

THE IMPACT EXPANDING THE POPPERIAN LEGACY OF CRITICAL THROUGH THE WORKS OF IAN C. JARVIE RATIONALISM

EDITED BY RAPHAEL SASSOWER & NATHANIEL LAOR



The Impact of Critical Rationalism

Raphael Sassower • Nathaniel Laor
Editors

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Expanding the Popperian Legacy
through the Works of Ian C. Jarvie

palgrave
macmillan

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ISBN 978-3-319-90825-0 ISBN 978-3-319-90826-7 (eBook)
<https://doi.org/10.1007/978-3-319-90826-7>

Library of Congress Control Number: 2018941747

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Cover designed by Thomas Howey

Printed on acid-free paper

This Palgrave Macmillan imprint is published by the registered company Springer International Publishing AG part of Springer Nature.
The registered company address is: Gewerbestrasse 11, 6330 Cham, Switzerland

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CHAPTER 1

Introduction: The Legacy of Ian C. Jarvie

Nathaniel Laor and Raphael Sassower

Ian C. Jarvie has suggested that “All that there is the flux of experience, bewildering and incoherent as it presents itself to us, and there is our yearning to give it order and thus to be able to enjoy it, to no longer be afraid of and threatened by it” (Jarvie 1981, p. 234). “I take it that all human encounter is self-making, and human encounter with the ideas and attitudes of others can be self-transcending. But this is an arduous and precarious endeavor that can leave us disillusioned or mad, as easily as enlightened and renewed” (Ibid., p. 254).

Jarvie is one of the leading philosophers of the social sciences of our age. In the early 1980s, while engaged in an ascending career, he described himself as a professional in an unresolved identity crisis. He seems to have experienced the crisis ever since he underwent two adjacent constitutive experiences. Maurice Freedman, the late chair of the Department of Social Anthropology at the London School of Economics, rejected his proposal

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R. Sassower, N. Laor (eds.), *The Impact of Critical Rationalism*,
https://doi.org/10.1007/978-3-319-90826-7_1

for a dissertation in the PhD program in that department. And the philosophers, Sir Karl Popper and John Watkins, approved of that same proposal for a dissertation in the PhD program in the philosophy department. He wrote his dissertation under their tutelage—in *Scientific Method*. Since this volume is presented to him on the occasion of his 80th birthday, at the height of a long and extremely fruitful lifework, and since he found this identity crisis important enough to be aired in an intellectual-*cum*-autobiographical essay on anthropology and the irrational, it is befitting that we ask in an introductory mode what was the crisis about and what impact it had on his rich output. Did it have a pivotal driving force in the development of his philosophical world of individual and social problem-solving and of the engagement with ideas in general?

The essays in the present volume grapple with a tour de force, namely, the ideas of a methodologist who accomplished two simultaneous feats: first, turning his personal crisis into a professional tool, and second, introducing Popperian insights in the philosophy of science writ large into particular studies in the social sciences (and of necessity in aesthetics too, as he was studying art as a social phenomenon). It is very beneficial to apply openly and critically the social domain and to the world of culture Popper's proposals, including the one to consider all activity as problem-solving (and not searching for justifications). This invites improvable explanations and ongoing rethinking. Possibly, this is why Jarvie has focused since his early studies on social and intellectual ideas also in aesthetics and practices in flux (e.g., theories of cargo cults) that are in crisis and always changing. He viewed them as challenging to and therefore offered his own criticism of the (structural-functionalist and symbolist) homeostatic theories of society as either Polanyi-style or Kuhn-style authoritarian and traditionalist theories of intellectual activity and their social organization. In doing so, he was able to challenge the orthodoxy of his day and critically examine the dynamic nature and internal logics of such mysterious, mystical, or even magical cultural phenomena. The ideas of relativism and idealism loom behind all these professional stances, and in this regard Jarvie is the champion of universalism and realism as part and parcel of any critical philosophy. From this perspective he ventured to offer a theory of social change.

