strategic importance for situating the self within the problematic of the times. Similarly, one must master the literatures of one's discipline. This means reading the classics, the standard texts, the great works. There is no shortcut to expertise, but then there is no surer way to situate one's stance than to understand the history of intellectual problems that repose in the classic literature of one's discipline.

One should also study philosophy and be conversant with meta-theory, as these will provide the conceptual tools for framing one's inquiry and for addressing foundational questions. We see how it pays off as Youniss recounts his remarkable journey from an early training in the behavioral paradigm to paradigms that were increasingly cognitive, developmental, and sociological. Finally, one should struggle to make an integrative point whenever possible, and to extend integrative ideas into new domains, even if this takes one beyond the friendly confines of narrow specialization.

There are common themes in the chapters on situating ideas (Youniss) and developing and nurturing them (Wentzel). Both Youniss and Wentzel emphasize the importance of personal motivations in situating inquiry and identifying interesting questions. Both insist on the importance of expertise. Both point to dispositional qualities of the researcher as critical to the success of inquiry—engagement and collaboration must be intentional (Youniss), and effort, commitment, and persistence must be sustained (Wentzel).

We come to see that the problem of situating inquiry, developing and nurturing ideas, and framing a problem are essential components of critical discovery that share common elements. The chapters in this section call for a reflective appreciation of historical frameworks, meta-theory, and paradigms. The chapters make demands upon researchers for expertise, invoke intentional strategies to see clearly and differently, require retroductive skills and skills at building literatures, and insist that one seek integrative possibilities and mark progress.

Indeed, what signals an important problem or a good idea is that which makes an integrative point or solves a puzzle in a way that represents progress in the elaboration of a research program, and this determination is always comparative against rivals. Finally, the chapters of this section show that the questions that motivate scientific inquiry are often deeply rooted in biography and personal interest. We seek answers to burning questions that are suggested by our personal experience, and these questions also serve to situate us within an intellectual landscape.

It is a great privilege to take up the life of the mind in this way, for what we often discover is that the products of our research are as much crucial to self-understanding as to theoretical understanding, and that the problem of situating, nurturing, and framing is both personal and scientific.

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4

Developing and Framing Meaningful Problems

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Induction into scientific practice hardly ever takes up the matter of how to formulate and frame meaningful problems. Most primers on research methods are geared to the culminating steps in research, on how to test, evaluate, and dispose of hypotheses that otherwise seem to show up, like masked wrestlers, from "parts unknown." One might gather that educational and social science is mostly a technical matter of how to grapple unruly variables into submission, of how to assert proper experimental control, fit statistical models, and draw valid inferences. Nothing is trained so assiduously as the ability to indict a study for its yield of flaws. What is absent, however, is sustained reflection on the source and object of these exertions, which is a theoretical problem worthy of the effort. There is barely a word on how to ask questions, frame a problem, or generate a theory; and very little guidance on what is to count as a meaningful problem or a good idea. In the absence of these considerations, the default criterion is often sheer novelty—the good idea is that which has not yet been expressed or been found in print. Of course, the fact that an idea has never occurred to anyone is scarcely reason to invest it with meaning.

Discovery and Justification: The Standard Account

Both Karl Popper and the Vienna Circle of logical positivists could claim some credit for this state of affairs. "The work of the scientist," Popper (1959) asserted, "consists of putting forward and testing theories" (p. 30), although in his view only the theory-testing part of the work presented any interesting philosophical problems. How one happens to put forward a new idea was not worthy of notice. It always contains an "irrational element" (p. 32). It is strictly a psychological matter of creativity, intuition, and inspiration that can have no implication for the logical analysis of scientific knowledge. Nor is it possible to rationally reconstruct the steps by which a scientist comes to propose a theory. No logical analysis is possible for understanding how hypotheses occur to scientists, how inventions or discoveries cross their mind, for understanding "the processes involved in the stimulation and release of an inspiration" (p. 31); there is "no such thing as a logical method of having new ideas or a logical reconstruction of this process" (p. 32), but only for subsequent tests of the products of inspiration. Hence, in this way are scientific methodology, logical analysis, and rational reconstruction reserved strictly for justification, but never for discovery.

These strictures have solidified into a standard account that relegates the creation, invention, and origins of theory, the "putting forward" of ideas, to the context of discovery, of which little can be said; while the appraisal of a theory's evidential warrant is remanded to the context of justification, at whose disposal is placed the whole armamentarium of a discipline's methodological, analytical, and logical tools. The context of discovery, if not entirely occult, is nonetheless beyond methodological specification and is only preparatory to the real work of science, which is to justify winners among those theories that do manage to show up for the match. One suspects that the lingering influence of the standard account in educational and psychological science has diverted attention away from the front end of discovery, invention, and theory construction and toward the back end of justification, appraisal, and evaluation.

Standard Account Revisited

Yet the distinction between discovery and justification has fallen on hard times. A consensus exists that there is a kind of logic of discovery that revolves around reasonable arguments for pursuing plausible lines of inquiry, and that these *pursuit arguments* are relevant for theory appraisal. The arguments that make a line of research reasonable to pursue also have implications for how it is appraised once it is launched.

Kordig (1978) argued, for example, that after the initial "hitting upon an idea," one does, in fact, subject hypotheses to a kind of rational appraisal. One can deem them promising, worthy of exploration, meaningful, plausible to pursue, and all for good reasons. A question could be worthy of pursuit if its confirmation ("acceptability") constitutes a lethal blow against a rival, if it extends a line of research into new areas, or if it anticipates novel facts or upends a settled convention. Plausible hypotheses are then put to the test, with empirical confirmation supporting its acceptability, but along with other considerations, including simplicity, fertility, extensibility, and the like. However, these other considerations are also good reasons to establish plausibility in the first place; they also constitute rational grounds to pursue this hypothesis but not another.

