

Systemism, Social Laws, and the Limits of Social Theory: Themes Out of Mario Bunge's *The Sociology-Philosophy Connection*

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The four sections of this article are reactions to a few interconnected problems that Mario Bunge addresses in his *The Sociology-Philosophy Connection*, which can be seen as a continuation and summary of his two recent major volumes *Finding Philosophy in Social Science* and *Social Science under Debate: A Philosophical Perspective*. Bunge's contribution to the philosophy of the social sciences has been sufficiently acclaimed. (See in particular two special issues of this journal dedicated to his social philosophy: "Systems and Mechanisms. A Symposium on Mario Bunge's Philosophy of Social Science," *Philosophy of the Social Sciences* 34, nos. 2 and 3.) The author discusses therefore only those solutions in Bunge's book that seem most problematic, namely, Bunge's proposal to expel charlatans from universities; his treatment of social laws; his notions of mechanisms, "mechanismic explanation," and systemism; and his reading of Popper's social philosophy.

Keywords: *theory; laws; mechanism; explanation; Popper*

I. POLICING THE LIMITS OF SOCIOLOGICAL THEORY: BUNGE VERSUS THE NEW ORTHODOXY

Expel the charlatans from the university.

Mario Bunge (1999, 221)

In a postmodern world, there are no more authors, there are no more works.

George Ritzer (1997, 203)

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Roughly half of the book deals with what Bunge calls interchangeably antiscientific, postmodernist, and pseudoscientific tendencies in the humanities during the past three decades (p. 210) (page numbers in the text refer to Bunge 1999). "Charlatans" is the mildest expression he uses to label his opponents; harsh language aside, he does show that they are often engaged in intellectually unfair business. Moreover, he—along with a multitude of other authors—has been engaged with these opponents for years. His own work shows that the efforts have been in vain, for we (meaning noncharlatans) are still being called on to "expel the charlatans from the university before they deform it out of recognition and crowd out the serious searchers for truth" (p. 221). For various reasons, such an undertaking looks unrealistic and belated.

To coordinate the exodus of frauds from the social sciences—indeed, from academia—Bunge sets out a Charter of Intellectual Rights and Duties, which consists of ten clauses and concludes the book.¹ The ten items are not novel for adherents of a scientific and objectivist approach to knowledge; the issue is rather in the feasibility of their implementation. First, the audience for the book is most likely to be confined to the like-minded. Second, even if the book happens to find a pair of perceptive ears among the producers of "cultural garbage" (p. 221), the rights and duties in the charter are easily translatable into their loose rhetoric as well. The only exception is perhaps precept 10: "Every academic body has the duty to be intolerant to both counterculture and counterfeit culture" (*ibid.*). It is not that the ruling—to be intolerant—would produce much commotion at any school today; what Bunge refers to as "counterculture" has become part and parcel, if not a prevalent ideology, of present-day humanitarian culture.² In addition, since Bunge touches on delicate organizational or administrative matters, it seems also impossible to draw a "we-they" division line that does not cut through the same schools

1. See pp. 222-23. The charter reaffirms the right of every academic to search for the truth and to teach it in a rational manner and to make and correct mistakes and the duties "to expose bunk" and to express themselves in "the clearest possible way." Academics have the right to discuss any "clear enough" views and the duty "to adopt and enforce the most rigorous known standards of scholarship and learning," but nobody has the right "to present as true ideas that he cannot justify in terms of either reason or experience" or "engage knowingly in any academic industry." The last item on the due treatment of counter/counterfeit cultures is discussed in the text.

2. Bunge points to the fact that many of "the enemies of conceptual rigor and empirical evidence . . . who pass off political opinion as science; and who engage in bogus scholarship . . . have acquired enough power to censor genuine scholarship" (p. 209).

and departments. When Bunge calls on “all genuine intellectuals [to] join the Truth Squad and help dismantle the ‘postmodern’ Trojan horse in academia” (p. 223), one might wonder if there is need to mobilize “genuine intellectuals”: they have been and are doing this thankless job by the very meaning of the expression. Nevertheless, the practical effect has so far been completely out of proportion to their efforts. True, there was Alan Sokal’s smart sortie, yet one of its lessons has been, in retrospect, that humanitarian “stables” can comfortably accommodate virtually any horses.³