Jarvie discusses the intellectual environment that rests on dogmatism and that demands conversion to these dogmas as conditions for entry into it. In Popper's view this is what renders these societies more closed than open. They both find these ideological environments hindrance to

intellectual curiosity, to critical examination, and to the development of the rational spirit of scholarship and of policy-making. Hence, contrary to the prevalent trend of the day, and contrary to Popper's proposals, Jarvie entertains an unusual, dual stance vis-à-vis magic, both in his anthropological studies and in his studies of rationality. He recognizes the place of magic in all human societies and applies to it his method of study: he does not equate the admission of its very existence with the admission of its claim to be the right way to solve problems or as the only way to understand human thought, behavior, and social relations. Rather, in the spirit of the expansion of the notion of rationality, he proposes (with Agassi) to consider rationality a matter of degrees, as a spectrum that ranges from the very strict to the somewhat lax, but still within the realm of rational discourse.

Has Jarvie met his own challenge to offer a theory of social change? Our answer is in the affirmative: Jarvie improves on Popper's theory of social change. The question is, are new social problems and solutions negotiated on the way to implementing social change. While Popper focuses on problem-solving within given problem situations, he allows for but does not explain how these emerge. Jarvie does: he offers recommendations for gradual implementation (piecemeal social engineering is the phrase commonly used) of new social ideas with a careful attention to their interactions with the problem situations that give rise to them. Popper underscores the inevitability of unintended (positive or negative) consequence of such processes and therefore insists on the trial-and-error approach, acknowledging human fallibility. Jarvie, in his turn, explains how, within the framework of Popper's methodological individualism, various individuals negotiate—clash, criticize, and tentatively agree upon—a plan for social action, given the different maps that they have outlined as cognitive representations of society as a whole and the social relationships that they embody. According to Jarvie, the unintended consequences are the very subject matter of the social sciences; unlike the natural sciences where we study inanimate objects or objects with no autonomous volitions, the social sciences deal with beliefs and anxieties, feedback loops, and mirroring effects. Within Jarvie's account of the workshop rationality (2005), very much the way he encountered it in Popper's seminar. (Jarvie describes the workshop mentality practiced there not as a personal experience but as a theoretical framework.) Jarvie's accounts of social process present them as capable of being thoroughly and inherently democratic. In doing so, he offers generations of scholars and educators a method of individual behavior (and social modeling) that is at once also a central constituent of the open society Popper-style.

Jarvie's contribution has closed a circle. His idea of the workshop rationality, when implemented within a given institutional setting, allows for the democratic critical examination and change of social standards, not only of the occasional change of individual behavior. Likewise, his proposed method for anthropological study recommends viewing individual encounters with others (whether other scholars in the commonwealth of learning or the targeted groups that we observe in remote regions) as potentially challenging the very commitments with which our encounters begin; when made operationally critical, this method also serves as a meta-commitment to identity—of both actors and those who observe them. Meta-commitments (as scholarly presuppositions or as intellectual prejudices), too, may be challenged and with them, the identities of all related participants. This way, perhaps, Jarvie's professional crisis has given rise to a philosophical-*cum*-anthropological extension of the methodology of anthropology: he indicates what change it requires to fall within the purview of the workshop mentality. He does this by prescribing that students of social and intellectual problems live on the edge: always ready to make a decision to endorse a hypothetical stance vis-à-vis certain descriptions and explanations, without giving up on their universalism, pluralism, and realism. (Note that Jarvie's pluralism rests on the varieties of particular perspective of individual thinkers facing particular problems as well as on the particular social/intellectual system under study. By contrast, the pluralism of the anthropologist rests on the particular singularity of the systems they study while overlooking their universal aspects.) Jarvie's self-professed personal and professional detachment has shown up as thoughtful humaneness and intellectual involvement, a method that transcends the standard interpretation of the Popperian methodological individualism and piecemeal social engineering in that it prescribes attending to the balance between the particular and the universal in a predicament as well as between the rational and the irrational parts of reasoning within the given system. Thus, whereas Popper proposes to shun utopianism for fear of historicism, Jarvie both recommends a utopian aspiration free of historicism and the study of historicist societies.