Although plausibility arguments occur prior to acceptability (one does not ordinarily attempt to justify implausible hypotheses), the sort of arguments that are relevant to acceptability are also relevant to plausibility, which suggests that there is no fundamental distinction between reasons for plausibility and reasons for acceptance. The various considerations that support the acceptability of hypotheses at the back end of justification are similar to those that signal plausibility at the front end of discovery.

Gutting (1972) argued similarly that the context of discovery consists of two movements: There is "inventive discovery," which describes initial conjecture, the thinking of hypotheses, and then there is "critical discovery," which judges whether hypotheses are plausible and worthy of pursuit. According to Gutting, critical discovery uses arguments that are guided by regulative principles. For example, critical discovery often appeals pragmatically to heuristic principles (e.g., simplicity, analogy) that "provide convenient ways of continuing research" (p. 389) when the empirical evidence does not indicate which direction it should take or what should be done next. As such, heuristics are maxims of convenience that point the way out of "desperate situations" (p. 389).

Critical discovery also appeals to meta-theoretical principles that summarize the scientist's views about the nature of scientific theories, or to broad metaphors and claims about the nature of the persons or the domain of inquiry (what Gutting [1972] termed "cosmological principles"). Scientists who are reflective about the meta-theoretical implications of their work and of the problematic that confronts a discipline can use this reflection to guide pursuit assessment, particularly during periods of ferment and transition among paradigms. Claims about the nature of persons are often derived from the core metaphors of research programs. For Piaget, the child was a naïve scientist who investigates the properties of the world; for Kohlberg, the adolescent was a naïve moral philosopher who struggles to understand Kantian ethics; for certain cognitive scientists, the mind processes information much like a computer; for some educational researchers, the variables that influence student achievement (e.g., class size, per-pupil expenditure) are modeled much the way functional relationships are established between inputs and outputs in the manufacture of commodities.

In addition to maxims of convenience, meta-theory, and cosmological principles, one can also point to the research tools of science as a bridge between critical discovery and justification. For example, Baird (1987) found the role of exploratory factor analysis useful to the logic of discovery. Gigerenzer (1991) argued for a *tools-to-theory heuristic* that envisions two steps: first, the entrenchment of research tools (e.g., statistical techniques, computers) generates new metaphors and concepts; second, these metaphors and concepts lead to greater acceptance of theories that partake of them if the research community uses the tools extensively. The widespread use of computers, for example, generated metaphors and concepts that modeled cognition in terms of information processing (Gigerenzer & Goldstein, 1996). The widespread use of statistics encouraged models of human decision making that traded on the metaphor of the person as an "intuitive statistician" who makes interesting errors when asked to generate or consider the probability of events. In this way, discovery is "inspired" by justification, rather than being independent steps in scientific problem solving (Gigerenzer, 1991).

Others have argued similarly that research programs are in fact prodded along by rational heuristics "which guide research by indicating both the method by which new theories should be constructed and the manner in which the whole program should deal with empirical refutations" (Zahar, 1983, p. 244). Indeed, Lakatos (1978b) argued that all research programs have heuristics that give direction to the progressive elaboration of their core commitments, including how to fend off recalcitrant evidence and prima facie refutation, although the work of these heuristics might be apparent only with reconstruction and historical analysis of a research problem. Once again, the heuristic that gives direction to lines of research on the "front end" of discovery is relevant to the appraisal of the evidence on the "back end" of justification.

It would seem, then, that the divide between discovery and justification is not the unbridgeable chasm once feared and that the historical neglect of the context of discovery is not warranted. Of course, none of this implies that the context of discovery is amenable to anything like mechanical generation of theories or hypotheses. As Cronbach (1986) put it, "Planning inquiry cannot be the subject of prescription because planning is the art of recognizing trade-offs and placing bets" (p. 103).

Perhaps it is not prescription that is wanted, but rather attested strategies for placing strategic bets. Gutting (1972) suggested that a scientist well-equipped to exploit the context of critical discovery would be conversant with meta-theory, philosophy, and theology. After all, scientific practice is by and for "Earthlings" (Cronbach, 1986), and as such, trades on the full range of human experience for its inspiration, for which no discipline or reflective practice can be excluded as a possible source. We bring our complete personality to the contest; our interpretive frameworks are forged in the heat of our biography as much as by formal training in theory, meta-theory, tools, and heuristics. Discovery is the prize of the prepared mind, to be sure, and there are no shortcuts to scientific expertise, yet our "nonscientific conceptual schemes," as Gutting puts it, our general views about the nature, purpose, and subject of inquiry, are often the starting point of critical discovery and scientific refinement.

Of course, science is not done from the safety of bleachers; it is not a formalized transcendental activity that leads easily to didactic formalisms. Rather, science takes place in the ring. Critical discovery is the result of one's wrestle with problems that seem crucial from the vantage point of one's intellectual biography. Hence it is the narrative, vignettes, accounts of critical incidents, and key decision-making points that provide the prism through which researchers wrestle with meaningful problems.

In the next section, I describe how this has played out in terms of my own research interests. I offer these examples not because they are remarkable, but because they are ordinary. They illustrate the struggle of critical discovery, the appeal to regulative principles, strategic bets, and nonscientific conceptual schemes in articulating the contours of a research program, but also the powerful role of key moments in one's personal formation as a scholar.

DISPENSING WITH THE SCIENTIFIC METHOD

A Formative Vignette

One of the first things that I learned as a relatively new doctoral student at the University of Wisconsin–Madison was that everything I knew about science was wrong. I thought science was regulated by a certain method with well-defined steps. One starts with a theory that yields one or more hypotheses. Data is collected. Hypotheses are then compared against the data, compelling theoretical chips to fall where they may. The scientific method pits theories against the facts, and theories must yield to the facts in the test of hypotheses. If all goes well, one can announce a new discovery; if not, then it's back to the first step, and the process begins anew. In the case where there are two theories that offer competing hypotheses, one might conduct a critical experiment to decide between them. Here the observed data has probative force for eliminating one of the contenders. Any reasonably astute middle school student who has read his or her science textbook or struggled with smoking volcanoes or bread mold at the science fair can vouch that this is how the scientific method works. It is a series of steps, a checklist of things to do on the way to great discoveries. Along the way, something about falsification might get added to the story, but this will only seem like a friendly amendment to the basic outline of the method.