To appraise current tendencies and Bunge’s proposal, we might find it instructive to look in detail at the following fresh example. In their “Introduction” to *Handbook of Social Theory*, editors George Ritzer and Barry Smart (2001) waver incessantly between two goals: to define the field and, at the same time, to avoid defining the field as a dangerous *political* act. How is that? The *Handbook*, we are told,

even if the editors did not intend it, will play a role in helping to define social theory at the dawn of a new millennium. However, such an exercise is not without controversy, for developments within social thought, in particular the construction of postmodernism, feminist and multicultural perspectives, have rendered the very activity of defining the key figures and perspectives to be found in the field as problematic, as representing something like the constitution of a canon, itself a potentially reprehensible act. We are all now acutely aware of the fact that defining a field is regarded by some commentators as a potentially dangerous political act. (P. 1)⁴

There is no word of assessment from the two prominent theorists of the claims of the above-mentioned developments and anxieties of

3. Thomas Nagel’s reaction to the impact of Sokal’s hoax may be said to be ambivalent. “Sokal revealed the hoax,” Nagel writes, “and nothing has been quite the same since. We can hope that incompetents who pontificate about science as a social phenomenon without understanding the first thing about its content are on the way out, and that they may some day be as rare as deaf music critics” (Nagel 1998, 32). On the other hand, in his discussion of Jean Bricmont and Alan Sokal’s *Fashionable Nonsense*, he sounds sociologically more measured: “It is important to follow up on the positive effects of the original hoax, but will teachers of cultural studies and feminist theory go through these patient explanations of total confusion about topology, set theory, complex numbers, relativity, chaos theory, and Gödel’s theorem? The scientifically literate will find them amusing up to a point, but for those whose minds have been formed by this material, it may be too late” (p. 33). See also Wight (1998, 553), who writes, two years after the hoax, about “the depressing lack of ‘real’ debate that has followed Sokal’s intervention”; and David Miller’s (2000) skeptical reaction.

4. Page numbers in the text refer to Ritzer and Smart (2001).

“some commentators.” Still more surprising is that Ritzer and Smart nevertheless do consciously and persistently commit both these sins, “a potentially reprehensible act” of constituting a canon, and “a potentially dangerous political act” of defining a field. However good their intentions may be, they are mutually cancelling. This makes the editors fill up the introduction with obeisances to recent commentators, express their loyalty to the standards of science, and resort to verbal acrobatics to reserve a place for classics. Fortunately for the field, Ritzer and Smart recognize the existence of considerable agreement within the profession as to who is to be included as classic.

When it comes to contemporary social theory, however, two peculiar criteria are put in place. It is clear for the authors that “the idea of a canon” entails certain problems, especially “in the effort to be as inclusive as possible” (p. 2), which is the first criterion. This bibliographic method of “selecting” theories entails also “ensuring the inclusion of those perspectives that have been most critical of the idea of canonical works.” A trouble for the canon as such looms: instead of being a model for the rest, it has to give up its limiting purpose and embrace the rest. The difficulty with such a suicidal canon is met like this:

Inclusion of contributions on postmodernism, feminism and multiculturalism is not simply a matter of editorial choice; any contemporary attempt to map out the field of social theory, to specify the range of perspectives utilized by social theorists, would need to acknowledge the capacity of the canon to accommodate critical approaches. (P. 2)

The editors confess that they are hostages of the present theoretical *Zeitgeist*, as they understand and shape it, so an oxymoronic all-inclusive canon will pass for today’s field of social theory. The next dilemma arises right away: unlike the pliant concept of canon, the physical canon is limited. Editorial selection is willy-nilly back again, together with a second criterion: complaints of the contributors about the underrepresentation of their theories. Let us see how the second criterion works. (The key terms have been italicized.) A few chapters “deal with approaches that frequently have been *marginalized* or *excluded*. The *Handbook* also includes chapters on theoretical contributions to substantive topics that have been similarly *neglected*” (p. 2). Several other authors “are acutely aware of the historic tendency to *marginalize* or deny the relevance of the topics of concern *to them*.” One of the writers attracts public attention “to the way in which questions

of ethics and morality have tended to be regarded as virtually *inadmissible* within sociological discourse"; another "argues that the embodied basis of social life has been *devaluated* and *marginalized* within the sociological tradition and that [or therefore?] a theoretical understanding of embodiment is central to a more effective understanding of the constitution of society." As a matter of course, there is a timely reminder of "the relative *neglect* of sex and sexuality in modern social thought." "This theme of *neglect* is central" also for the chapter in which we are told that "the productivist bias of most classical and contemporary social theory has led to theories of consumption being inappropriately *relegated* to a position of relative *unimportance*" (p. 2). Note: It is the proponents of the approaches, perspectives, and conceptions themselves who complain about their brainchildren being marginalized, devaluated, and neglected, and it is the theorists' complaints—not the cognitive merits of their ideas—that the editors plainly state to be the basis of their selection.