Jarvie reports that he has started out with a skeptical personal bent and that he found in Popper's philosophy a proposal that could be applied as an exploratory tool. Such a transition from the social to the cognitive could be construed as a matter of conversion, Jarvie's stress on autonomy and individual decision-making in term of his own intellectual development notwithstanding. Could one's intellectual choices within a given

social milieu transcend one's personal bent? This question has imposed itself on Sigmund Freud. His answer to it was in the negative. He dismissed all philosophy as but a projected metapsychology. Whether a social philosophy is a projection or an excuse for anything, Jarvie suggests we ignore its origin and the intention behind it and try to take it literally, so as to judge its merits and defects as impersonally as we can. He ventures to underscore the reciprocal critical contribution of individuals and their culture. Could Jarvie have chosen a different theory of critical rational bent? In at least one essay (in collaboration with Agassi) the answer is that it boils down to the choice among various lifestyles. Jarvie's claim is that commitments and beliefs ought to be entertained critically and thus always tentatively; but they should not be ignored by social thinkers or deemed irrelevant to their social investigations.

Let us note that the ideal of covering all options within social theory is universal. Tradition rejects all false hypotheses. Popper suggested social researchers offer hypotheses to explain social phenomena. These may fail to explain. If they do, then they may be false yet untestable; he suggests we try to render them as highly testable as possible. Refuted hypotheses have no room in classical methodology but are significant for Popper. A hypothesis that signifies in Popper's system ascribes full rationality to individuals; however, Jarvie allows for a partial rationality. This makes his system the first inclusive system: it allows for more hypotheses than any other system, and if testable, then they count as empirical, and as such it invites social research to cover more facts than any system before it.

Jarvie's style of intellectual and social involvement has benefitted us all, from his role as managing editor of one of the most erudite and critical journals, *Philosophy of the Social Sciences*, to his social commitment to the Popperian community of scholars, initiating, convening, presiding, presenting, and editing volumes of papers given in our central conferences, and in his initiating and editing *Festschriften* for his colleagues (Watkins, Gellner, and Agassi). In addition, Jarvie is an avid scholar who has not left to others to explore the implications of his own ideas, and his areas of interest carry their mark. Perhaps the record of Jarvie's output explains the variety of perspectives and topics covered in the present volume. Jarvie has published 13 single-authored books, from *The Revolution in Anthropology* (1964), *Movies and Society* (1970), *Concepts and Society* (1972), *Rationality and Relativism: In Search of a Philosophy and History of Anthropology* (1984), *Thinking about Society: Theory and Practice* (1986), and *Philosophy of the Film* (1987) to *The Republic of Science* (2001); he has co-authored

and co-edited 16 volumes, from *Hong Kong: A Society in Transition* (1970, with Agassi), *Rationality: The Critical Approach* (1987, with Agassi), and *Critical Rationalist Aesthetics* (2008, with Agassi) to the three-volume *Karl Popper: A Centenary Assessment* (2006, with Miller and Milford) and *The Sage Handbook of Philosophy of the Social Sciences* (2011, with Zamora Bonilla). In addition, Jarvie authored and co-authored some 61 essays, 52 book chapters, 28 film reviews, and 108 book reviews and essays. This level of intellectual engagement that covers everything from film and pornography to the methodology of science and the legacy of Popper testifies to a fertile mind, disciplined professional, and a collegial collaborator. No wonder that his own legacy is being acknowledged in a *Festschrift* of his own.

Above all, Jarvie is a creative and bold-thinker leader, modest in style and generous to colleagues and friends. His life course attests to the exemplary philosophy he has put before us. Even his critics recognize that their interlocutor is a worthy intellectual opponent, one who has left his mark on the academic landscape and the theoretical/critical appreciation of films/movies in society. And as you read the contributions to this volume, you will quickly realize how much respect Jarvie's colleagues, collaborators, former students, and academic fellow travelers have for him. They engage his work and ideas critically (which is, of course, the Popperian mark of respect), and they credit him with keeping a tradition alive: critical rationalism. What Jarvie (together with Agassi) managed to do is transform the debates of the Vienna positivists (of the period between the Two World Wars) all the way into the twenty-first century. And as we have all observed in the past five decades, while Popper's intellectual stock has risen, that of the others has not fared as well. The impact of this undertaking, as we have tried to explain, is not exclusively based on the merits of the methodology but has much to do with personal humility, intellectual integrity, and professional courage.

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PART I

Philosophy of the Social Sciences



CHAPTER 2

How Should Social Engineers Develop Critical Social Science?

