Crossing Boundaries: Contexts of Practice as Common Goods

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Since Thomas Kuhn introduced the idea of incommensurable paradigms as contexts of inquiry, contextualist analyses of science have abounded. However, the ways in which local and disciplinary contexts of inquiry involve *common goods* remain largely unexplored. Collaborative research practices clearly depend on common goods: on the one hand, scientists work together in order to increase the good of public knowledge; on the other hand, cooperation within and across research teams depends on shared epistemic values and conceptions of good science. Common goods thus structure the very practice of research. In this paper I argue that a contextualist analysis of scientific common goods casts light on the challenges of scientific communication that led Kuhn to the idea of incommensurable paradigms. I begin by clarifying two core ideas: disciplinary ethos and common good (secs. 1, 2). Drawing on research into collective intentionality, I then analyze ethos as a common good (sec 3). As a distinctive approach to the rhetorical challenges of communication within disciplines (sec. 4), contextualist, common-goods oriented analysis raises fruitful questions for the rhetoric of science, albeit without invoking the troubled specter of incommensurability (sec. 5).

1 Disciplinary ethos: from paradigm to common good

Decades of multidisciplinary research in science studies has by now rendered the idea of a disciplinary ethos non-controversial. In effect, R. K. Merton's "institutional ethos" of science has undergone a process of substantive compartmentalization, most famously at the hands of Thomas Kuhn. More precisely, in Kuhn's notion of the scientific paradigm we can see key features of a disciplinary ethos. As he eventually clarified the notion, a paradigm involves a "disciplinary matrix," a shared stock of generalizations and equations, techniques and methods, models, and values, along with concrete "exemplars" that teach novices how to use elements of the matrix to frame and solve problems (Kuhn 1996, 181-87). For Kuhn, paradigms distinguish not only disciplines but also different historical periods within a discipline, inasmuch as significant theoretical development occurs through paradigm shifts.

Kuhn developed his conception of a paradigm not only to explain the rhythm of normal and revolutionary episodes in the history of science, but also to account for the difficulties scientists experience in communicating during periods of revolutionary transition, when new and old disciplinary paradigms confront each other. As various critics have pointed out, however, Kuhn's fixation on the alleged incommensurability of paradigms had unfortunate consequences: it rested on a problematic meaning-holism and diverted attention away from his more significant insight into the structure of scientific practices (Zammito 2004; cf. Laudan 1996; Nickles 2003; Rouse 2003).

In rejecting Kuhn's incommensurability thesis, one need not deny the importance of the disciplinary ethos for structuring communication and argument in the sciences. In fact, the rhetoric of science has further illuminated the notion of scientific ethos, linking it with Aristotle's proof from character: the social psychology of persuasion presupposes a shared ethos, whose elements align with Kuhn's analysis of the disciplinary matrix. Picking up on Kuhn, Lawrence Prelli has developed a detailed typology of the technical substance of the scientific ethos: the goals, issues, and lines of argument that members of the science community employ in their construction ("invention") of persuasive arguments (Prelli, 1989).

The above developments, I propose, converge on the following picture of scientific practice: in coming to understand themselves as competent members of a discipline, scientists master the ethos or "matrix" distinctive of that discipline, the shared stock of theoretical and methodological commitments, standards of good evidence, assumptions about the proper aims and interesting lines of research, along with beliefs about the epistemic values that good scientific practice realizes, such as consistency, quantitative accuracy, explanatory scope, and so on. In mastering the ethos, they acquire the means to establish credibility with members of the discipline and fashion persuasive arguments. To be sure, some elements of the ethos are shared with other disciplines, at some level of abstraction. But much, perhaps most, of the theoretical and experimental know-how that guides actual practice within a discipline is specific to that discipline, indeed much of it is specific to the particular laboratory or research project, and must be acquired on the job—to be sure, with the help of one's broader disciplinary know-how and background.

Consequently, if the disciplinary ethos fosters cooperation among members of a discipline, then it can also hinder communication across ethoi and thus, interdisciplinary collaboration. In such collaborations, members of different disciplines bring quite different background assumptions to their joint project. Given how deeply rooted these assumptions are in members' very identity as scientists and lived sense of good science, failures to communicate should not surprise us. This is the kernal of the problem that Kuhn noticed. However, both the successes and failures of communication, I propose, can be traced back to the ways in which ethoi function as common goods for members. Because scientific practices realize epistemic values, they are experienced by members as ways of doing good science; because such values are open-textured, their determinate meaning and force partly depend on the lived experience of fruitful scientific practice. I thus propose that the difficulties of communication that Kuhn wanted to get at are better described as obstacles to communicating the goodness of a specific research practice, experienced by members as a common good. To understand this proposal, we must explore the structure of the common good in scientific practices.

