, Mitchison commented:

> Mitchison: The truth is, all these things are fair game. One of the wonderful things about physics is that there's such variation of personal style that some people just want to prove things with algebraic accuracy, and people who hate writing equations and would just want to get the idea home. There's so many ways of doing things that it's hard to think of something that would be a complete giveaway. Hearing David Deutsch talking, you might think, 'this is wild stuff' but of course, he's a sort of genius. So you really can't diagnose.
>
> I had another experience which was quite revealing to me. For a time I worked in computer vision; one way of looking at neurobiology, trying to understand what the problems are to solve the problems of vision in the brain. It's difficult computationally. There was a mathematician in the audience who had stopped being a mathematician and had decided to become a computer scientist, David Mumford. He happens to be one of the most extraordinary mathematicians in algebraic geometry. He just sort of created a whole field, really. Here was this man who had been working at the very highest level of mathematics attending lectures where probably the lecturer's level of mathematics had stopped at three years or something, you know, it wasn't very fine. I missed the talk and asked him, 'did you go to it?' and he said 'yes, do you want to borrow my lecture notes?' I saw his notes and he'd worked out every little detail. You might think this is sort of an undergraduate who goes to a lecture and kind of has to convince himself that everything is right. I was just amazed, because it made think, 'here's someone who really has to see how every different piece works.' It's completely the opposite of the powerful abstract mathematician who sees it in a twinkling without thinking more about it. I don't believe I can see

any pattern in that sphere.

6.4 The bodily experience of doing theoretical physics

I present two final examples of how we may link theoretical intuition to the somatic dimension. The first will deal with the prevalence of geometric thought in mathematics and theoretical physics. It seems that visualisation techniques play a very important role for these disciplines. Amongst the "facilities" that Thurston (1994, p. 164)) lists as important for mathematical thinking he counts:

> Vision, spatial sense, kinaesthetic (motion) sense. People have very powerful facilities for taking in information visually or kinesthetically, and thinking with their spatial sense. On the other hand, they do not have a very good built-in facility for inverse vision, that is, turning an internal spatial understanding back into a two-dimensional image. Consequently, mathematicians usually have fewer and poorer figures in their papers and books than in their heads. An interesting phenomenon in spatial thinking is that scale makes a big difference. We can think about little objects in our hands, or we can think of bigger human-sized structures that we scan, or we can think of spatial structures that encompass us and that we move around in. We tend to think more effectively with spatial imagery on a larger scale: it's as if our brains take larger things more seriously and can devote more resources to them.

This somatic dimension is also reported by Monsay (1997, p. 104), "I recall one particular clear example of intuition at work, later in my career in industry. I was working on a design for an underwater sound sensor [...] While discussing this situation with two engineers one afternoon, one of whom was the senior engineer I was assigned to work with, I suddenly knew I had the resolution to our problem. First came this 'knowing', then an essentially kinaesthetic feeling for what was involved, and finally, the words to describe the invention [...] The senior engineer immediately understood." R. Feynman was famous for the central role that visualisation played in his work, enunciating in his typically picturesque way, "what I am really trying to do is bring birth to clarity, which is really a half-assedly thought-out-pictorial semi-vision thing. I would see the jiggle-jiggle-jiggle or the wiggle of the path. Even now when I talk about the influence functional, I see the coupling and I take this turn - like as if there was a big bag of stuff - and try to collect it in a way and to push it. It's all visual. It's hard to explain. [...]The character of the answer, absolutely. An inspired method of picturing, I guess. Ordinarily I try to get the pictures clearer, but in the end the mathematics can take over and be more efficient in communicating the idea of the picture. In certain particular problems, that I have done, it was necessary to continue the development of the picture as the method before the mathematics could be really done."[5]

But perhaps the best illustration that links 'the body' with intuition is given by theoretician Ron Horgan, describing the process of identifying correct solutions to open theoretical problems being similar to car driving— one of the most characteristic examples of somatic tacit knowledge used continuously in the philosophical literature:

> Horgan: I think you might call it intuition, serendipity, and there's a gut feeling as well. There was a professor up here who was teaching solar dynamics to some undergraduates, and he went [mumbles] and then he said "...and so you write down this." And a student said, "I don't understand why you write that down." And he answered, "It's obvious. It can't be anything else" Then trying to uncode it, to explain to the student is almost impossible. You say, "but how can it be anything else? It has to be like this. And no, no, no, you can't do that." But that's an intuition. It's a bit like driving your car. After a while it becomes natural that you don't suddenly shoot off to the left. "Stay on the road, its obvious." That I can't tell you about. Some people have that in pure mathematics. Some people have that in engineering much better. 'Green fingers'...some people have that in gardening! Some people are very, very, very accurate calculators, so if they want to calculate something they won't make a mistake and absolutely push it to the end. Other people are not such good calculators but are very good at deciding what it is that should be calculated and sort of how.
>
> Reyes: Now that you mention car driving. One doesn't learn to drive a car just grabbing a manual. And you don't take off immediately. It's a process

[5]Quoted in Gleick (1992, p. 244).

> of being trained...
>
> Horgan: No, no, you have to work up to it. In the end, if you're a good car diver or a good air pilot the vehicle becomes an extension. It becomes like your hands and arms. It becomes an extension of your brain. *It's obvious...your hands are an extension of your brain, and the car is, and it's the same with all this theory. Very gradually these things become a mental extension. The mental landscape is increased in size.* (emphasis added)

6.5 Pure mathematics and applied mathematics, and their relationship to physical theory

Having discussed mathematical rigour as one factor that separates the general mathematical culture from that of physics, I finally wish to provide a more precise classification of the upper levels of the horseshoe diagram, so as to identify how the different 'micro-cultures' of mathematics and high theory can be identified. As one goes further away from the experimental and the phenomenological, there is a feeling that physics becomes more and more 'like' mathematics, to the point that it may sometimes be impossible to distinguish one from the other, just as it sometimes might be hard to pin down if 'the phenomenal' should be interpreted as theoretical or experimental argumentation, or as both, or perhaps as a new category. However, theoretician themselves are sometimes very careful to distinguish physics from mathematics:

> De la Peña: When I talk to mathematicians, I even grow envious, because the way they think is beautiful, but is completely different to mine. I can't think like they do. I clearly see the enormous difference that there is. Their purity of thought is very beautiful: the abstract way in which they construct things is very beautiful. But it does not correspond to the way of a physicist, who has to tie himself down to Nature.

There is a marked distinction between what types of 'objects' a physicist and a mathematician's work are supposed to deal with. Physics is 'empirically based', while mathematics' aim is not to explain real phenomena, but to deal with and construct abstract objects. But sometimes the division is not so clear. Superstring theory has been heavily criticised by many as lying outside of physics, and there is an underlying feeling in the general physics community that string theorists are in reality more mathematicians than actual theoreticians. This is because at the present moment, all of the predictions that string theory has made require experimental processes that lie outside of present experimental capabilities, or any that are likely to be available in the foreseeable future. Still, string theory has in its origin the idea of unifying different pieces of theoretical physics into a broader, universal mathematical framework. In this sense it is not pure mathematics; it is 'tainted' by a necessity to answer to 'the world', even if a relevant experiment is still impossible to carry out. Mondragón, who once worked in the field commented:

> Mondragón: Obviously people who do strings need a lot of mathematics; they're practically mathematicians but maybe other people from different specialties do not need, say, to know all of topology and differential geometry.

They may be "practically mathematicians" but this only means that they are not really mathematicians. Mondragón echoed De la Peña's appeal to the role of Nature in theoretical physics as opposed to mathematics:

> Mondragón: For me, physics can only be physics if it describes Nature. And Nature is those processes that we observe and measure. The other parts, mathematics or mathematical physics...Well, there was a very famous physicist W[...], who used to call them "impure mathematics" because they are not completely mathematics. For me, I do theoretical physics because although my models may be abstract, they do have to describe a physical reality that you have to measure and compare with experiment. For me that is what makes it physics, the theoretical part of these mathematical models.
>
> People do things where they don't compare. People in strings have certain physical parameters but they are much more abstract. I like more direct stuff. That is what makes it more physics, rather than mathematical models. [...] You look at how the Standard Model [of particle physics] was built

> and it was built phenomenologically: this symmetry works here, this one doesn't, etc. That is how physics is done. That is how physics is built.

Some theoreticians are highly suspicious view of the kind of physics that is 'too much' like mathematics, to the point of reinforcing the idea that one ought to be careful of becoming a mathematician:

> De la Peña: Those mathematical schools produced this way of seeing things, to the point of seeing mathematics as the only method of physics. I insist, mathematics are a very valuable auxiliary, an indispensable tool, even a research tool, a research method; but it is simply one method among others, and it has to be submerged in physics. If it is not submerged in physics, things usually go wrong.