My understanding that the work of scientists is governed by method came undone one afternoon, not in the laboratory but in a Madison tavern. My two mentors and several fellow graduate students turned a late afternoon conversation in the direction of meta-theoretical issues, and I found myself defending the scientific method as I had understood it since adolescence. Facts and theories, I was certain, are different things. Facts are hard, objective, and infallible. They are touchstones against which the truth is winnowed from theories that are provisional, fallible, and soft. This defense of science was forceful and, in my own mind, not a little heroic in the face of such intractable resistance on the part of my stubborn interlocutors, though I failed to convince them of the utter necessity of empirical foundations, of the self-evidence of hard facts, and the common sense of method. Of course, my naïveté was much deplored, and one of my mentors, Ron Serlin, an editor of this *Handbook*, soon put into my hands a volume of the collected works of the great philosopher of science Imre Lakatos.

When the importance of vignettes and critical incidents is noted as a source of ideas, I think of this discussion in a Madison tavern. I think of my first bracing encounter with the philosophy of science. At one level, this account is a key moment in a narrative about my professional formation. Yet the lesson is not that I once had untutored ideas about the ways of science, but now I see clearly. Indeed, seeing clearly is a constant struggle. Rather, one lesson of this vignette is that induction into scientific practice often takes place outside the formalities of the classroom and laboratory. It takes place whenever mentors suffer the exertions of their charges. A more important lesson concerns the possibility that philosophical conceptions about science can provide powerful frameworks for conceptualizing one's work, and that the history of science provides guidance for placing the next strategic bets in the evolution of one's research program.

Lakatos's (1978b) methodology of scientific research programs continues to be a source of insights for how I understand my own research and for how I appraise growth and progress in the academic fields of interest to me. I will illustrate this in two concrete ways, but I should like to preface my examples with a brief recount of some of the key themes from Lakatos's work that I found particularly useful. Lakatos had an expressive style that was by turns extravagant, memorable, and entertaining. Of course, I make no claims for the status of Lakatos's work in the contemporary philosophy of science.

Lakatos on the Methodology of Research Programs

One of the dogmas of positivism was the idea that observations and theory were things cleanly demarcated in scientific methodology. This was the dogma that I carried with me to graduate school, and it is one that is still widespread, at least in popular discourse. After all, evolution is still dismissed by religious doubters on the grounds that it is *just a theory*. However, the positivist demarcation between facts that are hard and theory that is soft is no longer a credible one. There are very few unreconstructed Baconians these days. No one believes seriously that facts are there to be gathered up like meadow flowers, just by the mere observation of them. Observations do not exist outside the texture of theoretical frameworks, and they become factual only in light of a theory that gives them significance.

Of course, the same stricture holds for the evaluation of cumulative progress in one's field. Just as one does not idly stare at nature waiting for observations to surface, one does not survey literatures waiting for progress to emerge unless aided by theoretical considerations about what progress amounts to (Serlin & Lapsley, 1985). If evidence of progress in a field of study is the fact that one seeks, then a theoretical framework is required to specify how it is to be ascertained. There are, of course, numerous philosophical conceptions about how science works, all of them fallible, like any other scientific theory, although some are less fallible than others. But one lesson I have drawn from Lakatos is that the history of science refutes philosophy of science. One must test normative conceptions of scientific rationality against actual scientific practice. Of course, Popper (1971) would add that there is no history as it actually happened; there is no "actual scientific practice," only history reconstructed from a particular point of view. But this means that we bring our conceptual frameworks to bear here as well to excavate the relevant historical datum proper to the task of evaluating scientific progress.

Rational Reconstructions

One of the pleasures of reading Lakatos is his comparative reconstruction of various conceptions of science. Lakatos picks up the story with *justificationism,* the view that scientific knowledge had to be *proven* knowledge. For the justificationist, intellectual honesty required one to assert nothing that was unproven. To pull this off requires an inductive logic that could transfer the truth-value of empirical propositions ("hard facts") to the universal theory. Lakatos pointed out that justificationism fails on both counts: Propositions cannot be proven by facts (only by other propositions), and inductive logic cannot deliver proof (no logic can infallibly increase content). "It turned out that all theories are equally unprovable" (Lakatos, 1978b, p. 11), a disquieting conclusion if one affirms that science is in the proving of knowledge business. After all, if scientific propositions cannot be justified as true and proven, then skepticism seems lurking around the corner.

But here probabalism steps in to ward it off. *Probabalism* asserts that even though we cannot prove scientific propositions, we can establish their probability in light of the evidence. On Lakatos's (1978b) account, this is a major but necessary retreat for justificationism, insofar as scientific honesty now "requires less than had been thought: it consists in uttering only highly probable theories; or even in merely specifying, for each scientific theory, the evidence, and the probability of the theory in light of the evidence" (p. 11). Yet probabalism fails, too. Citing "Popper's persistent efforts," Lakatos affirms that all theories have zero probability, whatever the evidence, and concludes that "all theories are not only equally unprovable but also equally improbable" (p. 11).

Hence, hypotheses cannot be proven, justified, or made probable by induction. All scientific theories are equally unprovable and improbable. These are pretty heady conclusions if one understands the scientific method as a truth-verifying mechanism. But if scientific theories cannot be justified or proven, they can certainly be disproved or falsified. The falsification doctrine was intended by Popper (1959) as a demarcation criterion between science and metaphysics, although Lakatos identifies at least two different versions of it: dogmatic and methodological.