2 Common goods in scientific practices

Because scientific practices are structured around disciplinary ethoi, we can analyze those practices in terms of the common goods they involve. The notion of the common good has received the most attention in political philosophy, where it tends to stand in some relationship with political justice. However, to understand how the notion of common good functions in science in various ways, we do better to approach the matter not through political philosophy but rather by analyzing the concept itself. Broadly speaking, any good that is pursued or enjoyed in common by members of some group merits the title "common good," at least putatively. To get at the notion of common good, I focus first on the good and then turn to commonness.

I take the term *good* as referring to desirable or beneficial objects, states of affair, and capacities or what we might call conditions of being, such as virtues or excellences. These goods can function either as means or ends, or as both in the case of intermediate ends. For the purpose of analyzing social practices, it also helps to distinguish between putative and actual goods. Something is *putatively good* (i.e., an apparent good) insofar as one desires or pursues it; this putative sense of good allows us to explain collective action by linking it with its goal. But whether or not that goal is *actually good*—in fact beneficial to anyone—poses a further question, which refers us to a second, normative sense of "good." The idea of an actual good, that is, grounds the normative judgment that one may, or even ought, to pursue a given end because of the benefits its achievement will bring. Taken together, these two interrelated senses yield the idea of goods as reasons for action: members of a group pursue some goal—a putative good—because they judge its achievement will be actually beneficial (perhaps to them, perhaps to others).

Although one might—and some scientists do—value scientific knowledge for its own sake, as a final end of research, such knowledge functions for many of us today as an intermediate end, a means we value for the sake of further ends, for example, improved technology (which in turn is a means to further social and personal ends). In either case, the disciplinary ethos counts as an instrumental good insofar as that ethos allows scientists to cooperate in the production of public knowledge. But for committed scientists, the disciplinary ethos cannot be merely instrumental, in the way that money is merely a means to acquiring something that otherwise has no relation to money. The reason is that entry into a disciplinary ethos involves a cultivation of intellectual excellence, without which members cannot appreciate high-quality science or judge the adequacy of knowledge claims and their justification. In addition, the process of becoming a scientist not only imparts a set of intellectual skills but also tends to form one's personal identity. Although some scientists might pursue a career in some field merely as a routine job, I suspect that many, particularly those at the doctoral level, enter a particular discipline because it fits with their personal interests and aspirations. The ethos for them cannot be merely instrumental, bound up as it is with their sense of who they are and want to be, what they hope to achieve in life.

The *commonness* of common goods in science appears at first glance to be a straightforward matter. After all, we usually regard scientific knowledge as a good that benefits society as a whole, at least overall and on average. And the various components that make up the disciplinary ethos—the standards of evidence, background knowledge, epistemic values, etc.—are shared by members of the discipline, for having that knowledge in common with other members is just what it means to be a member of a discipline. Finally, in virtue of their shared ethos, scientists are able to pursue knowledge in common—knowledge is a *shared end*.

However, debates over the constitution of common goods reveal a more complicated picture, particularly when we look into the relationship between the common good and individual goods (see Rehg 2007). To say that a good is "common" can mean several things. In a merely nominal sense, two or more individuals might value or enjoy the same type of thing (e.g., ice cream), although they never share the same token (the same ice cream cone). The more interesting senses of commonness concern the ways in which a *single* thing, state of affairs, or capacity can be a common good. On the one hand, that good might be "decomposable" or aggregative, such that its goodness reduces to its goodness for each of the individuals who enjoy it.[1] Public goods, like clean air, are good because of their benefits for individuals; a pool of profits divided among a group of investors is aggregative. On the other hand, a good might be an irreducibly social common good—what I'll call an irreducible common good— such that it

[1] I allow here for the possibility that aggregative and decomposable common goods are distinct kinds: although in both cases the common good involves benefits for individuals, the distribution of that good might not always involve simple aggregation; see Rehg 2007.

benefits the group precisely as a whole, in the way that winning a baseball game, say, is something good (i.e., victory) that we attribute precisely to the team as a team.