This policy manages somehow to embrace a devotion to the critical attitude. "Criticism is not an optional extra; it is an intrinsic part of the practice of social science" (p. 3), write the authors as if they were making up Bunge's charter where he, oddly enough, does not use "criticism." One can see the standards of criticism of contemporary social theory and science in the flesh, when the authors enumerate "the key influential figures in contemporary social thought" (p. 3): Foucault, Lyotard, Derrida (who does not believe in dialogue), and Baudrillard (though Ritzer has to confess elsewhere that he does not accept his idea of "death of the social").⁵ Ritzer and Smart anticipate that their choice will inevitably produce a grumble from the excluded. As a consolation prize, and a means to "re-define the field and re-codify something like a canon," they provide the readers—that is, more potential complainers—with the editors' e-mail addresses, modestly assuming their personal responsibility for the direction of the field. One can only wonder what the size and contents of new editions of the *Handbook* will be, if the principles "be as inclusive as possible" and "prefer the complainant" are taken seriously; and what would have happened to the hard sciences if they had been guided by the same critical and theoretical principles? The editors simply fail to see that their "to

5. Ritzer (1997, 201). Parenthetically, Foucault is infamous among professional historians: see Carlo Ginzburg ([1976] 1992, xviii) who says, "Irrationalism of an aesthetic nature is what emerges from this course of research"; cf. other professional views of Foucault's historical studies as "empirical catastrophes" (Gay 2000, 33), and on "the extravagant hyperbole of Michel Foucault and Jacques Derrida" (Haskell 1998, 9).

be as inclusive as possible" approach is well suited for an archivist but is rather counterproductive for a canon maker; as Willard Quine wrote about a different canon, "If the book is not normative it no more permits than forbids" (Quine 1981, 208).

There are more interesting features of the quite welcoming canon and the definition of the field. The approaches Bunge classifies as antiscience—existentialism, phenomenology, phenomenological sociology, ethnomethodology, and radical feminist theory (p. 210)—are all represented here in full. While Nietzsche and Foucault, along with Parsons and Elias, multiculturalism, feminist and critical theory, are allotted whole chapters in the part "Contemporary Social Theory," Robert Merton is dismissed from the renewed field; perhaps his work had been found less "canonical." No wonder that the "Introduction" is full of up-to-date theoretical slang. As if trying to conceal their intentions still more, Ritzer and Smart name one of the sections "(De)/(Re)Constructing the Canon"; sometimes they use quotation marks for inexplicable purposes ("great" texts, "classics," and the "terror" of the Soviet *Gulag*—was it not terrible enough?); "narratives" pass as theories. At the same time, political topics tend to dominate the contents of the book on social theory. Here is a characteristic fragment of this political theorizing: "Today we forget that Durkheim and Tönnies were both socialists, and this is one reason why we fail sufficiently to think of socialism as a social theory" (p. 486).⁶ One might then extend this type of inference: most members of the Vienna Circle were socialists, and this is one reason why logical positivism is a socialist theory.

A rejoinder is possible on these lines: the *Handbook* was intended to define the field of "social," not "sociological" theory—hence Bunge's charges that it is antiscientific, nonempirical, and so on would miss the target. Recall however that the authors speak of the intrinsic criticism of "social science" (p. 3), and the terms *sociological* and *social theory* interchange throughout the "Introduction" and the rest of the book. At the only place they touch on the issue, we read that the distinction between sociological and social theory "is far from clear" (p. 7). The social, or sociological, theorists shed nevertheless a bit of light:

Theorizing about social life is not confined to the discipline of sociology, indeed it might be argued that increasingly it has been analysts who, much like Marx, are not operating within a sociological paradigm

6. Beilharz (2001, 486).

who have had the most powerful impact on the development of contemporary social thought and the generation of more persuasive understandings of social conditions. (P. 7)⁷