Dogmatic falsificationism. What Lakatos called dogmatic falsificationism was the view that while scientific theories are conjectural and fallible, hard facts are not. Under this view, there still exists an infallible empirical basis by which to eliminate fallible conjectural theory, and to do science honestly requires one to specify in advance the conditions under which one would give up one's theory. If the factual empirical basis of a theory was unavailing, one must eliminate the theory from consideration.

Yet this version of falsificationism depends on the false assumption that there is a natural demarcation between soft conjectural theory and hard incorrigible facts. As Lakatos (1978a) put it, "The demarcation between the soft unproven 'theories' and the hard proven 'empirical basis' is non-existent: All propositions of science are theoretical and, incurably, fallible" (p. 16). Moreover, dogmatic falsificationism does not lead to the elimination of theories. Any specific theory includes an implicit *ceteris paribus* ("all things being equal") clause that is jointly put to the test along with the theory. When a refuting instance is observed, the decision to eliminate the theory is forestalled by appeal to the ceteris paribus clause—all things must not have been equal. Hence a theory can be retained regardless of what a test shows, simply by replacing the *ceteris paribus* clause. In this way, theories are tenacious in the face of putative refutation. The lesson drawn is that "*Scientific theories are not only equally unprovable, and equally improbable, but they are also equally undisprovable*" (Lakatos, 1978b, p. 19), a lesson that should cause anyone to lean forward just a little.

Methodological falsificationism: naïve and sophisticated. But methodological falsificationism rescues scientific rationality from the skepticism that fallibilism invites. Simply put, a way must be found to eliminate theories while not disputing the fallibility of scientific criticism. Popper's version of methodological falsificationism (which Lakatos called "naïve") affirms the necessity for scientists to make methodological decisions about what statements are to be considered the theory under test, which is to be considered the factual empirical basis, and which are to be considered unproblematic background knowledge. The decision to cordon off certain statements as the factual empirical basis is a methodological convention grounded by wide embrace of a relevant experimental technique that is well-understood by a scientific community. Put differently, certain elements of theory are conventionally treated as empirical facts under specific methodological conditions (e.g., there exists a relevant technique such that anyone who has learned it could agree on the observational status of a statement).

Popper's methodological falsificationism accepts as scientific any theory that is falsifiable. A theory is falsified when there is an observation statement that conflicts with it. Of course, experimental tests are not that clean. A theory comes bundled with numerous auxiliary hypotheses and limiting conditions. The methodological falsificationist makes a methodological decision to relegate some of these to "unproblematic background knowledge" in order to expose the theory to experimental hazards, and, should the study fail, to forbid *ad hoc* appeal to auxiliary hypotheses to rescue the theory under test. Hence, for Popper (1962), a theory can be either corroborated or refuted; there is no middle ground. Scientific rationality is a matter of conjectures and refutations.

But Lakatos's version of methodological falsificationism (which he calls "sophisticated") has different rules about when to accept a theory as scientific and different rules concerning falsification. For Lakatos, a theory is scientific, not if it is merely falsifiable, but if it anticipates novel facts over predecessor theories. A theory is admitted as scientific if it has excess empirical content ("acceptability$_1$"), some of which is corroborated ("acceptability$_2$"). Hence, the scientific status of a theory is a comparative-historical matter.

And so is the decision to abandon a theory. Mere criticism is never sufficient to falsify a theory, and beautiful theories are never eliminated because of a few ugly facts. This is how Lakatos (1978b) puts it:

> But, of course, if falsification depends on the emergence of better theories, on the invention of theories which anticipate new facts, then falsification is *not* simply a relation between a theory and the empirical basis, but a multiple relation between competing theories, the original "empirical basis," and the empirical growth resulting from the competition. Falsification can thus be said to have a *"historical character."* (p. 35)

According to Lakatos's reconstruction, scientists make a methodological decision to protect the hard core of their theory from refutation by a protective belt of auxiliary theories, such that the arrow of refutation must be directed away from the core to the auxiliaries. Lakatos (1978b) called this the "negative heuristic" of a research program. The negative heuristic specifies where the chain of refuting evidence must not lead—it must not be permitted to undermine the hard-core commitments of the theory. Instead, the

auxiliary theories must bear the brunt of the refuting evidence.

But research programs also have a *positive heuristic.* The positive heuristic specifies how successive modifications of a theory are to be developed so that anomaly and refutation can be digested and turned into supporting evidence. Each successive modification yields a new theory, so that scientific appraisal shifts from appraisal of isolated theories to appraisal of a *series* of theories "where each subsequent theory results from adding auxiliary clauses to the previous theory to accommodate some anomaly" (Lakatos, 1978b, p. 33). A research program is *theoretically* progressive if it has excess empirical content compared to its predecessor. It is *empirically* progressive if some of the excess content is corroborated. Of course, there are anomalies every step of the way.

But all research programs swim against the tide of recalcitrant or inconclusive data. All research programs swim in an "ocean of anomalies" (Lakatos, 1978b, p. 147). Yet scientists rationally decide not to allow these putative refutations to transmit falsity to the hard core of the research program if the corroborated belt of protective auxiliary theories, guided by the positive heuristic, is theoretically and empirically progressive. Hence, the methodology of research programs

> is more tolerant [than naïve falsificationism] in the sense that it allows a research program to outgrow infantile diseases, such as inconsistent foundations and occasional ad hoc moves. Anomalies, inconsistencies, and ad hoc stratagems can be consistent with progress. [But the appraisal of research programs] is also more strict in that it not only demands that a research program should successfully predict novel facts but also that the protective belt of its auxiliaries should be largely built according to a preconceived unifying idea, laid down in advance in the positive heuristic of the research program. (Lakatos, 1978a, p. 149)

When should a research program be abandoned? Only when certain criteria are met: There must exist a rival program that is powerful enough to account for the facts of the former program but also have sufficient generative power to anticipate novels facts, some of which have been corroborated. But even this is not sufficient grounds to abandon a theory so long as it is progressive, that is, so long as the positive heuristic is still capable of anticipating novel facts.