To summarize, we may speak of "common goods" in scientific practices in various ways. Three senses strike me as relatively straightforward:

> (a) Common good as shared end, thus a putative common good: because scientists pursue research by acting together in research teams, knowledge functions as a common good in the sense of collective end.
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> (b) Public (intellectual) good, thus a decomposable actual common good: insofar as scientific knowledge is published and widely known, its goodness consists in its intellectual benefits for individuals. Notice that knowledge has a status akin to a public good in the technical sense, namely, one person's intellectual enjoyment of that knowledge does not compete with or exclude its enjoyment by others. (Technological and economic benefits are a different story.)
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> (c) Aggregative actual common good: Scientific practices benefit their members individually in various ways, including aggregative economic benefits (e.g., a grant that pays the salaries of a research team).

A fourth sense, however, involves a more ambitious claim about collaboration:

> (d) Irreducibly social achievements (an actual irreducible common good): The products of team research are usually collective achievements, analogous to a team victory in sports, such that the achievement (as a good) cannot be attributed to any single member, but only to the group as a team.

According to fourth this sense of common good, the research achievement in science must be attributed precisely to the research team as a group of individuals *acting in concert*. To be sure, there are two ways this can come about, which raises further questions about the nature of excellence in the sciences.

On the one hand, the achievement might issue from a *decomposable division of labor*. In that case, each individual in the team carries out a part of the process that produced the achievement (the research outcome as a contribution to public knowledge), similar to the way that four of us, for example, might build a table: one person obtains the tools, lumber, and other material, another cuts the legs, a third makes the tabletop, and a fourth puts the parts together. The table is a collective achievement, properly predicated of the group, not of any one individual. However, we can subdivide the process into the discrete contributions of each member, each of whom contributes things or skills he or she possesses as an individual and can use or exercise as an individual. The members depend on one another, but only for the external conditions for the exercise of skill—for tools, material, and parts. On the other hand, there are some activities in which the cooperative process requires an *irreducibly social exercise of excellence*, as for example when two lumberjacks efficiently use a two-person band saw to fell a tree, or when two ballroom dancers move together in perfect harmony. In such cases, the skill of each partner depends on that of the other partner as its internal condition of exercise—indeed, neither person can carry out the activity at all except in synchrony with the other.

What sort of excellence does scientific practice involve—one that combines the discrete skills of individuals in a decomposable division of labor, or one that is irreducibly social? No doubt both play a role. I suspect, however, that differences in the success of scientific collaborations turn especially on aspects of cooperation that involve irreducibly social forms of excellence. Those forms, after all, seem to require a mutual attunement of minds in which members of the team work together in a way that surpasses the sum of the parts, as it were. Whereas divisions of labor appear fairly straightforward, a matter of careful organization, mutual attunement depends much more on the fortuitous mix of persons. When the mix fails, members are out of synch with each other and produce results that are less than the sum of the parts.[2]

3 Disciplinary ethos as common good: linking ethos and practice

This brings us to the idea of a disciplinary ethos as a common good. Again, the analysis appears straightforward at first glance. But here too we find interesting analytical challenges. The most obvious view is instrumental, which gives us a fifth sense of "common good":

(e) Common good as means: the disciplinary ethos is a means of producing knowledge.

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[2] A possible example would the discovery of the double helix. Judging from accounts (Watson 1968; Judson 1996, appendix; McElheny 2003), Watson and Crick seemed to work exceptionally well together, whereas Wilkins and Franklin had difficulty communicating.

Notice, however, that the ethos can function as such a means only insofar as members *collectively accept it* as good in the process of appropriating it through their training. Otherwise they will disagree on the aims, standards, and vocabulary of their scientific practice, and will not be able to cooperate in knowledge production. What such collective acceptance involves depends on how one understands the concept of collective intentionality. After introducing that idea, I develop a number of considerations that take us into a closer analysis of scientific practices.

(3.1) The notion of collective intentionality is controversial, and social philosophers have defended a range of views, some individualistic, others holistic.[3] I cannot pursue these arguments here in detail, except to note that my earlier remarks on the personal identity-formation of scientists fits best, I think, with a moderately holist account of collective intentionality.[4] That is, in the course of their training scientists internalize their disciplinary ethos in such a way that they identify themselves with the group and are ready to act as members of the group—which is to say, ready to act in a manner consistent with the ethos in situations in which group membership is relevant. Consequently, the ethos can function as a source of mutually binding normative commitments to which members hold one another accountable. As Tuomela (2007) puts it, the ethos supplies members with authoritative "group reasons." We find such reasons above all in the core consensus in a discipline: the basic beliefs about nature, proper experimental procedures, and discourse (vocabulary, basic equations, etc.) that normally lie beyond effective challenge—or that one dares to challenge only at great professional hazard (though with the chance of starting a scientific revolution).