Between pure mathematics and phenomenologically minded theoretical physics there is a hybrid land of neither pure-physics nor pure-mathematics, of which superstrings seem to be the most visible case nowadays. Perhaps superstrings really has in a way become mathematics and is no longer physics, but even its staunchest opponents have admitted that the developments made by strings have had enormous relevance in other areas of physics and even in other sciences:

> Mondragón: During the first stages of string theory, they thought that they understood everything [...] and it didn't happen. Other possibly interesting things happened that their calculating obsession blinded them to, and the realisation came later. They saw phenomenological calculation stuff as a lesser thing; I got to experience it as a student. [...] My own opinion is that it is very interesting, but it is still far from being the ultimate description of Nature and the physics of space-time. Nevertheless, there are connections between Hawking radiation and strings, and things like that, which make the theory intriguing from a mathematical standpoint. It is probable that the new astrophysical and cosmological measurements will find this contact with physical phenomena that they are lacking. In my opinion, even if it were not the description of the physical nature of space-time, it has given mathematical results applicable to other fields of

> knowledge that have proven extremely interesting. For example, these are tools in field theory that I mentioned, which were developed from superstring theory. There is also a lot from conformal superstring theory that has been applied to biophysics, in membranes, precisely because it involves similar extended objects. A new application is being developed with the Maldacena duality, to apply his development in string theory to a very concrete problem: the chromodynamic vacuum; a very hard problem because it is non-perturbative.

These two sides of string theory were commented on in an interview with Tong, who has actually made phenomenological string theory predictions:[6]

> Tong: There's two different ways you can approach string theory. One is the kind of thing you always hear about in the news, that this is the Grand Theory of Everything, it's solved all of physics. Hawking says it's M-Theory which is basically the same thing as string theory. I was a little bit on that and my work on cosmology was trying to use string theory in that context. Can we get experimental predictions down to something? But there's another side of string theory which doesn't get pushed very much because it's probably not as sexy which is that you can use the theory and the mathematical tools that come out of string theory to solve other problems in physics, and sort of not use it as a Grand Theory of Everything but just as a very useful tool [in] some other things like high-temperature superconductivity, various aspects condensed matter physics. So lately I've been playing with that side. [...]There's been lots of similar work that's really led to revolutions in mathematics by thinking about string theory. There have

[6]"You could actually, in principle, see strings stretched across the sky. So there's been a long speculation in cosmology of cosmic strings, which aren't necessarily the same types of strings as in string theory, but one type of cosmic string is the string of string theory. If these things were ever detected you could do further observations to know what kind of string you're dealing with and so maybe you'd see it. With Eva Silverstein and Mohsen Alishahiha I came up with a theory of inflation that has to do with extra dimensions and objects moving in extra dimensions and it gave rise to observable predictions for the Planck satellite. The Planck satellite launched over a year ago. It's three million kilometres away at the moment taking data and in five years we'll know if my model of the universe is right or wrong. I think all of these are long shots. So then what do you do? You've got a theory; you can't falsify it. It looks very good. It looks very compelling but if you don't know it's the right theory, you know, you've got to be wary about that." [From interview with Tong]

> been ideas in cosmology; I came up with a few; particle physics; and now in the last ten years it's spat out this other tool— very surprising, nobody expected it— but it's a tool to understand strongly interacting systems; this means when the interaction is between things like electrons and are so strong you can't possibly hope to solve the equations.

6.6 A typology of physics epistemically distant from experiment

This chapter has examined both mathematician's and theoretician's accounts of their own practice, and shown how 'rigour' has a higher status in mathematical than in physics practice. Yet both intuition and rigour form part of mathematical and theoretical practice, so it is not there that a demarcation between mathematics and physics can be found. It *is* the hallmark of pure mathematics that mathematics is a discipline that is separated from the nuances of the 'real' world; the stuff of pure mathematics are abstract structures, which may or may not have connections to the material world through scientific application. One can talk of 'impure' mathematics, or applied mathematics that although it doesn't have as primary concern the application of their techniques to a specific problem, it does create mathematical frameworks that are clearly geared towards applications in some domain. There are mathematical physicists who although they may be producing work that is arguably similar to applied mathematics still has the solution of a physical problem 'in the back of its mind'. It is because string-theory has this property that despite the strong criticism that it has subjected to, it must still be seen as a subset of physics. However unsuccessful or fruitless it may be in its phenomenological aspects, string theorist do after all dream of creating a great physical theory. It is also physics in the sense that its mathematical apparatus has branched out to explore other phenomena, and so the ties with the material world are still present, if not in the original manner. Furthermore, it remains closer to the physicists' idea of mathematics, where rigour plays a much lesser part.

In a review of Smolin (2006) and Woit (2006) and their critique of superstrings, Johnson (2006) refers the reader to the internet page Postmodernism Generator, which upon loading generates an academic-looking essay which the page authors describe as "completely meaningless and was randomly generated."[7] The 'essay' is an electronic version of the famous Sokal hoax that started the Science Wars. The interesting bit comes when Johnson continues:

> With a tweak to the algorithms and a different database, the Web site could probably be made to spit out what appear to be abstracts about superstring theory: "Frobenius transformation, mirror map and instanton numbers" or "Fractional two-branes, toric orbifolds and the quantum McKay correspondence." Those are actually titles of papers recently posted to the arXiv.org repository of preprints in theoretical physics, and they may well be of scientific worth — if, that is, superstring theory really is a science. Two new books suggest otherwise: that the frenzy of research into strings and branes and curled-up dimensions is a case of surface without depth, a solipsistic shuffling of symbols as relevant to understanding the universe as randomly generated dadaist prose."

All physicists are familiar with these sort of 'ultra-theorertical' but not-quite- mathematical physics papers. During my physics research years, a favourite game for my closest collaborator and I was to invent ridiculous sounding articles on the Casimir effect and then go through *arXiv* or *Web of Science* and find the closest fitting example; most of the time we were never too far off from at least one existing article.

Take for example an article by found after searching a physics database for the keywords 'Casimir' and 'torus' by Huang (2001), "Casimir effect on the radius stabilization of the noncommutative torus". The abstract reads as follows: "We evaluate the one-loop correction to the spectrum of Kaluza-Klein system for the $\phi 3$ model on $R, d\times(T\theta 2)L$, where $1 + d$ dimensions are the ordinary flat Minkowski spacetimes and the extra dimensions are the L two-dimensional noncommutative tori with noncommutativity θ. The correction to the Kaluza-Klein mass spectrum is then used to compute the Casimir energy. The results show that when $L > 2$ the Casimir energy due to the noncommutativity could give repulsive force to stabilize the extra noncommutative tori in the cases of $d = 4n{-}2$, with n a positive integral."

Dauntingly titled, these articles are commonplace in the 'mathematical' Casimir effect literature. Although I can understand to some extent what most of the terms mean

[7]`http://www.elsewhere.org/pomo/`

(e.g. 'a one-loop correction' is associated with a particularly simple Feynman diagram; the 'noncommutative' is describing the kind of algebraic structure of the system that is being analysed; a 'torus' is a geometric figure that in three dimensions can be visualised as a doughnut but then can be generalised to a higher dimensional space). The Casimir force can be seen to arise in any space where there is any kind of background field, which in this case must be the Kaluza-Klein system. If an object is put inside the field, it 'disturbs' the field and the energy of the field changes (this change of energy is the origin of the Casimir force). Thus, the author is putting a mathematically well-defined object in a weird but mathematically well-defined kind of space, and calculating the energy of the field associated with this geometry. The author then analyses cases for particular numbers of dimensions, and finds that some of them may give rise to repulsive forces (which is curious, because Casimir forces are generally attractive).

What is the point of such a paper? Experimentally, there are currently only two configurations where the Casimir force is measurable: the force between two plates, and the force between a plate and a sphere. Any other configuration proves too unstable, or experimenters have yet to set up an experiment that can probe them. Of course, we have no access to generalised experiments in $1+d$ dimensions, nor to a Kaluza-Klein system! And yet, the author is calculating something that in a very abstract form takes 'inspiration' from real phenomena, and could, maybe, possibly, perhaps, if we dream, if we dream a lot, one day find applicability in the real world.

Although many physicists may contend that these mathematical exercises might be mostly irrelevant to the profession, they nevertheless keep being produced and published. Mathematical physics may not find a lot of direct application in the material world of physics when it is of the more pedestrian variety, but when it touches upon more edgy or exciting-sounding topics, it seems to be quite popular. Superstring theory is such a field, where the contact from the phenomenal world seems very far away, but not entirely severed. That is has also fed technique to both physics and mathematics while not being the one or the other puts it in a favourable, if strange, position.

Though it may resemble mathematics, mathematicians like Jaffe and Quinn do not see physical theory as a branch of mathematics, but as a field of expertise that may influence mathematics, but is not quite at the same level. Different areas of theory therefore have a different relationship with the empirical content of the work. The different levels in which mathematics enters into physics and forms specialist domains is encompassed

Table 6.1: Relationship with experiment, mathematics and physics

Micro-culture	**Relationship with experiment**
Pure Mathematics	Irrelevant
Applied Mathematics	Relevant, but fortuitous
All Physics	Essential, but in different degrees depending on the micro-culture

in Table 6.1.

Theory can be fractioned depending on how the phenomenological 'enters' practice. For many theoreticians, the appearance of 'the world' in their work seems almost as fortuitous as for mathematicians, as happens in most of string theory, but is still kept as a far away, or separate goal. On the other hand, some physicists seem inclined towards the belief that one should not only play around with mathematical objects that may represent physical objects, but one also needs to explain and understand real phenomena, and gain a conceptual understanding of it. But perhaps the best argument for tracing a division between mathematical physics and more 'phenomenologically oriented physics', and for considering them distinct micro-cultural entities, is the disbelief in each other's work that sometimes arises, and particularly of a mild resentment sometimes shown by theoreticians concerning mathematical physicist's work being taken as the highest form of 'true' theoretical physics:

> Mondragón: It's like those theoretical physicists that live in their own nirvana, in their ivory tower; they're really convinced about what they do. These so-called mathematical physics... In my studying history I've come to the conclusion that the term 'theoretical physics' is an abnormality, per se. I think it originates in the twentieth century as a consequence of the overwhelming success of quantum mechanics, and of linear algebra. [...] And within theoretical physics, there's that second abnormality, 'mathematical physics'. [...] Anyway, I think there are two kinds of us. I do feel more of the pragmatic kind, although if you look at the everyday stuff I do it's just as abstract as the people who do mathematical physics [laughs].