Nimrod Bar-Am

SEEMINGLY UNRELATED QUESTIONS

Joseph Agassi and Ian C. Jarvie are acknowledged pioneers of the philosophy of technology. Half a century ago, they contributed to one of the earliest and most significant achievements of that field: securing its theoretical independence from the philosophy of science. Pure science and engineering, they convincingly argued, are distinct practices, approached by means of distinct methodologies and in light of diverging given standards of success. In particular, they explained, technological control is often achieved and improved by means of deliberate application of refuted principles and models, whereas scientific explanations ceaselessly seek unrefuted ones. Since notions of science and technology were then (and still are quite often) confused and conflated, and since Cold War governmental budgets were by that time shaping the course of academic fields, determining their fate, effort to secure such an incisive distinction was (and still is) a valuable public service carrying significant practical implications.

Let me return in brief to the arguments and conclusions that Agassi and Jarvie have offered. I do so here not because my elders require my aid in

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R. Sassower, N. Laor (eds.), *The Impact of Critical Rationalism*,
https://doi.org/10.1007/978-3-319-90826-7_2

recapitulating their message, but because it will allow me to highlight and suggest an interesting problem that may seem at first entirely unrelated and with which Jarvie has wrestled repeatedly throughout his long and fertile career: that of the possibly distinctive character of the social sciences, in comparison (and perhaps even in contradistinction) to the natural sciences. Since Popper urged social scientists to adopt the practices of modest social engineering in more than one respect, and even encouraged them to make use of patently false principles such as those on which modern economic theory is founded, the detailed distinctions between the scientist and the engineer that Agassi and Jarvie helped clarify may have sharpened Popper's message to the point where it invites the question of their dissent.

SCIENCE VS. TECHNOLOGY

The commonest blunder regarding technology, and one of the first to have been clearly noted by writers on the subject, well before Agassi and Jarvie have entered it, is its surprisingly persistent identification with applied science.¹ Let me set it quickly aside and return to the question of its persistence toward the end of this section: most of our technologies have little or nothing to do with science. Skills and crafts (techne) are acquired by exploration, practice, and imitation. Applied science, on the other hand, is the premeditated deduction of practical results from a well-formulated, testable theory. The Paleolithic species *Homo habilis* (literally "skillful man") was named so because it was believed that it commanded a distinctive set of practical skills that made it markedly distinct from its ancestors. Today we know this to be an oversimplification, since some of his evolutionary predecessors seem to have acquired a number of those skills (as have modern-day chimpanzees). But the general lesson to be learned here is obvious: technology made us who we are. It therefore almost always precedes science. Applied science, in contradistinction, and by definition, does not.

As many of the early writers on technology noted, it is quite natural that science should falter and totter after our crafts and our skills. Scientists aspire for refutable explanations with ever-growing precision and generality, and these often come with a price: precision, to name one, typically comes at the expense of successful action. Many of our behavioral skills, to take a trivial example, were acquired by tacit internalization of the behavior of others, as Ryle (1949) and Polanyi (1958) famously noted, and

rarely if ever have they been improved by a formal and rigorous deduction from a general and abstract theory of behavior. Were we to consult such a theory on our every move, in the hope of improving our behavior, we would quickly collapse.

The terrific metallurgist and historian of science Cyril Stanley Smith reminded us what an impressive variety of subtle manipulations of copper, iron, tin, lead, brass, bronze, and other metal alloys and solders have been known, practiced, and improved before any reasonable theory existed that could significantly contribute to these manipulations (Smith 1967, p. 59). Virtually all of the techniques of handling iron by means of fire that accumulated until the end of the eighteenth century, he noted, had been selected because they unknowingly, and yet often highly successfully, controlled the amount of carbon in it.

This is not to deny that in the past two centuries scientific knowledge increasingly contributed to the improvement of our technologies. It goes without saying that the pure scientist enriches the engineer's toolbox with computational tools as well as exploratory frameworks. But even today the general rule seems to be that exciting technologies are explored and tested mostly as improvements of existing technologies rather than as new and insightful applications of recent scientific insights as Derek Price (1968) noted. And it should be noted that the engineer often generously reimburses the pure scientist by hitting upon new challenges, by discovering unexpected puzzles, and even by suggesting original heuristic directions that could expand the pure scientists' metaphysical outlook. The atomic race, to take a worn out example, has had well-documented effects upon the development and popularity of the big bang theory.