Insofar as elements of the ethos function as group reasons, they satisfy what Tuomela calls

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[3] The classical representative of individualism is Max Weber, and of holism, Emile Durkheim; for overviews, see Schmid 2008; Tollefsen 2004; Miller 2001, chap. 2.
[4] See Gilbert 1989; for a holist view of science, see her 2000, chap. 3; I also understand Tuomela 2007 as moderately holist, though he is in some respects more individualistic than Gilbert. Note that the holist idea of collective commitment, as an endemic feature of stable social cooperation, also allows one to avoid the difficulties that rational choice theorists have in accounting for cooperation (see Heath 2001; Hollis 1998).

the “collectivity condition”: R is a reason for me if and only if R is a reason for every member of the group (2007, 28f, 49). Group reasons differ from the personal beliefs and commitments that scientists hold as individuals; such “private reasons” tend to vary from individual to individual, depending on personal background, history, desires, and goals. Consequently, groups that are held together solely by private reasons depend on the happy convergence of those reasons to motivate collective action. By contrast, a disciplinary ethos, as the source of group reasons, provides scientists with a set of publicly recognized basic guidelines for how members should act together when they engage in scientific inquiry. To be sure, the ethos leaves much to the individual’s discretion: where to take a job, which research projects to pursue, and so on. It even makes room for risky professional gambles at challenging entrenched beliefs—albeit with the demand that one meet the more stringent burden of proof such beliefs set for challengers. But the core elements of the ethos are otherwise authoritative for the conduct of inquiry and for constructing scientific arguments.

In sum, as a source of group reasons, the disciplinary ethos supplies beliefs, values, and standards to which the group is collectively committed, such that members mutually expect one another to respect those reasons in the relevant situations.[5] Because such reasons meet the collectivity condition, they count as an *irreducible common good*: they count as good reasons for a given member of the discipline—thus a good one is obligated to respect or acknowledge as a member of the discipline—if and only if they count as good reasons for all the other members. Something that does not satisfy this condition, say a privately held conjecture about life on other planets, cannot function as part of an ethos that unifies members in virtue of its authoritative status in the group.[6] For example, the fact that members of a research team each personally has a

[5] Note that such respect is compatible with personal divergence; rather, the obligation to respect group reasons means that one must keep doubts to oneself in group-relevant situations, or else justify the divergence with good reasons.
[6] However, the question of life on other planets seems to function as one that astronomers recognize as interesting, and thus could constitute a group reason used to justify a research project.

hunch that some hypothesis H is true may allow them to come together in a joint research project. Each team member's belief about H functions as a private reason that happens to coincide with the other members' beliefs about H. But that shared belief does not satisfy the collectivity condition for members of the discipline, and not even for members of the team. So long as it remains a mere hunch, any team member may change his or her mind without having to account to the others (though if she has agreed to the project, she would still remain bound by that commitment).

(3.2) Notice that the irreducibly social achievements alluded to earlier (sec. 2(d) above) are linked with those elements of the group ethos that function as group reasons. On the one hand, these reasons allow members to cooperate by providing them with unproblematic background knowledge and methods of research. To that extent, it makes sense to speak of the disciplinary ethos as a means to the achievement of public knowledge, as in (e) above. On the other hand, the ethos is not merely a means. To begin with, the ethos includes well-established scientific beliefs, presumptive knowledge of nature, and thus is part of the stock of knowledge about nature that constitutes the end of research.

Second, as I noted earlier, fully committed scientists—or what Tuomela calls the "operative members" of the group—internalize the ethos as a constituent of their identity as scientists, and thus come to judge their achievements by recourse to standards set by the ethos. To that extent, the ethos is constitutive of the excellence that scientists value as individuals—hence constitutive of the good of each member's flourishing as a scientist, together with other scientists.

This second observation leads to a deeper point about the connection between the end of research—the knowledge-product a team hopes to achieve—and the ethos as a means. Irreducibly social achievements presuppose that team members share elements of the ethos not only because these foster cooperation in a common research project. The connection is deeper: the ethos provides the beliefs, values, and standards in the light of which team members and other members of the discipline can recognize an achievement for what it is—*as a scientific achievement, a contribution to public knowledge*.