I want to underscore a number of interesting implications of Lakatos's methodology of research programs. First, the notion of *growth* becomes the defining characteristic of science. "It is the progressing problematic frontiers of knowledge," Lakatos (1978c) writes, "and not its relatively solid core, which gives science its scientific character" (p. 174). It is the progressive character of a research program, and the growth of knowledge that it represents, that determines whether the research program is scientific. Second, a theory is never eliminated because it fails an experimental test. Rather, falsification depends upon the emergence of a better theory that anticipates novel facts (excess content), some of which has been corroborated (excess corroboration). Moreover, there can be no "instant rationality" in the appraisal of a research program. The truth of a theory cannot be judged in isolation of historical considerations, but instead must be judged by the growth of knowledge that it represents in comparison with rivals. Scientific rationality depends upon progressive problem shifts and growth that can be determined only after comparative-historical appraisals of rival theories. This also means that there is no such thing as a "crucial experiment." Whether an experiment turns out to be crucial is an honorific title conveyed with long hindsight if it is seen to provide a "spectacular corroborating instance for the victorious program and a failure for the defeated one" (Lakatos, 1978b, p. 86).

Summary

I should like to illustrate with two examples how Lakatos's methodology of scientific research programs influenced the contours of my own work. But let me first summarize a few key points that I have taken away from this richly provocative framework:

- New theories must be treated leniently—"all theories are born refuted and die refuted" (Lakatos, 1978b, p. 5).
- The most important criterion for new theories is *boldness,* but this cannot be determined in

isolation. A bold theory has excess empirical content over rivals, and this must be determined in a historical context against background theories.

- But bold new theories swim in an ocean of anomalies. There can be no refutation before the emergence of a better theory, where "better" means excess empirical content (anticipates novel facts) and excess corroboration (some of which have been corroborated).
- "Evidential support is a historico-methodological concept" (Lakatos, 1978a, p. 184). Evidential support for a theory depends upon "*the corpses of its rivals*" (p. 184, emphasis added).
- The hard-core commitments of a research program are protected by a belt of auxiliary theories. The protective belt bears the brunt of refutation.
- A research program is progressive if its positive heuristic is successful in anticipating novel facts and in turning troublesome results into corroborating evidence—all in the face of recalcitrant data, prima facie refutation, inconsistencies, and anomaly.
- Research programs are degenerating if they run out of gas, that is, if the protective belt can only be held together with a series of ad hoc stratagems that explain away refuting instances without increasing empirical content. Degenerating research programs assume a strictly defensive posture in support of the core claims.

THE METHODOLOGY OF RESEARCH PROGRAMS: TWO EXAMPLES

The main burden of this chapter is to show how certain meta-theoretical frameworks can help researchers frame their scientific activity in a way that generates productive research ideas. My fascination with Lakatos's methodology of research programs (as he called it) provided me a useful way to conceptualize certain problems in two areas of research of long interest to me, one in moral development and another in adolescent cognitive development. We took up the challenge of how to understand psychological research with the categories of Lakatos's methodology.

The Good-Enough Principle

Soon after Ron Serlin's tutelage, we began to put Lakatos's framework to good use. We first took up Paul Meehl's (1967, 1978) famous papers that scored psychology for its slow progress, lack of cumulative knowledge, and reliance on null hypothesis testing. Meehl argued that there was an asymmetry in theory testing between psychology and physics. In psychology, the null hypothesis is always false, so that increases in precision always lead to weaker tests of a theory, while the converse was true in physics. Moreover, psychological research was plagued by slow progress, inconclusive results, and shifting faddish interests motivated as much by "baffled boredom" as by the empirical warrant. Apparently, social scientists were not as willing as their counterparts in the presumptively harder sciences to take seriously the Popperian requirements for intellectual honesty.

I was alerted to Meehl's (1967) paper by Lakatos's (1978b) gushing reference to it in two footnotes. In footnote 3 (p. 88), Lakatos notes Meehl's "brilliant" exposé of psychology, where many alleged research programs turn out to be little more than chains of ad hoc stratagems. In footnote 4, Lakatos underscores Meehl's attack on null hypothesis testing as a "machinery for producing phony corroborations and thereby a semblance of 'scientific progress' where, in fact, there is nothing but an increase in pseudo-intellectual garbage" (p. 88–89). Those seemed like fighting words, and so Ron Serlin and I set about responding to the challenge of Meehl's indictment in a series of papers (Serlin & Lapsley, 1985, 1990, 1992).

We attempted to account for slow progress in psychology by appealing to Lakatos's methodology of research programs—how there can be no instant rationality in appraisal of theories; how research programs set out uneasily in a sea of anomalies and take time to get their bearings; and how appraisal must take on a historical character that was nowhere present in Meehl's indictment but must be reconstructed from the evidentiary record in comparison with rivals, and how all this takes time! We also proposed a "good-enough principle" to fortify hypothesis testing and to put it on proper Popperian footing with respect to precision and falsification.

In a later paper, Meehl (1990) graciously conceded some points to us ("I cheerfully accept their criticism, as well as their 'good enough principle'") but not entirely ("although I am not convinced that their specific *statistical* implementation of the principle is as helpful as they think" [p. 108]). Looking back on this exchange, one thing is clear: For all his criticism, Meehl never gave up on psychology. If he could not abide the good-enough principle, he worked just as hard to fortify hypothesis testing, so that psychological theories could be put to severe tests. That's what we wanted, too. For his part, he proposed a "Lakatosian method" for appraising and amending theories and also indices to gauge when theories have a good track record (Meehl, 1990). He also touted a path analytic procedure to appraise the verisimilitude of theories (Meehl & Waller, 2002a, 2002b).