And yet technology and science, although undeniably intimately entangled, clearly advance by means of distinct dynamics, in virtue of applications of strikingly divergent research methods, aiming for different goals (and in light of markedly dissimilar ideals). This was the point stressed by Agassi and Jarvie. The pure scientist seeks truthful explanations of reality, they observed, whereas the engineer seeks effective control over it (Agassi 1974, 1985; Jarvie 1974). This difference is of considerable significance, they added, because control can often be achieved with little or no understanding of underlying principles, and sometimes even by deliberate acceptance of false ones. Some parameters for progress adhered to by the scientist and the engineer converge: both the scientist and the engineer seek precision, clear problem situations, and minimization of surprise. But they diverge in almost all other respects: the engineer, for example, seeks

improved durability, reliability, comfort of use, and reduction of production costs, while the scientist seeks increase of explanatory power, simplicity, generality, and abstractness.

Significantly, then, unlike scientists, and with good reason, engineers focus little, if at all, on either explanation or truth. Consequently, one of the most interesting differences between the engineer and the scientist is to be found in their attitude toward refuted theories (Agassi 1985; Jarvie 1986, pp. 304–308). The paradigm case here is Newton’s mechanics since it underlies more practical technologies than any other scientific theory. Indisputably, its usefulness to the engineer is greater than that of the theory of relativity, which replaced it: we made it to the moon based on Newton’s theory many years after it had already been widely acknowledged as, strictly speaking, an incorrect description of reality from an exact and uncompromising scientific perspective. The scientist, then, insofar as she is a strict and steadfast seeker of the truth, abandons the refuted theory when it is refuted and because it is refuted. As a scientist, she should not settle for mere approximations. As an engineer, however, she would often prefer the refuted approximation to its more exact and refuted alternative. She would do so for at least two good reasons. First, as already noted, it is often easier to plan one’s moves and anticipate their results with the help of a simpler, even if refuted, theory. This is undoubtedly the case if we compare Newton’s theory to Einstein’s theory, but it is also true generally for the rather prosaic reason that the way to show any theory to be a mere approximation and to improve upon it typically proceeds through improving the resolution and accuracy of its description, and this, in turn, almost always increases the complexity of the description of phenomena significantly and sometimes even requires a new level of description. Second, refutation allows the engineer to know the precise limits of successful application of the technology that she uses. The limits of application are clearer in refuted theories exactly because they are refuted. These limits may be still unknown in artifacts that have been designed and built in light of theories that have not yet been refuted and which may well someday be refuted in ways and in contexts that have not yet been tested or even imagined. (And so the mere word “approximation” seems to suggest to us an explanation of the curious durability of the technology-and-applied-science blunder: it is not easy to transcend it because applied state-of-the-art science is no less applicable, and sometimes more applicable, once it becomes applied refuted science and so, strictly speaking, technology.)

The last but by no means least item that I wish to mention in this short summary of a vast literature on the differences between science and technology is ethics. Pure science, Agassi and Jarvie argued, is more or less a platonic effort and so is either morally neutral or very near so. Technology, on the other hand, by its very nature as an enhancement of our powers and our options is not; it cannot be considered as morally unbiased or inconsequential. Every choice the engineer makes thus is rife with social implications, and these are charged with moral repercussions. The mathematics and physics of projectiles or ballistics, for example, are more or less morally neutral. It can be used to study the success and failure of imaginary baseball throws. Not so its application in the service of the weapon industry. Consequently, pure scientists seem morally guileless or near so, but engineers as well as their supervisors are, and should be, legally accountable for their acts. A world which can be annihilated by a mere push of the button has a fundamentally different moral makeup from that same world before such an act was available: its inhabitants carry moral responsibilities and burdens that their predecessors did not and could not possess (Agassi 2003).

NATURAL SCIENCE VS. SOCIAL SCIENCE

Having set the record straight in the matter of Agassi and Jarvie on the science/technology distinction, we can turn to the matter of Popper and Jarvie on the social science/natural science distinction. This, however, is by far the more difficult and delicate task. Popper's views on this matter are all too often shrouded by the thick fog of his outstanding battle against historicism. Unlike Jarvie, Popper did not take up the theoretical question of the possible uniqueness of the social sciences directly, aside from his polemics with well-chosen archenemies of the open society. This point is made as a cautionary note to what comes next, for we are entering a treacherous hermeneutic ground. In particular, polemic contexts may easily mislead those who are hoping to extract from them neat theoretical frameworks which they were not designed to express. Indeed, they invite various rhetorical biases, most notable of which is overstatement of one's dissent by means of excessively bold demarcation contours and conceptual dichotomies. I have observed elsewhere what a high price Popper had paid when, for the sake of accentuating his dissent from his positivist interlocutors, he offered the regrettable and misleading assertion (which he himself so successfully condemned elsewhere) that certain theories (Marxism and