CHAPTER 7

Collective tacit knowledge in theoretical physics

My first article"Yun-Qi Kingdom— Fundamental One" obviously has very well put today's PHYSICS into the DEAD END. Students, teachers and professors who have a solid background of mathematics and/or physics can easily examine what I say by yourselves.

If you think my first article does not sufficiently shake down today's MATHEMATICS, my second incoming article "Yun-Qi Kingdom— Fundamental Two" will clearly show you that the MATHEMATICS we have used for centuries is also considerably and lawlessly WRONG.

Please do not use today's knowledge as your prejudices to cut immediately what I have said down. Try to investigate it first. I hope one can beat Yun-Qi Kingdom down by using today's science and knowledge, but I'll be afraid to say that NOBODY CAN.

-Chang Yu Wang, from the 'Yun-Qi Kingdom' webpage (2011)

7.1 The 'collective' in 'collective tacit' knowledge

Collins' third type of tacit knowledge is collective tacit knowledge, and contrary to the other two types it lies not in the individual but in collectivities. Collective tacit knowledge is of particular importance to sociology because it is, by definition, a phenomenon that has roots not in persons but in societies. This does not mean that collective tacit knowledge does not enter the individual mind, but that to 'tap into' the pool of collective knowledge (tacit or not) it is necessary to first be socialised into a collectivity. I will introduce the role of collective tacit knowledge by looking at examples of people who practice physics outside of professionally sanctioned settings, so-called 'cranks'. Cranks are interesting because they are individuals who can be proficient in the technical aspects of mathematics or theory, but who have not been immersed in the physical or linguistic world of professional physics. Because of this they have not been able to 'tap' into the discipline' reservoir of the collective tacit knowledge, and are isolated by 'legitimate' physicists.

7.2 Crank science: an illustration of (missing) collective tacit knowledge

Berry is the current editor of the *Proceedings of the Royal Society A*, one of the world's most prestigious and oldest scientific journals. Being a high-profile scientific journal, the number of articles submitted for publication is much larger than the number that can actually be published (approximately seventy to eighty percent of papers are rejected). Along with the papers rejected for technical reasons, Berry mentioned that is a large number of submissions that can be placed in the 'crank' category, papers that are so outlandish or poorly written that they can instantly be seen to have been made by a non-physicist:

> Berry: The other problem is that with a journal of such prestige we get a lot of junk; people who aren't scientists with this new theory. Often retired engineers seem to be prone to this grandiosity. You instantly know if a paper is junk, but on the other hand you have to take into account that the author is serious, and has thought a great deal about what they've done— they're always men, never women— and so I encourage my board members to write not just 'This is rubbish', but helpful comments just why the paper is being rejected.
>
> Reyes: How do you know when a paper is rubbish?

> Berry: Many people have written about this. You instantly know. You know because their references are Einstein, Schrödinger, Heisenberg. There are lots of different ways. I shouldn't say this, but often they use bizarre typefaces, or there's a lack of contact with large current literature on all these problems; often, very often — almost always — a lack of concrete quantitative predictions; a failure to test whether this new theory of theirs will reproduce what is already known. When I get papers that claim to reproduce quantum mechanics I often write, 'Well, how would you explain with your theory... how would you explain the Casimir effect, or the Thomas precession?' And they don't know what that is. They've never heard of it, and that silences them. In areas that I know about that's what I do. You have to explain things that are already understood before you go off... It's complicated. There is a little literature on people who describe ways to recognize crazy papers. One just recognizes.
>
> Reyes: I suppose that there must also be a smaller amount of papers from members of the community that are not worth publishing.
>
> Berry: Of course there are. This is the normal rejection of papers; not deep enough or just publishing a small incremental advance. You see, there's this famous devastating criticism by Pauli of a paper, 'It's not even wrong.' That category is different. These are not cranks; they are mediocre. Sometimes, people think that if something is a correct result and has never been published before, this is a good enough reason to publish. And I have never been so unkind as to say what I'm going to say now to an author, but I'm waiting for a sufficiently unpleasant author to say this to him. 'Three hundred and thirty seven multiplied by seven thousand four hundred and sixty two equals...whatever it does.' This is undoubtedly a correct result and I'm convinced it has never been published result, but it's not worth publishing. I've never said this, but it makes the point. It can be correct, it can be original, but that doesn't mean it's worth publishing.

A number of high-profile theoreticians and mathematical physicists have written 'crank indices', shedding a comical light on the pathos that cranks evoke in mainstream physicists.[1] All of the these references are more or less part of theoretical physics 'pop culture' rather than formal documents— with the exception of Langmuir they are only available as internet resources— and give us an insight into a physicists 'everyday' world-view. Though they may be informal, they are not meaningless. 'Crank' is a category recognisable by physics researchers far and wide. It may even be a professional pitfall that can have severe consequences should a physicist come to be branded as one.[2]

A lot of the crank-characteristics in these popular satirical pieces deal with the crank's eccentricity: comparing oneself to great scientists, having persecutory delusions, ego-mania, etc. Siegel (2011) for example mentions that a telltale sign of being a 'quack' is inclusion of personal attacks claiming that "the establishment always rejects new ideas," or declaring "I knew you wouldn't listen, you scientists are too arrogant and closed-minded." In a similar piece, Baez (1998) awards 'crackpot points' for each use of the phrase "self-appointed defender of the orthodoxy" and particularly high scores for "claiming that the 'scientific establishment' is engaged in a 'conspiracy' to prevent your work from gaining its well-deserved fame, or suchlike."

But as Berry mentions, cranks in science are not necessarily lunatics or hopeless eccentrics; they can be "retired engineers" who are out of touch with the field and exhibit "lack of contact with large current literature". Most importantly, cranks are *not* identified by whether they are right or wrong. Scientists disregard crank theories without even noticing what the theory may say. This is not because cranks are wrong, but because in many cases even the questions they pose are 'illegitimate'; despite being what one might call conceptually eccentric, many of the most esteemed theories of contemporary physics are not regarded as crank science. As an interviewee mentioned, "if you hear people talk about many-worlds [theory], you'd think they are as mad as anything!" Ideas such as dark matter, dark energy, black holes, superstrings, inflating universes and

[1]See for example Baez (1998), Siegel (2011) and 't Hooft (2003); see Mulkay & Gilbert (1982) for an analysis of scientific humour as a resource for sociological investigation.

Langmuir (1953) discussed multiple examples of what he termed 'pathological' experimental physics in a well-known conference, but not all the examples that Langmuir chose constitute 'crank' science as defined here; some of these experimental projects are more akin to 'fringe' science, where an experimental group that started out as legitimate physics spun off into a closed research program. Langmuir's examples also include 'pseudoscience' as often denounced in the popular press by scientists (e.g. UFOs).

[2]'Crank' is defined by the *OED* as "a person with a mental twist; one who is apt to take up eccentric notions or impracticable projects; esp. one who is enthusiastically possessed by a particular crotchet or hobby; an eccentric, a monomaniac." The crank scientist, and particularly the crank physicist, fits this definition.

particles that 'exist' yet are literally undetectable are all part of respectable science.

In his popular account of crank science and pseudo-science, Gardner (1957, p. 8) supported the view that cranks are separated from experts not by the truth of their theories but by the degree of their "scientific competence". Gardner points out that cranks come in many varieties, from the "stupid, ignorant, almost illiterate men who confine their activities to sending 'crank letters' to prominent scientists" to others who are "brilliant and well-educated, often with an excellent understanding of the branch of science in which they are speculating." Gardner asserts that the most salient characteristic of cranks, whether idiotic or genial is that "cranks work in almost total isolation from their colleagues. Not isolation in the geographical sense, but in the sense of having no fruitful contacts with fellow researchers." Gardner explains that although pre-Renaissance science may have allowed for the working of individual scientists, because of the emergence of modern science's vast and complex network of cooperative dynamics nowadays "it is impossible for a working scientist to be isolated."[3] He also points out that it is not the outlandishness of a crank's hypotheses that a community refuses but that rather, because of his isolation, "the crank is not well enough informed to write a paper with even a surface resemblance to a significant study."

Cranks tend to believe that through individual efforts they have found a fracture in a scientific worldview to which they have no legitimate access; but at the same time, that their views should be placed on par to those of a legitimate expert. Crank papers often pose problems that are not 'legitimate' problems for the community which is expected to read the paper, for example casting doubt on a particular experimental result that is seen as unproblematic, or a foundational theoretical result that has been uncontroversial for decades. They fail to recognise the sociological dimensions of scientific knowledge, championing a highly individualistic viewpoint of scientific practice. The infamous crank journal *Progress in Physics* for example states in its 'Declaration of Academic Freedom':

> A scientist is any person who does science. Any person who collaborates with a scientist in developing and propounding ideas and data in research

[3]'Crank' is defined by the OED defines as "a person with a mental twist; one who is apt to take up eccentric notions or impracticable projects; esp. one who is enthusiastically possessed by a particular crotchet or hobby; an eccentric, a monomaniac." The crank scientist, and particularly the crank physicist, fits this definition.

