Section 3
Challenges in Formulating and Framing Meaningful Problems

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Section Editor’s Introduction

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, 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, 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.

Karl Popper and Vienna Circle of logical positivists could claim some credit for this state of affairs. “The work of the scientist,” Popper asserted, “consists of putting forward and testing theories” (Popper, 1959, 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 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 towards the back end of justification, appraisal and evaluation.

Yet the distinction between discovery and justification, like other venerable antimonies (e.g., fact-value, is-ought), has fallen on hard times. There is a consensus 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 not irrelevant for theory appraisal. As Kelly (1987, p. 441) put it, “When the one sort of procedure squeezes through the door, the other is difficult to exclude.” 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; 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. But 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 are 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 acceptability. 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” that 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 (1972) 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 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 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 when new theories should be constructed and the manner in which the whole program should deal with empirical refutations” (Zahar, 1983, p. 244). Indeed Lakatos (1978) argued that all research programs have heuristics that give direction to the progressive elaboration of its 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 tradeoffs 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 “non-scientific 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.

These themes are in evidence in the chapters of this section. Three renowned scholars, James Youniss, Kathryn Wentzel and Susan Harter, take up the problem of how to formulate and frame meaningful problems. The authors were invited to discuss fundamental challenges in doing first-rate research, propose strategies for overcoming these challenges, and to discuss rationales for their decisions. 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 through narrative, vignettes, accounts of critical incidents and key decision-making points that provides the prism through which the authors describe their wrestle with meaningful problems.

James Youniss examines the problem of how to situate one’s inquiry from a vantage point that is deeply reflective of his distinguished career. The biographical narrative is not mere reminiscence but instead points to an inescapable fact: that the problem of “situating inquiry” and of
“situating the self” are not two different sorts of activity, and that, indeed, one is parasitic on the other. To understand the narrative of one’s intellectual formation is to reveal the problematic that brings it meaning. Situating inquiry requires interpretive frameworks but these emerge from a rich and varied personal experience. The starting point of critical discovery, then, is biography. A first-person account is required to capture its regulative principles.

And what do we learn? The problem of situating must be pursued intentionally. One must seek out colleagues, engage new ideas, attend colloquia, form study groups and work collaboratively in both formal and informal settings. A policy of intentional engagement and collaborative inquiry is of 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 short cut to expertise, but then 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. But 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. This was Gutting’s suggestion, as noted above, yet 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.

Kathryn Wentzel takes on the problem of how to develop and nurture interesting and researchable ideas. For Wentzel interesting ideas that are researchable have two qualities: they hold personal interest, and this is critical to sustain research effort over the long haul; and they are interesting to the research community, to educators and practitioners, or to those who set policy. She advocates three specific strategies for generating interesting ideas: identify and challenge theoretical assumptions, document the published literature, and generate new variables by utilizing the person-process-context features of a developmental systems model. The use of these strategies is illustrated by examples from Wentzel’s research on goal setting (challenge assumptions), peer relationships (document the literature) and teacher caring (generate variables).

Of course one gains facility in executing and profiting from these strategies to the extent that one is sufficiently expert in the relevant background literatures. In each of her examples notice what is the opening move regardless of the strategy: an extensive and deep reading of the literature. In addition to expertise Wentzel suggests that talent at generating ideas is also a matter of persistence and commitment to task. It requires cultivating an ability to frame an argument so that it can be sold to skeptics and doubters. She writes “It is necessary to explain ideas in the language of other researchers, describe them in ways such that they become extensions of what has come before, and articulate ways in which other perspectives might contribute to their further development.” In other words, interesting research ideas are those that contribute to an elaboration of a research program by accounting for settled facts of rival ways of framing a problem and by anticipating novel facts, some of which are corroborated. Interesting research ideas are both integrative and extensive, which are criteria by which Lakatos (1978) defines a progressive research program.

There are common themes in the chapters on situating ideas (Youniss) and developing and nurturing them (Wentzel), and both chapters illustrate key aspects of critical discovery noted by Gutting (1972) and Kordig (1978). 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). Both illustrate key features of the philosophical analysis of critical discovery noted earlier. For example, Youniss illustrates Gutting’s point that knowledge of meta-theory and philosophy is foundational to situating inquiry, particularly when paradigms are rattled by winds of change. In turn Wentzel shows how pragmatic heuristics of critical discovery can be understood in terms of intentional strategies. In addition she shows that interesting and researchable ideas are just those that are framed as
progressive elaborations of research programs, a point that underscores Kordig’s (1978) claim that arguments suitable for plausibility assessment on the front end of discovery are required as well for judgments of acceptability on the back end of justification.

Susan Harter’s chapter on the challenge of framing a research problem brings closure to this section. Where do we turn to frame a problem worthy of study? Too often, Harter notes, we turn our first gaze in the direction of methodology rather than of theory. We slog away using our favorite measures or paradigm. We cut our pet ideas thin but spread it wide. She deplores the tendency to reject theoretical perspective wholesale insofar as these theories, or some reconstruction of them, are often the source of new perspectives or integrative possibilities; and, besides, cultivating such ruthless dismissive tendencies discourages one from stepping up to the theoretical plate in one’s own right. It dampens enthusiasm for trying out new ideas, even if ill-formed. Unlike the physical sciences, where new but presently untestable ideas are given a respectful hearing for quite a long time, the social sciences are more demanding of instant rationality of its theories, which perhaps explains why there are no grand theories of much of anything anymore.

Harter appeals to her own innovative and productive research to illustrate how she responds to the challenge of framing research problems. Sometimes historical frameworks (e.g., William James’s notion of the self) can be exploited with profit. Sometimes theoretical perspectives that explain adult functioning have to be turned on its head to account for developmental and individual differences. Or else a construct must be assessed differently at different developmental levels. Her work on multiple pathways to low self-esteem illustrates the value of not assuming that group “main effects” always applies to subgroups. Her research on imaginary friends, multiple emotions and multiple selves uses clinical material to inform normative developmental processes. Sometime outcomes that are disconfirming and counter-intuitive, and serendipitous findings, are worthy of pursuit, as are instances suggested by real world events (e.g., school shootings) and trends in culture (e.g., physical aggression among girls; cultural importance of physical attractiveness and internalizing symptoms and eating disorders). These and other examples are marshaled to make this point: that the challenge of framing a problem is bound inextricably with what is considered one’s “burning question.” The starting point is personal and biographical.

So with Harter’s chapter the section comes full circle. 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. All three chapters call for a reflective appreciation of historical frameworks, meta-theory and paradigms. All three chapters make demands upon researchers for expertise; invoke intentional strategies to see clearly and differently; 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, all three chapters show that the questions that motivate scientific inquiry are often deeply rooted in biographic 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.

References