In these ways, I like to think we were playing on the same team. And I think Meehl would endorse the central theme of this chapter: that meta-theoretical perspectives—certain writings in the philosophy of science, for example—could be enormously helpful in showing scientists the way during moments of theoretical crisis, scientific revolution, or in simply appraising growth and degeneration in research programs. Of course, he would put it in his inimitable way: "Much of scientific thinking is of poor quality and it could be improved by explicit metatheoretical education" (Meehl, 1993, p. 707). That's the point of this chapter, too—that applying meta-theoretical frameworks to one's work is useful for generating ideas and formulating the problematic of a research program. I hope to illustrate the usefulness of the Lakatos methodology of research programs by two examples, below.

Moral Stage Theory

While we were working out how best to use Lakatos against Meehl, we came across another paper that used the Lakatosian framework to appraise "Kohlbergian moral development." Phillips and Nicolayev (1978) argued that Kohlberg's moral stage theory was a degenerating research program that was held together by a patchwork of ad hoc maneuvers that fended off criticism but failed to increase the empirical content of the theory. They write, "In the final analysis, then, the philosophers of science must come to endorse the conclusion reached by common sense—the Kohlbergian program is degenerating and has little recognizable merit" (p. 300).

Phillips and Nicolayev (1978) came to this swaggering conclusion (apologies to Denis Phillips—it seemed so at the time) by first identifying the hard core of the Kohlbergian research program and then by analyzing how it stacked up against the evidence. They identified three hard-core commitments: (1) Moral judgment development unfolds through stages; (2) the stages are in constant order of succession, that is, the sequence is invariant; and (3) the sequence is vouchsafed by logical necessity, that is, a higher stage logically presupposes a lower stage (through hierarchical integration). Each claim was found wanting in turn. The claim that stage succession is carried by logical necessity was dismissed as unsubstantiated (since logical truth has no empirical content) or absurd (as matters of logical truth require no empirical demonstration).

The claims for stage and invariant sequence were dismissed on empirical grounds. For example, the complex process of stage assignment is too vexed to make unambiguous judgments. In their own research, the Kohlberg team report wide variability of mean stage scores, some individuals show stage mixture, and some show stage regression. "The absurdity of this hardly needs to be pointed out," Phillips and Nicolayev (1978, p. 294) noted. Moreover, even one case of variance (regression) would be enough to refute the theory, so a study that reported 20% of participants regressing to a lower stage should have been treated as a stunning failure of the theory. But the Kohlberg team did not regard it as a refutation, and that was a pity.

Phillips and Nicolayev (1978) identified several maneuvers in the Kohlberg research program that functioned as a protective belt but found them thin and unavailing. One maneuver was to fiddle with the stage scoring procedures, possibly "to give results required by their theoretical assumptions" (p. 295). Another strand of the protective belt was woven out of evidence of stage regression. Instead of refutation, the finding of stage regression was crafted to make it evidence for a transitional stage 4½. But the authors deemed this an *ad hoc* hypothesis that did not increase the empirical content of the theory. A third maneuver was to use *ad hominen* arguments for why results went awry. For example, stage regression was blamed on study participants—some were mentally ill, persons over 65, or incarcerated criminals. A fourth was to use confusing presentation of study findings or to conflate tendency claims with universal claims, while a final protective maneuver was a strategy to seek evidence of confirmation rather than refutation.

Phillips and Nicolayev (1978) concluded that "there is good reason for believing that the hard core of the Kohlbergian research program is implausible" and that the protective belt "seems unable to restore the credibility of the hard core" (p. 300). Kohlberg's research program fails the Lakatosian requirement for continuous growth. In the face of anomaly and prima facie refutation, it was "patched-up with a series of 'pedestrian empirical adjustments'" (p. 300) that weaken its case for acceptability as a legitimate scientific theory.

This was unwelcome news for a new doctoral student just learning the foundations of the cognitive developmental tradition, so Ron Serlin and I (Lapsley & Serlin, 1984) set about mounting a Lakatosian defense of the Kohlberg program. We argued that Phillips and Nicolayev misidentified the unit of analysis: It was not Kohlberg's theory, in isolation, that constituted a research program, but rather the cognitive developmental approach to moralization, of which Kohlberg's theory was a content-increasing improvement over predecessors. "The proper question (for the moment) is not whether the revisions of Kohlberg's theory since 1958 are content increasing. Rather the proper question is whether Kohlberg's theory is content increasing vis-à-vis other psychological theories of morality" (Lapsley & Serlin, 1984, p. 161). On this score, we argued that Kohlberg's theory was content increasing when the proper comparisons are made.

We defended the Kohlbergian hard core by noting that the positive heuristic of the cognitive developmental approach required a stage model, but not any particular stage model. The development of alternative stage models was already proceeding apace, as was revision of the Kohlberg theory. For us this meant that "This research program is actively solving problems and digesting anomalies" (Lapsley & Serlin, 1984, p. 164). We objected to the notion that even a single instance of regression should refute Kohlberg's theory. This was not countenanced by Lakatos's method of appraisal. This was naïve methodological falsificationism. In Lakatos's view, there can be no refutation before the emergence of a better theory, and none was on the horizon. Moreover, theories unfold in a sea of anomalies, and mere criticism is insufficient cause to throw one overboard. We tried to clarify Kohlberg's muddled use of "logical necessity" in his account of hierarchical integration—he could only have meant *theoretical* necessity. That is, the sequence of stages is a research hypothesis that results from a structural analysis of moral judgment with ordering criteria that devolves from cognitive developmental theory.