The disciplinary ethos plays this definitional role in various ways. The ethos can include collective beliefs about the most important problems in need of solution. It supplies the vocabulary, mathematical techniques, known facts that any convincing presentation of a putative knowledge claim must employ or not contradict. Of particular importance are the norms that govern sound methodology—to a large extent, research results are most closely scrutinized for experimental design, statistical methods, and the like. Finally, the ethos includes more diffuse values by means of which scientists judge the elegance, simplicity, strength of confirmation, etc., of a hypothesis. For example, the fact that Einstein's relativity theory received striking corroboration from Eddington's famous experiment in 1918 has entered into scientific folklore as a "proof" of the theory. Here we see an aspect of the scientific ethos, namely the assumption that experimental confirmation of an improbable theory provides strong evidence for its truth—hence for Einstein's singular achievement in advancing our understanding of nature.

This deeper connection between the disciplinary ethos and our very ability to recognize an achievement for what it is buttresses the point that the ethos is not a mere means. If the ethos defines what counts as a scientific achievement, a genuine contribution to public knowledge, then we cannot fully separate knowledge as an end from the ethos as a means. Rather, we do better, I think, to regard the ethos as the *symbolic context* or *cultural system* within which achievements appear as such.

(3.3) So far I have treated the disciplinary ethos as a constellation of intentional contents: above all beliefs about nature, about how to conduct experiments, about standards of good evidence and proper discourse, use of vocabulary, epistemic values, etc. However, the intentional components of a disciplinary ethos cannot be separated from the material practices of inquiry: what scientists do with things in the laboratory and field site, the actual flow and social organization of discourse, the distribution of resources and money, and so on. Ethos and concrete practice are at most analytically distinct. As I understand the notion, social practices in general are normative patterns of intentional cooperative activity: members are able to name the practice in which they cooperate, and adept practitioners can normally explain the normative elements of their practice—the methodological rules, values, shared beliefs,[7] and aims of their practice. Along with the broader scientific ethos, the disciplinary ethos thus supplies the social-material practices of scientists with their sense and meaningfulness. Scientists can refer to elements of the ethos in explaining why they do this and not that.

This sense-making function of the ethos operates at various levels, from the mundane to the existential. Many simple operations, for example a chemist's choice of a solvent for extracting some synthesized material, rest on the stock of shared background knowledge, including in this case beliefs about solubility that are acquired over the course of the chemist's education. Given that background, it "makes sense" to try this solvent rather than that, and other chemists would understand the choice. Other operations rest on common rules that govern everyday experimental and data-processing procedures: for example, the assumptions about the conditions necessary for "quantitative transfers" in analytical chemistry, or about the number of samples required for reliable measurements. At the level of scientific discourse, it was once the case that the ethos of paper-writing dictated an impersonal style, with heavy reliance on passive voice. Finally, at the level of existential values, the ethos involves ideas about the value of science for human life, thus enabling members to regard their participation with self-esteem.

However, methodological and normative elements of the disciplinary ethos remain to a large extent rather vague and indeterminate. For example, standards of accurate measurement and reliable detection vary considerably by discipline, and within a discipline, such standards can

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[7] Notice that the fact-value distinction breaks down from this perspective: core beliefs have a normative status for members in the sense that one may not contradict them without considerable explanation and evidence.

vary by research site, depending on the instrument, stage of experimentation, and target object. In each case, scientists operate with a broad value of accuracy, which then receives its determinate sense in response to demands arising within the local context of practice. In a word, local practice *contextualizes* elements of the ethos, often in ways that do not follow straightforwardly from the ethos as a set of abstract ideas and general beliefs. This point has been generalized by the ethnomethodology of scientific work (ESW), which trades on the "reflexive" relationship between the symbolic elements of science (formulations of methodological rules, procedures, representations, etc.) and material practices. Here reflexivity refers not to heightened self-awareness but to the idea that the determinate sense of these symbolic elements, which make up the written and spoken "accounts" of research practice, depends on their contextualization in the practice itself (see Lynch 2001; 2000; more generally Lynch 1993; Garfinkel 1967).

In making the above point, ethnomethodologists have something quite mundane in view, namely the connection between practical laboratory skills and behavior, on the one hand, and what scientists say about what they do or should do, on the other. For example, formulated rules of method (experimental, mathematical, discursive, etc.) rest on *ways of doing things* that to some extent are so ordinary as not to merit fully explicit mention in published procedures and written work. This mundane point has interesting implications for the relation between ethos and practice, however. If ESW is on target, then we may say that local material practices give meaning to the ethos, inasmuch the ethos remains incomplete, intentionally indeterminate as to its precise import for practice, until scientists succeed in contextualizing it in their actual conduct of research. Thus the sense-making described at the beginning of this section goes in both directions: the disciplinary ethos allows scientists to make sense of their local practices of research, but only insofar as the latter give the ethos a determinate sense and import for a particular, locally situated research activity.