> or application is also a scientist. The holding of a formal qualification is not a prerequisite for a person to be a scientist.

Naturally, this sort of declaration marks the crank not only as an outsider, but as a hostile outsider, and it renders their likelihood of success even smaller.

7.3 Knowing the literature, knowing the field

Some of the characteristics listed as telltale signs of crankness may initially seem odd, for example, when Berry mentions that "their references are Einstein, Schrödinger, Heisenberg." An outsider to physics would suppose that to cite the great masters, rather than secondary work, would be an obviously good thing. In many disciplines, sociology for example, this is often the case. Physics is different in this respect. Although foundational papers are sometimes cited (for example, Casimir force papers invariably begin by citing Casimir's foundational 1948 article), meaningful debates are not centred around these papers but on the present literature. Citing the foundational literature *in extenso* by itself, without reference to modern developments, is a sign that the author is not familiar with what is 'common knowledge' in the field, or in terms of the previous chapter, what ought to be treated as conventional (relational) tacit knowledge.

More surprising is that even recognised experts may sometimes, if they're not careful, behave like cranks. Scientists may have been away from the field long enough to no longer recognise what's important even when they have achieved high proficiency in other areas:

> Reyes: We have a saying in Spanish…it's a bad translation; it sounds better in Spanish…, 'The Devil is clever not because he's the Devil, but because he's very old'.
>
> Berry: Yes, I like that, but if you're in the world of proverbs, there's another one in English which is, 'There's no fool like an old fool.' So you just choose. These old fools…are such people who have had good scientific careers and who in their retirement think that they can look at some problem they've not studied before and they can make some contribution to it, and they can't. I'm now dealing with an author who I don't know how to deal

> with because he was a very senior person, made some useful contributions in mathematical physics. And now he thinks that he has an understanding of Cosmology, which is fundamentally new, and he doesn't, and it's rather awkward to deal with because these are not people who…in their lives, they have not been cranks. They think that because they were clever— and are clever— they can understand problems without getting into the details. That's the old fool, so to speak.

The prime characteristic of cranks is therefore *no*t that they lack technical proficiency, for they can be brilliant ex-scientists, but that they lack the skills to realise what is and what is not relevant in the professional physicist's social landscape. For example, they are unable to 'keep up with the current literature', even when they may have been contributors to that very same field years before:

> Berry: You have to be very careful. In my own life I've been very wary of this because choosing not to work in fashionable subjects, but having been fortunate enough to do work that has made some subjects fashionable, I then have a dilemma. Quantum chaos is an example. I hardly work on it now, but if I did, I would be having to keep up with the literature when I already…long ago we worked out the fundamental principles. I'm delighted when they're applied, and I've made some applications myself, but I don't want to continue along that line, and there are other people just as clever as me now doing this work. So I change, every few years I change what I do. But I'm very careful when I do that. The areas I go into are organically related to the ones before, because I'm really aware of the danger of trampling into a subject and making pronouncements about it simply because I've done good work in the past.

7.4 The changing world of theoretical publications: *arXiv*

The problem of becoming a crank through lack of continuous of contact with 'the field' has been exacerbated by the way the mechanisms of diffusion of papers has changed in physics in the last few decades. In today's physics even the last published article in a specialty's preferred journal can be old news, and today it is the electronic pre-print service *arXiv* and not the paper journals which are the primary sources of published material. Although printed publications are still a measure of academic prestige, the de facto standard for keeping up to date with published work is no longer the peer-reviewed journal, but the *arXiv* website.[4] As Tong described:

> Tong: Oh, I don't look at them [journals]. I don't think anybody does. I suspect it's different for experimenters. The only place I've looked at journal papers lately are experimental condensed-matter papers. String theory, high-energy physics, it's all on the *arXiv*. The first hour of every day I spend reading what's interesting in the *arXiv*.
>
> Reyes: I was talking to Graeme [Mitchison] yesterday, and he said that a lot of theoreticians would be very happy if journals just stopped existing and everything was on the *arXiv*. I don't know if that's possible but...
>
> Tong: I think it's true. A few years ago a lot of the top theorists just stopped publishing in journals. The only real reason not to, and I think it's an important reason, is sort of testing people's...how you test the quality of somebody's work. A lot of that still depends on how many papers you've published in journals. I think the peer-review process is sometimes worthwhile, but not often. And often you write a paper and the person says, "this is a really nice paper," and sometime you get an excellent referee's report back where he says, "you've got to think about this or that." That's somehow a bit more satisfactory. [In *arXiv*] the way your bad reports happen is by ignoring a paper, a complete lack of interest in it. But certainly if a paper is interesting it gets informally refereed. And it's very odd. [...] There was a big breakthrough a couple of years ago on understanding membrane theory, Bagger-Lambert theory. This paper came out by Bagger and Lambert about three months before a group of people, different people around the world suddenly sort of had been playing with these and had ideas, and then it just became exponential. So it got to the point where a paper would be published on the *arXiv*, two weeks later I'd get to referee it, but it was already out of date by then because there had

[4] For a general description of *arXiv* and its relationship to printed journal publications in contemporary physics, see Gunnarsdóttir (2005).

> been thirty other papers in the intervening two weeks pointing out mistakes in that paper even though it had infinitesimal progress, so you were not quite sure whether you should accept that paper or reject it. It had been useful but it was already superseded by that time. I think that's an example of how the *arXiv* works really well, with this informal refereeing process and the whole movement just pushing things forward.

'Bad' or 'irrelevant' papers that make it to *arXiv* are thus filtered in the same way that cranks are filtered out of mainstream physics: they are simply ignored. The more so since unlike traditional journals, *arXiv* is only slightly moderated; 'endorsement' has recently been introduced into *arXiv*, whereby an article can be posted on the website only if it is 'endorsed' by an already recognised poster, thus limiting the probability that cranks, pranksters or other community-wise unwanted posters could upload their papers. This is far from peer-review in the traditional way. As Tong mentions, an article becomes 'interesting' in *arXiv* when it is talked about, when it causes open controversy, and when recognised scientists generally engage with it, either positively or critically. Quoting Andy Warhol, *arXiv* success follows the dictum "don't pay any attention to what they write about you. Just measure it in inches."

> Tong: In the old days you could often read rebuttals to rebuttals to rebuttals in the journals. Somehow with the arXiv that seems to have stopped a little bit. It happens occasionally... a comment on this paper, and then a rebuttal to the comment. But if an idea is boring, if a paper is boring, it won't get much attention. If a paper is interesting everybody wants to read it, everybody wants to work on it and very quickly any mistakes come to the fore. One good thing about the arXiv is that this can turn over very, very quickly. If you write an interesting paper within a couple of weeks there can be sponsors to that paper. People try to build on your theory or generalize the idea, and people also say, "the guy did this wrong."

An *arxiv* publication's reputation is upheld by 'word of mouth'; by the amount of attention that the community invests in it. Cranks, being at the fringes or completely outside of scientific circles where the talk is being carried out, have little or no access to this knowledge. The way that theoreticians in practice acquire this collective knowledge is to meet and talk to other theoreticians, that is, to immerse themselves in the language of their practice.

7.5 How to gain collective tacit knowledge: identifying legitimate problems

Cranks negatively illustrate the importance that interacting with recognised members of a community plays in modern theoretical physics. But how does one in fact gain entry into a community of physics experts? The first thing most senior scientist makes a novice pupil do is to get acquainted with the relevant modern literature and understand it as far as the young student's skills allow. This allows, as described in the previous chapter, for the student to become acquainted with the technical relational tacit knowledge that is intrinsic to the field, but it also permits the student to identify which are the 'good' articles simply by having the supervisor point them out as a worthwhile read. It also allows the student to develop a feeling for what the important discussions in the field are. So important is the ability to identify what the relevant problem is that displaying this skill is in fact often cited as one of the indicators that an aspiring theoretician has actually become a competent expert, for example:

> Mondragón: There is of course a training period. Which skills do you need? Curiosity is necessary, I think. You need a true desire to understand Nature, or a part of it. It can be through solid state, materials science, elementary particles, or astronomy. That is a pre-requisite. I think you need to exhibit logical thought, and a certain mathematical ability that can vary in scope, depending on your specialty. Obviously people who do strings need a lot; they're practically mathematicians but maybe other people from different specialties do not need, say, to know all of topology and differential geometry. You need some specialty-dependent training that is partly the mathematics of your subfield, and partly the phenomenology that you want to describe with these mathematics. After that you have to do things on your own. You have to find a problem and to solve it. Not necessarily alone; maybe it'll be in a group. But you need a problem. Once you're at that stage where you're looking at a problem that no

> one has solved with your own eyes and your own ideas from a theoretical perspective...that is when you can consider yourself a true theoretician.

Before arriving with a senior scientist, the apprentice may have digested tomes and tomes of textbooks, but these will not tell him what the 'allowed' topics are. If starting from the textbook (as one may suppose many of Berry's retired engineers do) there is high probability of descent into crank territory. But even very experienced scientists, recognised experts in their field, need to exercise the utmost caution when moving into other fields of specialised expertise, like Berry mentions. Mitchison described how his relatively recent transition into the world of quantum information was enabled by the close guidance of mature experts.