We were critical of the Phillips and Nicolayev (1978) account of the evidentiary status of the hard core. For one thing, they argued that Kohlberg's hard core was, by turns, at risk, untenable, refuted, and unintelligible. We thought this was a misunderstanding of what Lakatos meant by the hard core of a research program. The hard core, by methodological decision, is protected from recalcitrant evidence. We wrote,

> It is quite impossible for the hard core to be at risk prior to the collapse of the protective belt, prior to the thorough testing of appeals to *ceteris paribus,* prior to the inability of the positive heuristic to anticipate novel facts and, importantly, prior to the emergence of a better theory. (Lapsley & Serlin, 1984, p. 167)

We were also critical of the Phillips and Nicolayev account of the protective belt. The authors did not consider the protective belt to be a set of substantive auxiliary theories that bore the brunt of refutations in the evolution of a research program. Rather it was more like "disingenuous tricks and mirrors" (as we put it) that involved methodological chicanery (variation in the scoring method), obfuscation and deception (ad hoc and ad hominem arguments, inadequate presentation of data, inaccessibility of key manuscripts), and outright intellectual dishonesty (conflation of universal and tendency claims, seeking confirmation rather than refutation). We doubted this is what Lakatos had in mind when he considered the role of auxiliary theories in protecting the core commitments of a research program. In the end, we concluded that Kohlberg's theory was a progressive problem shift in the study of morality and that the cognitive developmental approach was a progressive research program "not in present danger of degeneration" (Lapsley & Serlin, 1984, p. 169).

That was then, and what a difference a couple decades make. I take it all back now. Although I do not retract the general defense of Kohlberg in this paper (although specific claims now seem labored or incorrect), nor do I retreat from at least some of our criticism of the way Lakatos was used by Phillips and Nicolayev (but certainly there was no intellectual dishonesty), I do not stand by our conclusion that Kohlberg's is a progressive problem shift, or that the cognitive developmental tradition is a progressive research program. On this score, Phillips and Nicolayev were vindicated in the end.

It is now clear to me, at least two decades after our skirmish, that modifications (e.g., stage 4½, A and B substages) were designed to fend off strong countervailing evidence with respect to the reality of moral structures and moral stage sequence, and that these modifications were ad hoc stratagems that turned the theory into something cramped and narrow, on the margins of developmental science, and unable to engage alternative models of intellectual development or moral psychology (Lapsley, 2005). In full accord with Lakatos's methodological falsificationism, and with the prescient conclusion of Phillips and Nicolayev, one could abandon the Kohlberg stage theory for just cause.

Adolescent Egocentrism

In our analysis of moral development, we appealed explicitly to Lakatosian categories to appraise growth and degeneration of the Kohlberg research program. It is an exercise that I recommend to any researcher looking for ideas about the next step in the evolution of a research program, for undertaking a literature review, or framing the significance of a line of research. The Lakatosian categories also come in handy when defending one's work, perhaps for a dissertation defense or when one's manuscript is making its way through a peer review process.

In my second example, the Lakatos framework provided more implicit guidance for framing research on one of the venerable topics in all of adolescent psychology, which is the notion that young adolescents are beset by a cognitive egocentrism associated with formal operations. This was the famous claim by David Elkind (1967) in one of the most cited papers of all time, a staple in every adolescent development textbook.

Elkind (1967) argued that when youngsters begin the transition to formal operations, there is a period of over-assimilation that gives rise to egocentrism. *Egocentrism* is a Piagetian concept that refers to a lack of differentiation between some aspect of self and other. The twin pillars of formal operations include the ability to think about possibilities and the ability to think-about-thinking, which is sometimes rendered as thinking introspectively about the self. So when the self-absorbed adolescent begins to wonder what is possibly on the mind of others, he thinks they are thinking *about the self!* They commit a differentiation error—adolescents fail to differentiate between what is of concern to them (which is the self) and what are the concerns of others. Adolescent egocentrism is the conviction that others are as concerned about the self as much as is the adolescent. It results in two kinds of ideational constructions: Adolescents construct imaginary audiences and personal fables. They believe they are on stage while everyone else is the audience. They construct fables about their invulnerability, their exquisitely sensitive inner qualities and personal uniqueness. One must be quite special indeed if the whole school is buzzing about you.

I was introduced to this theory by my first mentor, Robert Enright, back around 1980. I found it highly attractive for two reasons. First, it seemed to account for a wide variety of typically observed adolescent phenomena. The construction of imaginary audiences seemed to account for teenage self-consciousness; for characteristic affective concerns about shame, shyness, and embarrassment; for show-off and exhibitionistic behavior. Personal fable ideation seemed to account for risk-taking behavior and for feelings of personal uniqueness. Second, there was elegance to the theory that was highly appealing. These ideations were explained in terms of the very powerful Piagetian framework and so traded on its prestige and authority. And the immediate recognition of the two constructs by undergraduates when I taught the theory seemed like a corroboration of sorts. I found the theory irresistible.

But the absence of empirical research was puzzling. The theory was proposed in 1967, but empirical research had not yet appeared by the late 1970s, possibly because there were as yet no suitable assessments of the core constructs of the theory (or perhaps the constructs were so recognizable that no research was thought necessary).

Bob Enright and his team were the first to construct an "adolescent egocentrism-sociocentrism" scale that included items tapping the imaginary audience and personal fable constructs. This scale was used in cross-sectional studies whose results were generally favorable to the theory (Enright, Lapsley, & Shukla, 1979; Enright, Shukla, & Lapsley, 1980). Elkind followed up with his own Imaginary Audience Scale (Elkind & Bowen, 1979), and in just a few years, the empirical record was expanding at an encouraging rate. But extant research had not yet tested the crucial theoretical assumption of the theory, namely, that formal operations had something to do with the appearance of these twin ideations. My research team and others took up this question, and the evidence was not encouraging (Goossens, 1984; Gray & Hudson, 1984; Lapsley, Milstead, Quintana, Flannery, & Buss, 1986).