Harold Garfinkel once expressed this reciprocal relationship in a rather dense statement of the reflexivity postulate:

> *recognizable* sense, or fact, or methodic character, or impersonality, or objectivity of accounts are not independent of the socially organized occasions of their use. Their rational features *consist* of what members do with, what they "make of" the accounts in the socially organized actual occasions of their use. Members' accounts are reflexively and essentially tied for their rational features to the socially organized occasions of their use for they are *features* of the socially organized occasions of their use. (Garfinkel 1967, 3-4)

Simply put, the rationality or cogency of scientific papers and articles—in which scientists provide accounts of their research practices—is inseparable from the practices themselves. Notice, however, that Garfinkel's notion of an "account" includes more than elements of the disciplinary ethos. As I noted above, the actual conduct of research for which scientists account in their published work does not flow directly from the shared ethos. Between the ethos and the account lies the actual process of experimental inquiry: the numerous choices scientists make in designing experiments, the results they obtain, the mathematical analyses of those results, the interpretation of results, and so on. Thus the paper or published article is a complex symbolic entity, in which scientists account for their local practice by making arguments that link the details of inquiry with elements of the ethos.

The above analysis implies that account-making in science involves three key aspects: the disciplinary ethos, the local practice of research for which scientists give an account, and the account itself, i.e., the report, paper, or article. To understand the rhetorical challenge of scientific communication, we must examine the relationship between these three aspects. As I shall argue, however, the notion of the common good creates an interesting obstacle.

4 The rhetorical challenge: communicating the goodness of local practice

In referring to "account"-making, Garfinkel draws attention to the fact that members of a scientific discipline *hold one another accountable* for the "methodic character, or impersonality, or objectivity" of their research. In doing so, they rely on a shared vocabulary, standards of proper method, and the like, which are tied to the use of practical skills they have acquired in the course of their training as scientists. That is, they can hold one another accountable only because they share a disciplinary ethos, as *that to which* members hold one another accountable. This suggests a way to think about communication of research within a discipline: members working at different laboratories are able to communicate precisely because they can frame their accounts in relation to a shared ethos. Linking ESW with the notion of an ethos thus puts us on familiar ground, which scholars like Kuhn and Prelli have already covered (as noted in sec. 1 above). The ethos functions, in effect, as the shared language, grounded in lived practice, that enables scientists to communicate with one another.

However, if the disciplinary ethos enables such communication, then does the spectre of incommensurability arise once scientists attempt to communicate across disciplines? Not immediately. One of the main considerations that led to Kuhn's incommensurability thesis—a controversially strong meaning-holism—is absent in the analysis of disciplinary ethos.[8] At the same time, there are positive reasons for thinking that contexts of research remain connected even across disciplines. On the one hand, scientists remain generally committed to the idea of a common objective world; on the other hand, research contexts tend to be linked by relations of relevance that make results in one context relevant for another.[9] Geologists, for example, rely heavily on physics knowledge; conversely, sensitive physics experiments sometimes require knowledge of the local geology. These sorts of cross-context links can motivate scientists to communicate across disciplines, on the assumption that true beliefs about the common objective world must cohere.

Nonetheless, a problem of communication remains—a problem that arises not only across disciplines *but even within them*. To see this, consider the implications of regarding the

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[8] There are numerous critiques of Kuhn's incommensurability thesis: Zammito 2004; Davidson 2001; Laudan 1996.

[9] For further argument on these points, see Rehg (2009a, chap. 7; 2009b).

disciplinary ethos as a common good, on the one hand, and as indeterminate, in need of local contextualization, on the other hand.

The disciplinary ethos functions as a common good because in the course of their training scientists have collectively accepted it as a source of group reasons that guide research choices. However, the ethos mainly boils down to the core consensus, what scientists acquire through textbooks and standardized laboratory courses at the undergraduate level. In the course of their education, students typically find some parts of their discipline interesting and other parts puzzling or difficult to master; I thus assume that most students who go on for higher degrees choose their specialty according to that educational experience, though job considerations can also play an important role, of course. In choosing a specialty because one finds it interesting (and finds one can master it), one responds, then, to one's *direct personal experience of the goodness* of the research practices that make up the specialty. But because the student is responding to a prepackaged disciplinary core—the disciplinary ethos—the student's experience also aligns with the *common* goodness of those scientific practices, which lies safely in the hands of one's teachers, for the most part beyond serious challenge.[10] The student is introduced, that is, to practices and results that have established themselves as good science across the discipline.