> Mitchison: What happened was that I went to some lectures that were being given in Bristol and on the train I met one of the lecturers and we started talking and he became a friend, Sandu Popescu, and then I met Richard Jozsa, and he had posed in New Scientist a problem which he had thought interesting about counterfactuality in quantum mechanics. I started working on that and corresponding with him, and he very kindly helped me through incredible gaps and misunderstandings and eventually wrote a paper together. I sort of had the benefit of private tuition from two very brilliant people, which I think is just incredibly good luck. I'm sure if I just sat down and tried from a beginner's position to get into research in the subject it would have been quite difficult.
>
> Reyes: Why do you think it would have been difficult?
>
> Mitchison: I think that you only understand what are the important ideas if you're a beginner by talking to somebody who's been in the field a long time. It's not that there are things that you can't know on your own. It's that there's a kind of perspective and a weight that has to be given to it, to the important ideas. Half an hour of talking to somebody who knows the subject completely changes the landscape, and I have the benefit of Richard's email correspondence and we worked out ideas by me making mistakes and he correcting them.

> Reyes: Which were the important parts of the landscape which he showed you, which would have been harder to find on your own.
>
> Mitchison: One thing is to understand how classical information theory looks in a quantum context, and to realize what the questions were that he thought important to try to understand. I would never have seen the correct way, for example, to think of quantum computation, which you do using a number of models, and the way that he moved between them was very educative. There are a lot of tricks and techniques and viewpoints. The same is true of Sandu; he's an incredibly good teacher. One reads a paper and says— as I did, because the paper was incredibly badly written — 'how can people produce such a trashy paper?' and he said, 'actually, that's one of the best ideas!' and I realised I was being put off by the wrong thing. Those sort of things are vital!

Using the Collins & Evans' criteria of expertise, Weinel (2007, 2008) has come up with the concept of 'domain specific discrimination' as a collection of skills that allow experts in an esoteric field to recognize 'legitimate' scientific controversies. The four criteria that Weinel proposes make up domain specific discrimination are all applied by theoreticians to recognise crank papers:

1. Conceptual continuity in science — Weinel asserts that "if a body of work or a claim is too far removed from science so that there appears to be no intent to make it part of science, it cannot be said to be part of a scientific controversy." Many crank papers do not try to 'add' to existing scientific theory, but rather attempt to overturn the entire edifice of physics (see quote at beginning of chapter) or of a stable piece of theory.

2. Expertise of the originator — Weinel writes that " [expertise] criteria can be operationalised by examining the social networks in which the person making the claims is embedded. To the extent that these include networks of relevant experts, then there are grounds for believing the person making the claim knows what she or he is talking about." Although some cranks may be practicing scientists, they are characteristically not part of the scientific network which they area

attempting to enter. Thus they are seen as treading on an expert area to which they do not legitimately belong.

3. Constitutive work — this category is connected to understanding the scientific 'form of life', in Fleck's sense, which for Weinel is made up of activities that 'look like' science to scientists. For example, the lack of constitutive work in cranks can be seen in their inability to understand that empirical, mathematical or theoretical results are collectively and not individually supported in science. Cranks fail to understand that to practice science it is not enough to have a bright idea, but to play the sociological game of convincing expert communities that the idea is clever, and that this does not go against the 'spirit' of science. Cranks for example would fail to understand the importance of virtual empiricism for theoretical practice, and the chains of trust that have to be kept unless a full migration towards phenomenological work is carried out.

4. Explicit argument — although it is probably only the minority of crank papers which eventually get read by scientists, this does not mean that scientists are unable or unwilling to engage in argument with unusual ideas; change in science would be impossible otherwise. It is likely that a theoretical proposal that covered the previous three categories would at least pass the preliminary refereeing stage of a journal, although eventually it could be rejected for technical reasons.

7.6 Socialising into a new field

Lacking the possibility of keeping prolonged contact with a recognised expert close at hand, an alternative way to initially map out what a 'new' field looks like and thus obtain partial social immersion is to attend introductory workshops or 'schools.' For example, the "2010 Casimir, van der Waals, and nanoscale interactions school" at the École de Physique des Houches, one of many centres around the world devoted to these kind of events, was described as follows:

> The Casimir, van der Waals and nanoscale interactions school is organised to provide its participants a complete overview of the state-of-the-art in the field of the Casimir effect and of some of the related research areas. The

> school will offer a pedagogical introduction to the theory of the Casimir effect, from the origin to the questions which nowadays focus interest of physicists, chemists and engineers. A series of lectures will describe the major issues and how they are related to innovative methods of Casimir force measurements, thus illustrating the trends for future activities in this vibrant research field. Several distinguished lecturers coming from other areas of research will report on the basic concepts on which their scientific community is growing.

The conference was attended by both old members of the Casimir community (most of them standing in as "distinguished lecturers") and by physicists from other fields as diverse as gravitational wave detection and AFM microscopy that were interested in understanding the relevance of the Casimir effect within the context of their work. Schools such as these are not just easy substitutes for reading the literature, but rather ways to interact directly with established members of a field. Scientific conferences and seminars always include references to and explanations of the 'relevant' recent articles and results, which act as social 'pointers'. They also allow direct contact with the experts, and serve to identify who else is considered an expert straight from the horse's mouth. If a recognised expert cites a particular theoretical result or an experiment in a conference, the novice can assume that this is proper reading and reference material. If a paper is discussed in a school, just the fact that it's mentioned is a pointer that this is accepted work up for discussion.

The same thing happens with the topics of research covered in schools and seminars. In order to establish dialogue with and within a community, a physicist must first recognise that all problems are not created equal. There are problems that interest communities, and other that do not. Attending a conference with 'experts' gives one a fair sampling of 'what is being talked about' in the field and what constitutes a problem of concern for the community of relevant experts. Many of the scientists that attended the Les Houches school were senior theoreticians from 'related' areas who were looking for opportunities to network directly with Casimir physicists, or to check out if the topics being researched into could be relevant to their work.

7.7 What is a 'good' theoretical citation?

Although in principle theoretical results are said to be open to reconstruction by anyone who possesses sufficient knowledge of the mathematics involved, as was argued in the previous chapter, most theoretical results that the individual physicist comes across in the literature remain unchecked unless one has a very particular interest in them. When one is forced to cite a paper outside of one's field of expertise, it is often done without exhaustively checking the entire paper's argument, and often times one is led by other heuristic indicators. For example, one can choose to cite a paper that has a high citation rate. But this does not afford a general method of identifying the 'proper' sources to cite.

On a personal note, I was able to experience the importance of collective tacit knowledge during the writing of the first published paper I was involved in, which dealt with an acoustic classical analogy to the Casimir effect. The group I worked in had refined previous work on the topic by extending the applicability of the original theoretical setup for this effect.[5] Previous authors had shown that given two parallel plates inside a gas filled cavity, if one introduces a background white-noise field a force between the plates arises that pushes or pulls them apart depending on the parameters of the background field and the distance between the plates. However, in acoustics one normally deals not with forces, but with pressures (forces per unit area). The pressure itself is usually derived from a fundamentally 'higher' theoretical quantity, the so-called wave stress-tensor. In our original submission, we referenced a paper from a flagship journal in acoustics (the same one we were submitting to) that contained— so we thought— a nice historical discussion of the concept of acoustic pressure and cleared up some conceptual issues that arise from how one defines what the pressure is starting from the stress tensor. The refereeing process was rather fast and the article was almost immediately accepted for publication, pending the resolution of some recommendations made by the referees. One in particular stood out in reference to the this paper, which I reproduce directly the referee report:

> ...reference[15] may not be the best reference for the acoustic radiation pressure, Eq (11). This article dealt with a silly polemic that surfaced in

[5] See Barcenas et al. (2004).

> the 70's over an idiotic misunderstanding of radiation pressure in one-dimension (see Lord Rayleigh, Philos. Mag. 3, 338-346 (1902); Lord Rayleigh, Philos. Mag. 10, 364-374 (1905)). Using reference [15] may just be an invitation to renew this silliness. Alternative references may be L. D. Landau and E. M. Lifshitz, Fluid Mechanics, (Pergamon Press, London 1959), Sec. 64; C. P. Lee and T. G. Wang "'Acoustic radiation pressure," J. Acoust. Soc. Am. 94, 1099-1109 (1993)

The author we had cited had several dozen articles just in the very journal we submitted to, with this very article having been cited more than 60 times, and yet the referee's words made it clear that we had committed a small blunder by citing an article that had at some point stirred up deep disagreement in the field. Although the main author of our paper had been working in acoustics for some time and had other publications in this journal, it was not his main topic, and we decided to drop the reference and stick to the suggested references. So why did we not identify that this was the 'wrong' article to cite? The cited work's credentials seemed impeccable; the article itself was highly engaging, and there were no outstanding evident flaws in his argument. It was something that only a deep 'insider' to the world of this brand of acoustics would know; a piece of information left unstated in the formal literature, but critical when it came to the reception if the article by the specialised peer-reviewers.

Thus a good theoretical argument in this case depended not only on giving a technically correct argument, but also on supporting this argument on *acceptable* sources. Our source might have been acceptable by citation standards, but the controversy it had caused in the past was not visible to us outsiders. We lacked the 'inside' collective tacit knowledge to identify this. Only an expert could point this out to us, as all outside standards pointed to this as being a 'good' paper. In this case, the collective tacit knowledge was made explicit by the referee, but only because the issue came up, as otherwise it would have remained something only an expert in acoustics would have known to avoid.