Freudian psychology) rather than certain attitudes (dogmatism and defensiveness) can be targeted by means of his demarcation criterion (Bar-Am 2014, pp. 693–696). Attempting to extract and reconstruct Popper’s view regarding the differences between the natural and social sciences from his polemical masterpieces, *The Poverty of Historicism* (1961) and *The Open Society and Its Enemies* (1945), offers similar opportunities for confusion of the rhetorical with the philosophical, by reader and writer alike.

Consider the classic Section 27 of *The Poverty of Historicism* (1961, pp. 105–119). Popper presents there a polemically powerful distinction between trends and laws. They are, he argues, *radically different things* (pp. 115–116; italics in the original). Physical laws, he explains, are genuinely universal statements, whereas historical trends and social generalizations (even biological law-like statements such as “All vertebrates have one common pair of ancestors”) are not. Despite their logical appearance, such historical generalizations are in fact existential statements expressing particular matters of fact, he concludes. This neat division between matters of fact which (by their nature? by accident?) seem to allow genuine law-like generalizations and those historical ones which (by their nature? by accident?) seem too particular (too deeply involved with the specifics of “initial conditions”) to be explainable by genuine and neat law-like generalizations facilitate Popper’s formidable attack against historicists, like A. Toynbee, who were attempting to derive (by some form of crude historical induction) long-term social predictions from historical progressions of events demonstrating at most historical trends. But it may also entice readers to inquire further about the general principles underlying this fine critique: Can there be social laws equivalent to Newton’s law of inertia? Can there be genuine law-like (nontrivial) universal explanations within the social sciences? If yes, where are they? Is the law/trend distinction useful for distinguishing the physical from the social? (Note that Aristotle’s contention that ‘all earthly objects that have been thrown into the air will fall back to earth’ should be regarded as an existential statement à la Popper, and yet it was clearly intended by Aristotle as a law of nature.) And if no social equivalents of Newton’s laws can be formulated, why not? What is the metaphysical reality that underlies this curious situation? Should we allow metaphysical stipulations and/or empirical conjectures to influence our logical analysis of statements? Are vertebrates and societies incapable of participating in genuinely universal explanations? Are “force” and “space” more general somehow than “custom” and “vertebrate”? How are we to recognize the seemingly universal from the genuinely so, the genuinely general law from

the (accidental) trend?² I could not find unequivocal answers to these questions in Popper. Polemically he may well be guileless: these fundamental questions are external to his immediate task which was critical. But, following Jarvie, readers may seek more than polemical guilelessness: they may seek the critical rationalists' philosophy for the social sciences.

It is for this reason that I regard Jarvie's attempts to reconstruct "Popper on the differences between the natural and the social sciences" (Jarvie 1982) as an admirable achievement. It is Jarvie's characteristically humble manner of politely inviting us to follow him beyond his mentor's shadow, without loudly boasting his originality. (Jarvie is easily the gentleman among us critical rationalists.) His mere acceptance of that task invites discussion of various possible ways of sharpening Popper's message, and so unavoidably he leads us to the delicate question of their possible dissent. When Jarvie generalizes his interpretation of Popper's polemical remarks into a philosophy of the social sciences, we are already treading in philosophical terra incognita. To take a fleeting, but by no means minor, example: unlike Popper (and following Agassi), Jarvie is unmistakably clear about his rejection of the nature vs. convention dichotomy (1986, pp. 7–10). In light of this clarification, the problem situation of seeking the uniqueness of the social realm in possible contradistinction to the physical realm has already been improved profoundly.