However, the more the student engages in original research (in the course of advanced education), the more significant the indeterminate character of the ethos becomes. Original research extends the stock of knowledge, and thus moves beyond the confines of the core consensus in ways that are not wholly predetermined by the ethos. If that is the case, then my earlier claim that the ethos allows scientists to recognize an achievement for what it is requires qualification. As the context for recognized scientific achievements, the ethos is at most a necessary condition for such recognition—but it is not sufficient. Thus, when members of a research team judge that their local results count as an achievement, as a result of good science

[10] Student conferences provide the exception to this generalization; as the student moves into advanced degrees, he or she is increasingly beholden to the wider audience of scientists.

and a significant contribution, their judgment cannot rely on the ethos alone. What makes up the difference, I suggest, is the team members' direct experience of their creative work as a team, the direct, locally situated engagement of their skills as scientists, by which they contextualize the disciplinary ethos, extending and determining its import for the research problem at hand. The scientific achievement, as a good, has its "proof," so to speak, in the team's direct experience of the fruitfulness of their local research practices. Team members directly experience their practice as a way of doing good science together.

The challenge that confronts scientific account-making should now be clear. In publishing an account of their research, the research team makes a claim to contribute to public knowledge—they thus claim their work has yielded a common good for the discipline at large. In saying that some local scientific work is a "common good for the discipline at large," I do not mean that the work is useful or good for every member of the discipline, but that it meets disciplinary standards of good science and, in addition, is fruitful for the discipline—potentially relevant for researchers beyond those reporting the results.[11] If the foregoing argument is on target, however, then the published account of a research achievement draws its force not merely from the common disciplinary ethos, but also from team members' lived experience of their local experimental practices. Because this achievement claim involves a local, directly experiential dimension, the cogency of evidential arguments contains an indexical component that resists communication to outsiders. What is more, the dimension of fruitfulness means that scientific arguments often involve a claim that a particular line of research will yield fruit for other scientists and interested parties. But only the members of the team have a direct experience of such fruitfulness, indeed a very limited experience at that. In sum, the claim that a result represents a *common* good for the discipline at large partly rests on the team's *local—thus non-common—experience* of its goodness.

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[11] Fruitfulness thus involves the idea that one's research has produced a "significant truth," an idea developed by Kitcher 2001.

Whence the rhetorical challenge: how can the team communicate its local experience of the goodness or fruitfulness of its research practices? Appeal to shared elements of the disciplinary ethos is important, but it takes one only so far. Indeed, gaining the approval of referees and journal editors to publish is only the first step toward achievement: at that point, one merely has a defeasible confidence that one's work apparently satisfies disciplinary standards and is apparently relevant for the discipline at large. The more daunting challenge is this: to overcome outsiders' indifference and show that one's local common good is actually relevant for wider contexts.

5 Conclusion: a task for the rhetoric of science, beyond Kuhn

In giving oral and written accounts of their experimental results, scientists strive to persuade their audiences that their research counts as good science, and thus as a contribution to their discipline and its pursuit of public knowledge. However, a contextualist analysis of the common goods of scientific practice shows that scientists confront a particular rhetorical obstacle in pursuing their persuasive goals. My central argument might be summarize as follows:

> 1 In writing up an account of their research findings as a contribution, scientists claim to have achieved something that is a common good, something of value for their discipline and possibly beyond it.
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> 2 A research finding counts as an achievement only if it counts as good science.
>
> 3 Therefore, members of a research team can judge their work to be an achievement only if they judge it to be the outcome of good science.
>
> 4 As an established common good for the discipline, the disciplinary ethos sets forth standards of good science that function as authoritative group reasons for members, in light of which they can recognize achievements for what they are.
>
> 5 But as an element of the shared group ethos, standards of good science are not fully determinate: standards must be contextualized and extended at local research sites.
>
> 6 Therefore, the research team's judgment that their work counts as an achievement, an outcome of good science, cannot be fully secured by recourse to the ethos as a common good, but it depends in addition on the direct experience of their local practice as good science.
>
> 7 Therefore, in writing up an account of their research findings as an achievement, a common good for the discipline, the research team faces a rhetorical challenge: they must

communicate to others their own local experience of the goodness of their practice.

If the above argument is on target, then it raises a question for the rhetoric of science: how do scientists attempt to meet this challenge? There is, of course, a large body of work on the rhetoric of scientific persuasion. The analysis in this paper brings a relatively neglected perspective to that work, namely the rhetorical problem of communicating a local *experience* of goodness to wider audiences.