The core characteristic of collective tacit knowledge is thus that it allows individuals to walk about, perform and talk *effectively* within a social sphere, and has much in common with Bourdieu's concept of *habitus* as "a sense of the game", and as "the social game incarnate", which is imbued by tacit knowledge in the forms of "strategies" and "dispositions". Thus, says Bourdieu, 'The constraints and requirements of the game, although they are not locked within a code of rules, are imperative for those, and only those, who, because they have a sense of the game's immanent necessity, are equipped to perceive them and carry them out".[6] Cranks in this context are the uncomfortable strangers at the edge of the physics playground who are eager to join in the game, but demand that the rules be changed for their own benefit, or a *private* understanding of how the game ought to be played that does not match the habitus of physics.

7.8 Historical cranks

Historical studies of reactions to Einstein's theory of general relativity show that both the existence of cranks and the modern physics status quo's reaction to them have not changed much since at least the early 20th century. Reviewing a recent monograph on opposition to relativity since the 1920s by Wazeck (2009), van Dongen (2010) notes that "the actions of many of Einstein's opponents resemble those of the thinkers now often referred to as, in perhaps an all too derisive manner, 'crackpots'. It thus appears that this phenomenon is at least as old as the existence of institutionalised science, which arbitrates authoritatively what is, and what is not, sound scientific practice and established truth; crackpots, with their own unshakable beliefs, in the end rather deny that authority than give up their ideas."

Regarding the closure of scientific circles to 'illegitimate' critics of relativity such as the engineer Arthur Patschke who wrote numerous pamphlets condemning relativity, Wazeck (2011) writes that "non-academic researchers like Patschke announced public lectures, submitted essays, and tried to establish contact with Einstein and other leading scholars in order to warn them— as well-intentioned colleagues— of the falsehood of the theory of relativity and to convince them of the veracity of their own scientific worldviews. Patschke and others like him were often simply ignored; in other instances, it was patiently explained how their criticisms of the theory of relativity had completely missed the mark", in the same way that modern cranks are ignored by academic physics.

[6] P. Bourdieu, in Lamaison (1986).

7.9 Making up worlds

I have argued that the salient feature that distinguishes cranks from professionally sanctioned physicists is that cranks, because of their lack of enculturation, lack the crucial pieces of collective tacit knowledge that would make them part of the community. I finally wish to discuss the case of a person that although not a professional physicist, has managed to convincingly portray the language of physics and of science in general to a wide audience with much success. I was led to interview the author Ian McEwan by a comment made by Berry,

> I've just read— this is required reading for you— the new novel by Ian McEwan called *Solar*. I've just read it this weekend and I'm waiting for all my colleagues to come back to tell them to read it, because that's about a particular individual doing theoretical physics and...it's not very complimentary about the guy. It's a novel, so the human weakness is all there and it's full of tremendous insights. I mean, he really...he's not a sociologist, but *novelists understand people* and *they get into other people's worlds* and I found it very uncomfortable because you know...I was wondering all this while why I was finding it uncomfortable and I realised that there aren't many novels about theoretical physicists, and I suppose anyone who is reading a novel about the world in which they live— a lawyer for example reading a novel about bad behaviour among lawyers— would probably feel uncomfortable if the novel is a hit. I found myself feeling guilty for the bad deeds of this guy in the novel, guilty on behalf of theoretical physicists. It's very interesting to read. It's also funny. He exaggerates a little bit here and there; it goes over the top but I forgive him that because it's such a good read. (emphasis added)

McEwan is well-known for his extremely realistic portrayals of scientific characters, and these feature often in his novels. Importantly, McEwan does not shy away from using technical language in his work, but the manner in which he acquires the ability to use this language *convincingly* is particularly interesting. I interviewed McEwan, who received help from Mitchison for *Solar* in which the main character is a theoretical physicist,

> McEwan: I needed help not necessarily on the ideas, but *on how physicists would say them*. There were a number of times when Graeme Mitchison said to me, "oh well that's correct, but we don't say that. We don't say... we don't keep saying 'General Theory'." There was phraseology like that that mattered to me. And you do have to separate those out. They are simply matters of fictional realism, to get those things right. I did a lot of research for a novel called *Saturday*, I don't know if you've read that. I had to describe surgical procedures of a neurosurgeon and *I followed closely* one particular surgeon [for two years]. When I showed him what I'd written most of his comments were of the kind, "yes that's right, but that's not what we say." I think that is broadly one of the important areas where one does need the input of the profession, because *you can get all the facts correct* but it's the way in which people represent these facts to themselves within the profession that *you can never invent*. [...] Yes, the turns of phrase that a surgeon might use to an anaesthetist; the shorthand that develops when people work in a team, you could never invent. And likewise in a community like the community of theoretical physics, if they refer to an equation, they might do so in a shorthand or in the way that *a layman could never guess at*, and those little things are very important. I had many letters from surgeons. (emphasis added)

McEwan perfectly illustrates the point made by Collins (2010, p. 124) that one must make a distinction between exhibiting mechanical technical skills or 'plain' proficiency in the usage of language, and exhibiting these in a "socially sensitive way". Some cranks are extreme examples of individuals with high technical skills and perhaps high proficiency in the language, but minimal or no 'social sensitivity' to professional physics. McEwan, through immersion in the for-of-life of scientific language, is the other extreme: a master in social sensitivity, with no technical skill at all. The wide acceptance of McEwan's work (he proudly stated how he had only received one complaint about the language in the novel from a theoretician, and the complaint was brushed off by other physicists who scrutinised it) and the wide rejection of crank's highlights the relevance of 'social sensitivity' to the social world of physics and the acquisition of collective tacit knowledge as an *intrinsic* part of theoretical physics practice.

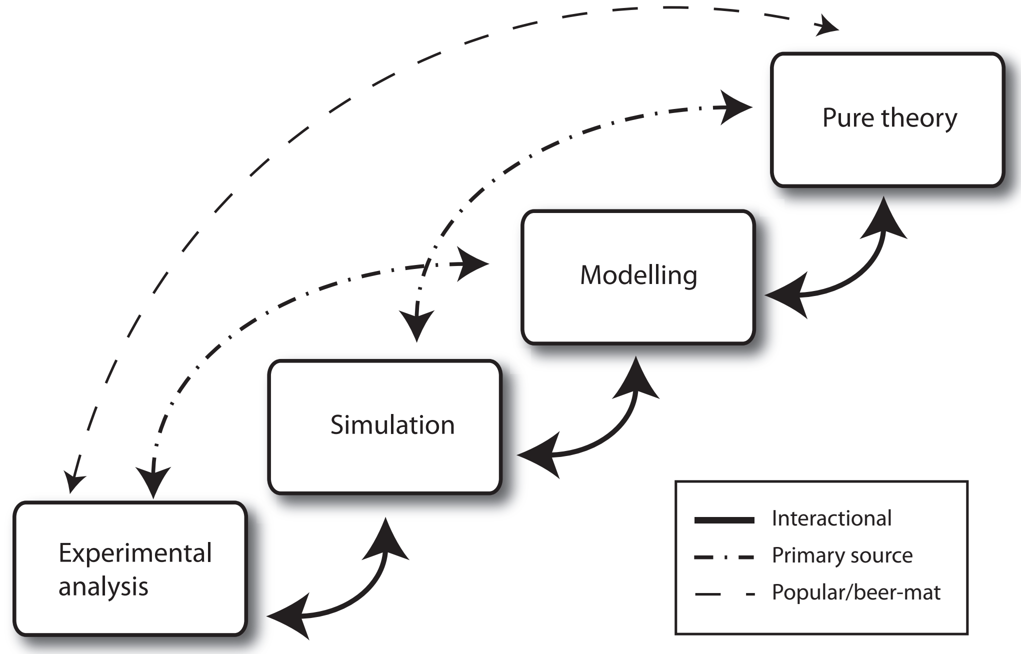
Conclusions

Isn't it clear to you, a mathematician, that only differences — differences in temperature, only thermal contrast — only in them is there life.

— Yevgeny Zamaytin, from 'WE'

The problem of communication and the heterogeneity of physics practice

Although I have tried to draw up as comprehensive a picture of theoretical physics as possible, the present work does not exhaust all the dimensions of the field that could be sociologically probed, nor have I reached the maximum possible depth in the micro-cultures that were examined. One of the most fascinating features of modern physics is that it has become such a wide field of knowledge, and with so many connections to other sciences, that for one person to be intimately acquainted with all its topics seems impossible, such is the heterogeneity of practices and expertises that make up the field. It is this heterogeneity that gives rise to the main research question of the thesis, that is, to find a solution to the problem of communication between dissimilar micro-cultures in physic; within a homogenous social group, the problem of communication is not a problem, by reductio ad absurdum. As such, the problem of communication can be put into a wider context beyond the sociology of science not as a feature of theoretical physics, but as a question regarding the integration of heterogeneous social groups



in general. The final answer given here, reproduced from Chapter 4, is the following figure:

I reiterate the main characteristics of the horseshoe diagram as the principal way to address the problem of communication in physics:

1. Physics is a field of knowledge within which one can observe the division of labour into a number of 'micro expertises' (the lozenges that make up the horseshoe diagram).

2. These expertises comprise esoteric fields of knowledge that display epistemic autonomy from each other.

3. The expertises are defined as esoteric domains and are delimited by localised sets of tacit knowledge.

4. Although the expertises are autonomous, there is a flow of knowledge across their boundaries. The flow can be modelled using the SEE approach.

5. The flow of knowledge follows the pattern in the diagram: expertises which are epistemically/ culturally/ sociologically close to each other interact through the

elaboration of interactional expertise bonds. Expertises that are distant from each other establish interaction through lower-level knowledge.