And it is here, I suggest, that Jarvie's outstanding inquiries into the methodological status of technology are invaluable, for they sharpen the close, many-faced, and sometimes rash analogy that Popper seems to have drawn (and recommended) between the social scientist and the engineer (often in contradistinction to the "pure" physical scientist). Popper, to be clear, had no explicit philosophy of technology. And so Jarvie's patient inspection of the differences between the engineer and the scientists invites open discussion of the theoretical limits of Popper's analogy. Why, asks Jarvie, should social scientists settle for piecemeal engineering when physicists freely discuss the state of the universe billions of years into the past or the future? (1986, pp. 328–351). Clearly, the fact that social scientists cannot justify long-term predictions by means of observed social regularities is irrelevant here, since justification is impossible anyways. Shouldn't utopian engineering, then, openly clear and critical about its known methodological limits, be just as acceptable as long-term physical predictions?

The most striking and distinctive feature of social explanation, Popper and Jarvie seem to agree, is that it presupposes goals, and goal-directedness. This feature is sharpened and clarified by Jarvie's discussion

of the uniqueness of technology. For, as we have noted, in contradistinction to purely theoretical physical science, engineering oozes with moral repercussions and responsibilities. Does this extend to social science, then? Certainly. But, can we not have social theories that are ethically neutral? Can we have social scientists that are ideology free? If not, how, if at all, should this influence the methods of social science? And how should we best approach immoral practices? Popper touches upon these crucial questions in brilliant footnotes of his *Open Society*. But Jarvie puts them at the center of our attention in and in unprecedented resolution. And since he finds it difficult to imagine that Popper intended to suggest that social scientists abandon their search for truth, for the sake of some form of pragmatic social control over short-term social phenomena, he endeavors to improve the obviously false principle of rationality (Agassi and Jarvie 1973).

CONCLUSION

I would like to conclude with a brief word of criticism. A reader's complaint really. Popper and Jarvie share an awkward observation about the uniqueness of the social sciences whose significance (outside of criticizing inductivists) is unclear to me and to the best of my knowledge was never properly clarified by them. My own impression is that it is either a reminder of positivistic speech habits (which is at times noticeable in Popper, though quite rare in Jarvie) or an expression of metaphysical absentmindedness or naiveté. I am referring to their claim that social phenomena (and thus social research) are somehow more involved in the presupposition of "initial conditions" than their physical parallels (Jarvie 1982). For Popper and Jarvie, this means that social phenomena are supposed to be somehow more particular and thus more accidental, perhaps even more chaotic in some subtle sense, or otherwise less liable to be successfully generalized in some sense that is intimately connected to the trend/law distinction, and so its metaphysical status is nebulous at best. Here, as elsewhere, Jarvie's explication of the social situation by means of its translation into the context of technology is extremely helpful, but it is helpful not just in sharpening his main point (that methodologically, this difference is minor) but also in exposing the problematic status of this contention.

It is with much commonsense that Jarvie explains it by noting, for example, that transportation and shelter challenges or food-seeking tasks around the world are far more context dependent than general speed and

distance calculations. And, he adds, since all technological artifacts are bounded by specific demands for precision, cost limits, and various time-frame restrictions, technology is more of a parochial affair than general physical quests. It is very difficult to comprehend what this contention amounts to, metaphysically speaking. Does it imply some general hierarchy of scientific abstractions? Is such a hierarchy implied by the trend/law distinction? Are abstract supply and demand equations less abstract than Newton's laws? Most importantly, is the science of attempting to explain the success and failure of engineers different somehow from the science of attempting to explain the physical nature of our world? A clarification of this point would prove helpful for the future development of critical rationalism.

NOTES

1. See, for example, the interesting early collection of essays *Applied Science and Technological Progress*, published by The National Academy of Sciences in 1967, and especially C.S. Smith's paper discussed in the following pages. See <https://books.google.co.il/books?id=BTcrAAAAYAAJ&printsec=frontcover&dq=inauthor:%22National+Academy+of+Sciences#v=onepage&q&f=false>.
2. Agassi (1971) argues that metaphysical frameworks render some generalizations of natural laws and other uninteresting trends. Would Popper concur? Let us assume that he would. Would this have led him to reformulate his abovementioned Section 27 of *Poverty*? It is extremely difficult to answer these questions.

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CHAPTER 3

On Economic Methodology Literature from 1963 to Today

Lawrence Boland

INTRODUCTION

Since the famous 1962 American Economic Association meeting's session on the "Problems of Methodology," economic model builders have rarely if ever talked about methodology in their published research. This is particularly so in the case of research journal articles. The question to be considered here is why this is so.