Partly as a result of this discouraging evidence, I started to have doubts about the theory. Being a good Lakatosian, I understood that research programs (1) have to be appraised from a comparative-historical perspective; (2) chug along in spite of inconsistency, anomaly, and prima facie refutation; and (3) cannot be refuted before the emergence of a better theory. By 1985, I was convinced that the hard core of the research program was that adolescents construct imaginary audiences and personal fables. I was equally convinced that cognitive egocentrism had nothing to do with it and that formal operations is not the optimal theoretical framework for making sense of these ideations.

But I needed a better theory. My first attempt made the case for social cognitive development as the proper grounding for imaginary audiences and personal fables (Lapsley, 1985; Lapsley & Murphy, 1985), in particular, the movement of youngsters to Level 3 in Selman's theory of interpersonal understanding (Selman, 1980). This view was augmented by a larger claim that the two ideations were bound up with adolescent separation-individuation (Lapsley & Rice, 1988), for which we reported some encouraging corroboration (Lapsley, FitzGerald, Rice, & Jackson, 1989). I came to this insight after reading Peter Blos's (1962) highly absorbing account of the phases of adolescent individuation. According to Blos, the adolescent copes with mourning reactions and the loss of object relational ties by means of a "narcissistic restitution strategy" that seemed identical to Elkind's personal fable ideation. And the adolescent attempts to hold onto object relations by means of "object relational ideation" that seemed nothing less than Elkind's imaginary audience (Lapsley, 1993; Lapsley & Rice, 1988).

Hence the alternative theory I was building attempted to account for the adolescent egocentrism constructs, but now in terms that were broadly integrative with social cognitive and ego development, and without the anomalies that attended egocentrism explanations (Lapsley & Murphy, 1985). Here is how I described the emerging "new look"—and note the Lakatosian motif:

> Hence the model being advanced here can be thought of as a theoretically progressive research program insofar as it accounts for the anomalies of the earlier [egocentrism] theory and has generative power to anticipate novel lines of research. The new look appears to be an empirically progressive research program as well, inasmuch as some of its theoretical content has been corroborated. (Lapsley, 1993, p. 571)

The expansion of theory included the claim that personal fable ideation was a plural concept that includes subjective omnipotence, invulnerability, and personal uniqueness, and that each construct required independent assessments, a line of research that we have undertaken. We have shown, empirically, that these constructs are differentially related to adaptation. In one study, for example, we showed that subjective omnipotence was strongly associated with narcissism and with numerous indices of adjustment, including self-worth and mastery coping; that invulnerability predicted risk behavior and positive adjustment; and that personal uniqueness was a strong predictor of internalizing symptoms, particularly among girls (Aalsma, Lapsley, & Flannery, 2006). In another study, we showed that our measure of "danger" invulnerability was a more robust predictor of risk behavior than was a rival decision-making construct called "optimism bias," and that our measure of "psychological" invulnerability was negatively associated with a range of adjustment problems (Lapsley & Hill, 2010). Our more recent work is making the case that subjective omnipotence, invulnerability, and uniqueness are manifestations of the grandiosity-vulnerability narcissism phenotype, and that our assessments of these

constructs are viable, theoretically derived measures of normal adolescent narcissism (Hill & Lapsley, 2010; Lapsley & Stey, in press). These constructs have come a long way since Elkind's adolescent egocentrism, and whether any of it represents a progressive problem shift awaits the judgment of the history of science.

CONCLUSION

This chapter started with a reflection on discovery and justification. It was shown that much could be said about the context of discovery—how to develop and frame meaningful problems—than is typically realized. What is needed are well-attested strategies for placing strategic bets, and if one is to exploit the context of discovery, one will need to be conversant with meta-theory and philosophy of science. These regulative principles help the scientist articulate arguments, maxims, and heuristics and train our vision to see better and more clearly. I have profited from Lakatos's powerful philosophy of science as a way to frame meaningful problems in fields of study of interest to me, and I have tried to show where and how the deployment of Lakatosian categories were particularly helpful.

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5

INQUIRY THROUGH A KEYHOLE

Retroduction

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Both Clif Conrad and I, the editors of the *Handbook*, teach courses that describe and delineate research methods—one of us introducing methods to be used in what one might typically define as a qualitative research tradition, the other teaching procedures thought to be more appropriately applied under a quantitative research rubric. Notwithstanding differences in our syllabi, we both present research methods as tools to be used in the process of sifting and winnowing ideas in educational inquiry. In turn, we emphasize that it is the researcher's overall question that should drive the research enterprise—a compelling idea that has personal as well as professional significance.

We often stress to students and colleagues that the methods we teach in our courses, as important as they are to scientific research, are techniques used to acquire information that are akin to instruments used in astronomy. They allow us to see beyond the haze of the atmosphere to discern phenomena that might have otherwise gone unnoticed. But even an enlarged and clearer image often does not explain what is observed. The thrust of the research and the interpretation of findings must always go back to the animating questions of interest.

Conducting educational research that is both consequential and rigorous is intellectually demanding work. Most significant, the subject matter in education—with schooling and its effects on student learning at the epicenter of the field—is inherently difficult and challenging to study. Moreover, scholars, policy makers, practicing educators, and the public at large often have strong and competing views on the worth and rigor of the research that has been conducted. Despite these obstacles, researchers continue to make impressive contributions to our knowledge and understanding of education. Still, meaningful and first-rate research—across all subfields—is very much needed if we are to significantly advance and deepen our understanding of education.

Our students—and our faculty and practitioner colleagues—often ask us how questions of interest are generated and how they, as scientists themselves, can come up with one or more. Clearly, this is a major mentoring challenge to us as faculty. We variously suggest that they immerse themselves in the prominent research journals in their field; see what problems are being addressed; and reflect on how the frameworks and theories guiding the research help to explain the corroborating results, are invented to explain anomalies, and help to develop and improve educational programs. We then opine that they should read the publications referenced in the

To Julia Conrad and Susan Anderson

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