6. Physics is thus a 'disunified' science, but the disjoint micro-cultures remain connected by SEE-type interactions.

Physicists in particular tend to develop interactional expertise with colleagues within their own 'families' of practices (i.e. within high-theory, phenomenology, and experiment). Although sometimes they choose to migrate to different families or expertises, this is not common, as it requires the apprehension of a new form-of-life. Even considering that a 'culture of physics' underlies all of these traditions, the picture of physics that I wish to project is one of multiplicity in the whole; a multiplicity that is 'bridged' in practice by personal knowledge transfer, and by sociological trust. These esoteric domains can therefore be practiced 'for their own sake' while at the same time being socially connected by SEE type interactions to the wider web of physics.

Heterogeneity as a feature of modern physics

As sociologists have recognised since the 19th century the transition from 'traditional' to modern societies was parallel to the transition from social and cultural homogeneity to a state of heterogeneity. The fragmentation of physics into its distinct micro-cultures is the reflection of this 'modern condition', and could perhaps be historically traced along the classical sociological lines of the division of labour, specialisation, etc. It would be a worthwhile investigation to ask, for example, if physics has always been as fragmented as it is today. My guess is that one would find it has not, but however interesting, a more complete answer is beyond the scope of this work. The heterogeneity that has been posited since the first pages of this work is not a trivial one. The sociological partition talked about here implies large amounts of cultural incommensurability, distinct forms-of-life, autonomous styles of knowledge creation, large amounts of unshared tacit knowledge, and localisation of social networks. True, one must not forget that beneath all these micro-cultures there is an underlying 'culture of physics', but as I have tried to show the incommensurability and autonomy arise despite these ubiquitous cultural elements being there. Collins & Evans' model of expertise provides a precise way to talk about how social tendrils are stretched across these divides.

The beauty of Collins & Evans' model answer to the problem of communication is that it openly allows incommensurability to flourish as part of a stable social system. By positing that interactional expertise necessitates that one group become 'parasitic' on the tacit knowledge repository of the other, it permits these two groups to function independently. The figures of 'interactional ambassadors' , 'interactional translators' and 'special interactional expert' allow the communication across boundaries to occur. Crucially, they do not blur the boundaries, but operate 'with one foot in each side'.

8.1 Tacit knowledge in theoretical physics

I have also illustrated how tacit knowledge in all its forms enters theoretical practice at the interface between the lozenges, but also how tacit skills shape the everyday practice of theoretical physics. Tacit knowledge is seen to play a Janus faced role in relation to communication in physics: collective tacit knowledge allows communication inside an esoteric group to be more efficient, but limits access to the group for outsiders; somatic tacit knowledge allows for skilful, virtuous elaboration of theory, but is impossible to transmit to other individuals; collective tacit knowledge makes physics a living, growing discipline, but severely bars outsiders from joining its ranks. Although mathematical logic and proof construction at times serve as mechanisms to diminish the tacit dimension, it is impossible to do away with it completely.

Nothing of what has been said in this work about physics should be particularly new to physicists. In their daily lives, physicists— most of them— are quite aware of the sociological dimensions of their practice, for they must take professional decisions based on these existing social circumstances. However, most of these social circumstances are invisible to outsiders, and being part of the form-of-life of science may even remain hidden behind the veil of the collective tacit for the practitioners themselves save for exceptional circumstances. The task of sociology can then be visualised as being strongly oriented towards bringing the collective tacit to the fore.

Regarding the somatic tacit and intuition, the situation for sociologists is complicated by the fact that it is not linguistic immersion but practice that permits its acquisition, and thus the sociologist is left only with second-order accounts for illustrative purposes. Nevertheless, here philosophy comes to the sociologist's aid, allowing one to transpose what proficiency means in particular domain and translate it into another.

An important point of SEE is that all human beings, simply because they are social animals, have proficiency in at least their native culture and their native language. Thus by examining our own manner of using language, we can understand what it is like to be proficient in carrying out general skilled activities, in having gained somatic skills in other domains. In this, theoretical physics is also no different to other domains of human activity, with the exception that the set of skills for theoreticians lies almost exclusively in the realm of the purely mental. Nevertheless, as Polanyi argued, the difference between the purely mental realm and the physically-embodied one is insignificant in terms of how knowledge is structured, acquired and often times also experienced; as Lakatos (1980, p. 14) pointed out, it is after all only an *assumption* "that there is a natural, psychological borderline between theoretical or speculative propositions on the one hand and factual or observational (or basic) propositions on the other".

8.2 Heterogeneity in modern societies

The comprehension of forms-of-life through the acquisition of localised tacit knowledge is the heart of Collins & Evans' expertise model. The analysis offered in this work is an example of this structure, albeit in a precisely bounded cultural setting, the world of theoretical physics. But one can refract the answer given to the problem of communication in theoretical physics back onto a wider sociological context, and ponder on the kind of answers that sociology has given to similar problems in our intrinsically and increasingly heterogeneous modern societies.

I began this thesis by placing my analysis of theoretical physics and its cultures and forms-of-life within the larger problem of the fragmentation of cultures which social studies of science has named *the problem of disunity*. To recapitulate, this problem focuses on how it is possible for sciences such as physics, made up of diverse epistemic elements and forms-of-life, to form a coherent community where cooperation and knowledge exchange is produced *despite* the existence of cross-cultural boundaries. My answer is that the existence of trust and of interactional experts is the two-fold solution to the problem of disunity. There are many practical problems intrinsic to complex modern societies where the expertise model could find application — not as a natural solution to these problems, but as a means to *reframe* current debates within an approach that can deal from the start, with the multiplicity of forms-of-life coexisting side by side.

The problem of cultural autonomy of ethnic minorities, for example, is a challenge to many if not most nations; whether it is due to the cultural diversity that is a historic legacy of modern nation-states such as is faced by societies up and down the American continent, or the challenges posed by the increased immigration influx nowadays faced by Western Europe. The sociology of ethnic minorities poses a challenging situation, one where problems of cross-cultural fragmentation, communication and political integration seems to lead to terrains where Collins & Evans' model and the extrapolations made by this work might seem applicable. In the remaining section I will focus on considering two such problems where cultural and form-of-life fragmentation have arisen in two very different socio-political settings.

8.3 Heterogeneity: two challenges

Cultural heterogeneity is a problem that is faced by all modern societies. Since the Zapatista Army of National Liberation's 1994 declaration of war on the Mexican government, the problem of multiculturalism became highly visible in the nationwide political agenda to Mexican society at large. The anti-neoliberalism and anti-globalisation Zapatista movement was based in the state of Chiapas, one of Mexico's poorest and most ethnically diverse regions. One of its hardest fought demands was the political recognition of this diversity, along with the legal protection of 'indigenous rights' of the Chiapas native peoples.[1] In 1996 the conflict received partial resolution with the presidential signature of the *San Andrés Accords*, which granted autonomy, the right to self-rule and recognition of the indigenous rights of to these ethnic minorities. After months of negotiations, the Mexican Constitution was subject to modifications based on the San Andrés agreement (modifications to which the Zapatistas, international NGO's and human-rights agencies were extremely critical, as they saw them as a watered down version of the agreements, and an insult to a legitimate political movement and a grave national problem).

Despite the Zapatista's discontent, the Mexican Constitution now explicitly acknowledges the 'pluricultural' nature of the Mexican nation, and grants indigenous communities the right to 'autonomy' regarding their traditional forms of self-organisation,

[1] See Stahler-Sholk (2007) for a historical summary of the Zapatista movement's development.

as well as the right to self-regulation of their social, economic, political and cultural activities. This, however, was a quick political fix to a complex problem that has not received a solution. Chiapas and its indigenous people still lag behind the rest of Mexico in terms of development and wealth, probably as much as before the constitutional amendments.

The Zapatista movement is now widely recognised as a social movement that not only attempted to bring recognition to the cultural and financial plight of the indigenous peoples of Chiapas, but as a Latin America-wide struggle that has faced off ethnic minorities and underprivileged social groups against a neoliberal national projects that have, in these groups' views, only deepened amplified their already dire situation. But as the Zapatista movement developed, it found necessary not only to struggle against the Mexican government's neoliberal agenda, but also to find new definitions of autonomy that would allow the movement to circumvent neoliberal versions of autonomy such as were seen to be reflected in the Constitutional changes. One of the most interesting facets of Zapatismo has been the redefinition of autonomy as a concept that recognises the special situations of localised ethnic groups but that does not confuse the boundaries between autonomy and secession from the Mexican nation at large.

Nash (1997) has pointed out that this balance between autonomy and political cohesion has been a recurring topic in Latin American history, as nations attempt to balance the heterogeneity of their societies with the development of 'national projects: 'Nation building has often been assumed to require the assimilation or even annihilation of marginalized cultures. The pluricultural base of Mexican society (like that of many other Latin American states with large indigenous populations) was seen as an obstacle to modernity". Thus Zapatismo not only fights for autonomy, but struggles internally to find new boundaries for autonomy, new definitions of what it is to have a plural society where no micro-culture is left behind, but where traditional cultures retain this newly-developed autonomy while still being 'fully Mexican'.[2] As a Zapatista quoted by Nash puts it, "we are all Mexicans, but each lives and feels his or her Mexicanness differently". In fact, the Zapatista movement demands a re-examination of what 'Mexicanness' implies, how the Mexican form-of-life can be compatible with a traditional indigenous form-of-life — in fact, a large number of them, since the indigenous cultures of Mexico are themselves deeply differentiated.