Obviously, I am not the first to try to explain why. Part of a 2001 issue of the *Journal of Economic Methodology* was devoted to explaining why. Bruno Frey (2001) argued that the disregard is due to careerism: promotion and tenure process and the refereeing process. Both demands leave little room for discussion of methodology. Wade Hands (2001) argued that methodology is of no interest to scientific researchers who think methodology provides rules. Other than the rule concerning testability, there are no rules in published economic methodology. In a 1994 issue of that journal, Tony Lawson (1994) argued that the reason for the disinterest in methodology is because economists think methodology is rooted in

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R. Sassower, N. Laor (eds.), *The Impact of Critical Rationalism*,
https://doi.org/10.1007/978-3-319-90826-7_3

positivism which they reject, although it is not clear what he meant by “positivism.” Stavros Drakopoulos (2016) argued that it is because economists consider the scientific ideal of physics to be the appropriate methodology for economics, and so there is no need to consider the views of economic methodologists. My argument will be similar to Lawson’s but more specific to the behavior of analytical philosophers who dominate North American philosophy departments.¹ But before arguing this, we need to consider the history of economic methodology since 1962.

A BRIEF PREHISTORY OF ECONOMIC METHODOLOGY

At the session on “Problems of Methodology,” six economists and one philosopher participated.² The session has become famous mostly for Paul Samuelson’s discussion of Milton Friedman’s view of methodology. But for ten years after that one event and its publication, research in the methodology of economics was basically banned from being published in any major economics journal until briefly in 1973. That year a major methodology article was published. That article was by my former student, the late Stanley Wong, and was about Samuelson’s view of Friedman’s methodological views.³ Until 1973, no major journal published any article about economic methodology. And after Wong’s (1973) article, there was none until 1979. Books fared a bit better as there were four books on economic methodology published, but none gained notice until Mark Blaug’s (1980) methodology book. After that, books specifically on methodology began being published by major publishers.

After Wong’s 1973 article, the next major methodology article published was probably my 1979 article in the *Journal of Economic Literature* that presented a critique of most of the criticisms of Friedman’s methodological pronouncements and concluded by presenting my brief critique which identified Friedman’s methodology as merely a form of eighteenth-century instrumentalism. Despite the publication of Wong’s article in 1973, the editor of *The American Economic Review* continued to reject articles on methodology, including my subsequently published 1981 article about the neoclassical maximization hypothesis. It was subsequently published only after a new editor, Robert Clower, took over the editorial duties.

ON THE HIJACKING OF ECONOMIC METHODOLOGY

The history of economic methodology can be a little confusing since it is not clear what is meant today by “economic methodology.” In this regard, 30 years ago Deirdre McCloskey raised a distinction between what she called “small-m” methodology and “big-M” methodology which suggested a difference between what methodologists like me talk about and what philosophers of economics talk about. What I have been doing for over 50 years is trying to address a simple question: Why do economic model builders assume what they assume? I once called my version “metatheory,” since it really involves theorizing about the process of building theoretical explanations and models. In contrast, philosophers of economics worry about questions such as: What do economic model builders mean by “realism” or “realistic” assumptions? Are economic models testable? What is the cognitive status of economic theory? Is economics a science? And so on.

Needless to say, there is a considerable overlap between these two approaches, but they are different. While small-m methodologists will often be aware of the views of many philosophers, few will have studied philosophy. Similarly, most big-M philosophers of economics rarely know much about economics beyond the level of Economics 101. And when they do look into the writings of economists, they are doing so with an interest compatible with philosophical studies that usually involve what Don Ross (2014) calls “conceptual analysis.” In North America, this is thus limited to the interest of analytical philosophers.

Most big-M methodologists today think any discussion of methodology must be in terms of the views accepted in mainstream philosophy departments. However, among those discussing economic methodology, there is rarely any discussion about the views of philosophers of science other than Karl Popper’s. Today, even those debates have disappeared. While the philosopher Daniel Hausman in the 2001 issue of *JEM* argued that there is now a merging of these two views, to the extent that he is correct, I see this rather as a hijacking of small-m methodology by analytical philosophers.

One main question to be discussed in this chapter is why today’s mainstream North American economic model builders still do not say anything about methodology in their published research. Another question to consider is why those attending methodology conferences say little about the small-m methodological decisions that model builders nevertheless must