I conclude with two implications for further research. The first concerns the Aristotelian understanding of ethos as "proof from character," i.e., the idea that proficient speakers aim to establish their competence as judges of the matter under consideration. As I noted above (sec.1), Prelli analyzes argumentative ethos above all in light of the shared disciplinary ethos. Scientific arguments succeed in their communicative aims with the help of rhetorical framing devices: recognized research agenda, types of questions, lines of argument, and the like. A contextualist analysis shows that shared *topoi*—the sayable "commonplaces" of argument—only go so far. The idea of proof from character, however, involves a further idea that goes to the heart of the rhetorical challenge in the above argument. The rhetorical moves that establish the author's competence instill trust in the audience, in this case, the trust that the author is competent to the *judge* the goodness of his or her local research practice. This move solves the rhetorical problem indirectly: rather than attempt to communicate a local experience, one establishes one's trustworthiness in *assessing* that experience in light of shared disciplinary norms. A contextualist analysis thus brings out a dimension of proof from character that we miss if we only focus on shared disciplinary beliefs and norms.

Second, natural and social sciences face the same kind of rhetorical challenge. Rhetorical analyses of research article introductions support this conclusion. In such introductions, we should expect to see authors grapple with the rhetorical challenge described above, for the introduction (along with the abstract) is the place where one expects authors to lead potential readers to believe that the account promises something of potential interest—a common good for the discipline at large. In both natural and social sciences, the structure of introductions tends to follow a similar pattern, which displays aspects of the foregoing analysis. We can see this by examining two rhetorical models of introductions, as analyzed by Swales and Najjar (1987).

The "problem-solution" model of introductions emphasizes elements of the disciplinary ethos. According to that model, authors tend to rely almost entirely on the shared ethos to spark interest: "The researcher addresses the goals, current capacities, problems, and criteria of evaluation that derive from and operate within that discipline" (Zappen 1983, 130). However, the problem-solution model misses the way in which introductions are also embedded in the researchers' own local context of past research. Swales and Najjar thus propose a "create-a-research-space" model of introductions, according to which authors typically make four moves in an introduction: (1) they note that their own area of research is significant, (2) they allude to previous work in that area, (3) which proves however to be incomplete, so that (4) a gap or space opens up, in which the authors situate their own work. Surveying 100 articles, 66 in a physics journal (over a forty-year stretch, 1943, 1963, 1983) and 44 in an educational psychology journal (from 1963 and 1983), Swales and Najjar (1987) found that more than eighty percent of the introductions employed the fourth move.[12] Recent prescriptive statements of article writing seem to mesh reasonably well with this descriptive study.[13]

This finding provides preliminary support for the claim that in both the social and natural sciences, authors are aware of the same kind of rhetorical challenge, namely that of communicating to readers the common goodness of their work for the discipline. The research-space model in particular attends to the ways that authors typically use the introduction to

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[12] Swales and Najjar also found an interesting contrast between the physics and educational psychology journals: the physicists were far more likely to include announcements of their findings. A further question, which Swales and Najjar do not address, concerns the prevalence of the first three moves. Elsewhere, Swales notes that of 48 introductions, "more than half the cases" displayed the four-step structure (1984, 80).

[13] E.g., Scott's step by step construction of an introduction matches the four moves (2003, 44-48); though less detailed, Laszlo hits on the idea of bringing the reader into one's local experience when he says "[w]riting an introduction is equivalent to throwing open your room, turning your private space into public space" (2006, 26).

emphasize the disciplinary relevance, and thus the common goodness, of their own research. Authors do so precisely by placing their own local commitment in a broader context of previous research in the discipline. As Swales and Najjar describe the first move in the model, the authors attempt to establish that their "area of research is of some significance. This is most commonly done by claiming the area is nonperipheral; authors may claim that there is interest in it, that it is important or relevant, or that it has been widely investigated, or that standard procedures have evolved" (1987, 178f). The second move then goes on to summarize previous research. The implicit idea behind these moves, it seems, is this: the authors in effect claim that they are not the only ones to find the area to merit attention. Hence, this typical introductory strategy attempts to overcome the problem of communicating a local experience of goodness by drawing the readers' attention to other research sites and contexts, where members of the discipline have had a similar experience of the goodness of the research agenda. As Swales puts it, "many Article-Introductions are essentially exercises in public relations" (1984, 82).

These two concluding points do little more than point to an area of research where one might test the overall approach—namely, the rhetoric of science, for which the challenge of communication provides one of the major research agenda. However, if the analysis holds up, then we would have an approach to collaboration in science that captures some of the concerns that motivated Kuhn, but without the drawbacks of his incommensurability thesis.

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