[2] See Mora (2007).

Zapatismo thus offers a cultural locus where cultural complexity is in constant struggle with a vision of cultural homogeneity embedded in a national project. A similar situation, albeit under different circumstances, is faced by many First World nations where immigration has confronted cultural minorities suddenly immersed within a larger homogeneous culture. The scholarly literature points to two generalised political strategies to face this situation, the so-called *assimilation perspective* versus the *multiculturalism perspective*.[3] The former is a homogeneity-centred view in which the end result of immigration processes is ideally the full subjugation of ethnic identities to a central national project, while the second places diversity and the maintenance of ethnic minorities' identities above any national identity. Lambert et al. (1990) have identified these differences in the attitudes of both immigrants and natives in the United States of America and its 'melting-pot', integration-centred politics, and the Candian multicultural politics which are in fact seen as a defining quality of 'Canadianness'.

Currently, with an increasingly interlinked world — politically, economically, and in physical mobilisation terms — Modern Western democracies in general, particularly in Europe, have recently been forced to rethink their policies regarding the heterogeneous composition of their contemporary societies in the face of a political claim of multiculturalism's 'failure'. In general, there has been a widespread backlash against the idea of a pluralistic society by the right-wing political status quo. Nearly a year ago, German chancellor Angela Merkel stated the following regarding the failure of the 'multikulti' public policy of immigrant integration in Germany,[4]

> We are a country which at the beginning of the 1960s actually brought guest workers to Germany. Now they live with us, and we lied to ourselves for a while saying that they wouldn't stay, and that they would disappear one day. That's not the reality. This multicultural approach, saying that we simply live side-by-side and are happy about each other, this approach is failed. Utterly failed.

Merkel also added

[3]See Lambert et al. (1990). The dichotomy is also sometimes posed as the debate between *integration* versus *assimilation*; see Harles (1997).

[4]"Merkel says German multicultural society has failed", BBC news, 17 October 2010, `http://www.bbc.co.uk/news/world-europe-11559451`.

> We should not be a country either which gives an impression to the outside world that those who do not speak German immediately or who weren't raised speaking German are not welcome here. That would do great damage to our country. Companies will go elsewhere because they can't find the people to work here anymore. That means that the demand for integration is one of our key tasks for the time to come.

This problem, particularly regarding Muslim cultural integration, has received similar answers from other European heads of state. David Cameron for example declared that[5]

> In the UK, some young men find it hard to identify with the traditional Islam practiced at home by their parents, whose customs can seem staid when transplanted to modern Western countries. But these young men also find it hard to identify with Britain too, because we've allowed the weakening of our collective identity. Under the doctrine of state multiculturalism *we've encouraged different cultures to lead separate lives*, apart from each other, and *apart from the mainstream*. [...] We've even tolerated these segregated communities behaving in ways that run completely counter to our values. So when a white person holds objectionable views, racist views for instance, we rightly condemn them, but when equally unacceptable views or practices come from someone who isn't white, we've been too cautious, frankly... frankly even fearful, to stand up to them. [...] This hands-off tolerance has only served to reinforce the sense that *not enough is shared*. (emphasis added)

French president Nicolas Sarkozy also declared multiculturalism "a failure", adding that, 'the truth is that, in all our democracies, we've been too concerned about the identity of the new arrivals and not enough about the identity of the country receiving them". [6] Spanish ex-prime minister José María Aznar declared during a speech at Georgetown University that he believed that "multiculturalism is a big failure", furthermore adding, "I'm against the idea of multiculturalism. Multiculturalism divides

[5] "State multiculturalism has failed, says David Cameron", BBC News, 5 February 2011, `http://www.bbc.co.uk/news/uk-politics-12371994`.

[6] "Sarkozy joins allies burying multiculturalism", Reuters U. S. on-line edition.

our societies, debilitates our societies, multiculturalism does not produce tolerance, nor integration. And this is one of the reasons of the great failures in several European societies at this moment". [7]

Is homogeneity intrinsic to social integration?

Can the expertise model say anything at all about problems like the ones posed by these heads of state? Is cultural heterogeneity really "one of the reasons of the great failures in several European societies at this moment", as Aznar believes? Does positing a society where autonomous cultures coexist side by side necessarily lead to a failed society? Should we accept that the only path to 'integration' is homogenisation of the cultural landscape? Are ethnic minorities in the UK as segregated from British modern culture as Cameron claims? Is it really impossible to live side-by-side and be happy about each other unless we all 'speak' German? Are the indigenous ethnic groups of Mexico as autonomous and free from cultural homogenisation as the Mexican Constitution claims them to be, or have they been forcibly integrated into a modern society that has erased a major part of their indigenous cultural inheritance despite putative protection from the law? What is it, in the end, to 'integrate' a minority group into the wider society into which it is embedded by forced physical contiguity? According to the changing tides of European politics, integration seems to imply the adoption of a 'parent' culture in deterrence of the immigrant's native one. In practice, the Mexican state has left indigenous cultures to their own devices to fend for itself within a society that repudiates their 'backward' traditions.

Rethinking these problems in terms of Collins & Evans' expertise model, I believe, offers another option, which is the possibility of cultural understanding within diversity, and not one of cultural dominance, nor of cultural fragmentation. This opens up the possibility to think in terms of heterogeneity and homogeneity simultaneously: heterogeneity in practices, but within one linguistic universe-set that is bound by very general cultural practices. This does not mean that a maximal-heterogeneity choice

[7]"Aznar asegura en Georgetown que el multiculturalismo 'divide y debilita a las sociedades" ("Aznars assures audience in Georgetown that multiculturalism 'divides and weakens' socities", El Mundo España, 27 October 2066, `http://www.elmundo.es/elmundo/2006/10/27/espana/1161909568.html`.

is intrinsic to this model, only that it allows a reformulation of the problem itself. It also allows one to pose the question — the empirical question — of whether or not interactional expertise-type integration has occurred or not in particular societies relative to their most 'isolated' groups, and whether multicultural projects are per se the sources of modern European social project 'failures'. As Ribeiro (2007a, p. 562) notes, issues regarding interactional expertise are directly relevant "to the much larger problem of how cooperative projects between two social worlds, forms-of-life, paradigms or cultures that involve linguistic, cultural or conceptual discontinuities can be made to work".

Have the 'young Muslim men' of Britain really no understanding of British culture? Collins and Evans' imitation game methodology could, in principle, give an answer to such a question by exhibiting whether or not young Muslim men have any interactional expertise in relation to the wider British culture. If, as is my gut feeling, one were to find that young Muslim men in the UK are good interactional experts relative to the wider British culture (for are they not fully immersed in, at the very least, the linguistic aspects of British culture which is the crucial element in developing interactional expertise?) then the 'multiculturalism is a failure' thesis could be, perhaps, re-examined by asking whether the problem is not in young Muslim men's lack of understanding of white British culture, but in the white British political establishment failing to understand any other cultural possibility that is not a domineering version of its own white British version. In the Mexican context one might find that despite the political class' rhetorical stress on the importance of incorporating ethnic autochthonous minorities into its 'national project', imitation games could show these groups to be completely segregated from the general socio-cultural milieu. Again, I do not propose that the expertise model can or will provide an immediate answer to these extremely complicated problems, but only that it gives a strong analytical tool to frame the problem of cultural heterogeneity differently, just as in this work it allowed the problem of communication in a fragmented science to be reframed from a different perspective.

Let us turn back to the case of physics and the problem of communication. I have argued that the micro-cultures of physics are fragmented — culturally fragmented — despite the fact that they are 'embedded' into the universe-set of physics as a whole. Yet how is this different from the fragmentation of modern Western societies that nowadays face the problems of multiculturalism? There may or may not be significant differences, but whatever the case physics and its micro-cultures offer at least one example of a collection of fragmented cultural groups that nevertheless remains 'integrated' despite a large degree of heterogeneity and autonomy between its constituent cultures. I have tried to show how there is ample participation from all the micro-cultures in the grand scheme of physics, which is creating knowledge about the physical world even when physics micro-cultures in the horseshoe diagram only have low-level knowledge regarding those cultures that are far away. By any standards, physics is not only pretty good at what it claims to do in creating knowledge, but is also one of the most successful intellectual enterprises in modern human history in terms of material accomplishments.

It would be all too easy to fall into the 'naturalistic fallacy' of suggesting that because physics is well integrated by interactional expertise and trust-based mechanisms, these same mechanisms of integration could be applied to any social context and that this would render good results. For example, there is the most particular situation that in the grand scheme of physics the micro-cultures have mostly equal epistemic grounding amongst themselves: experiment, phenomenology and high-theory share, so to speak, equal amounts of epistemic power and though one may be more popular at particular times or spaces, the distribution of power seems to be well spread out. Modern Western democracies, despite their titles, do not generally exhibit widespread balances of power. Young Muslim men in the UK can hardly be said to have equal access to places of political power, which are mostly populated by upper-class white British men. In Mexico, a country that only became anything like a proper democracy a decade ago, having the skin colour and appearance of an indigenous people still carries with it deep and cruel social stigmas in a society obsessed with the white European phenotype. It would be terribly naïf to propose that lack of mutual cultural understanding is the crucial problem in every case, but it is worth asking whether the solution of bulldozing cultural differences is the only possible answer to integration.

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❧This thesis was typeset in LaTeX using the XeLaTeX engine, using a Garamond typeface. This thesis definitely contains nuts, and was manufactured in several nutty environments. ❧