Now, what about “theory”? It probably allows for calling the following accounts theoretical. Keeping in mind that knowledge is power, Ritzer and Smart’s book is just a reproduction of, say, the republican electorate and political status quo: “The power shifts from author to reader,” reminds Ritzer elsewhere, even though he observes in the same passage that “in sociological theory in the postmodern era . . . there are no more authors, there are no more works”⁸ (strictly speaking, it depends on the authors). Or else, the contributors of the *Handbook* study a bundle of textually and socially (de)(re)constructed simulacra, not “““r-e-a-l”” social issues. Or, the very intent of the *Handbook* is an expression of the editors’ masculinity, class arrogance, and phallogocentrism. All of these are ideas that the editors do not disdain. Such sorts of interpretations are considered legitimate, sensible, and theoretical in the redefined field. The trade of uncovering another “social condition” on the basis of one’s personal sensibility replaces arguments, empirical evidence, clarity, and consistency—ultimately, any understanding. Just as has happened with “canon,” the swollen out “theory” invites now any account, however non-arguable it is.

To make more sense of the relations between social and sociological theory, the reader is invited to compare the *Handbook* with three other comprehensive volumes by Ritzer. They are called *Contemporary Sociological Theory*, *Sociological Theory*, and *Modern Sociological Theory* (Ritzer [1983] 1992a, [1983] 1992b, [1983] 1996, respectively), and their contents are virtually identical (except *Sociological Theory* has a chapter on the classics). Most illuminating in the comparison might be the fact that even a cursory content analysis reveals very few differences between these three volumes on sociological theory and the *Handbook of Social Theory*: the same classics, and almost the same menu of topics and key figures in modern-contemporary theory. A brief comparison of their past works with the *Handbook* suggests that the canon and the field had been defined by Ritzer and Smart long ago

7. These social analysts and their insights include Foucault, Derrida, Lyotard, Deleuze, Guattari, Virillo, Baudrillard, and “the narratives of literary and cultural analysts such as Jameson and Bhaba, the psychoanalytic reflections of Lacan and Kristeva.” This is the whole explanation. The editors proclaim their responsibility for the field yet simply turn away from this conceptual difficulty.

8. Ritzer (1997, 203).

and reflects in the first place their personal preferences,⁹ that social theory is a blend of sociological and postmodernist (nonsociological) theories,¹⁰ and that the “intrinsic criticism” of social science does not apply therefore to social theory in general.

The above-discussed volume illustrates the already deeply institutionalized tendency that Bunge calls the Trojan horse. However passionate one’s will to expel the beast might be, the intent is after all quixotic. To revise Bunge’s metaphors, social-sociological theory resembles now not a stronghold led by a “Truth Squad” but a farm where nobody may be marginalized or excluded. “Postmodern social theory now seems to have become a part of the very canon it itself sought to discredit,” Ritzer and Smart report authoritatively (p. 7). A rather marginal question arises: Who has been canonizing it? Well, many sociologists have in reserve a needed professional explanation: systems, structures, and Zeitgeists.

The volume just discussed may usefully address another claim of Bunge, who speaks of the “distrust of theory” among “data hunters and gatherers . . . as if theoretical research did not exist,” and calls on them to fix on crafting theories of the middle range (Bunge 1999, 10). Almost any volume on sociological-social theory will show, however, the affluence of traditions, paradigms, approaches, theories, and conceptions of any thinkable range and degree of irrefutability. The enormous theoretical literature overwhelmingly consists in cataloging countless past and present theories, but rarely contains what theory presupposes, namely, critical assessment. Nothing is being given up, no matter what has been offered—probably because “in a postmodern world, there are no more authors, there are no more works,” as Ritzer confidently informs us, and very consistently goes on:

Every sociological theorist, indeed everyone, is an empowered reader of all theoretical works, even those emanating from the “geniuses” in the field. As a result, there will be (and, in fact, there are) almost as many

9. See also Smart (1976, 1983, 1985, 1992) and Smart and Smart (1978).

10. Ritzer assumed that “the simple fact that postmodernism can no longer be ignored by sociological theorists” and, on the basis of Lyotard’s incredulity toward grand narratives, that “sociology has moved beyond the modern period, into the postmodern period” (Ritzer [1983] 1996, 470-73). Even though he does not follow Baudrillard’s idea of “the death of the social,” he nevertheless thinks that Baudrillard “is offering a sociological theory” (ibid., 483). Moreover, Smart and Ritzer find in Foucault “several sociologies” (Ritzer [1983] 1992b, 507). And, since sociological theory is defined as “the ‘big ideas’ in sociology” (Ritzer [1983] 1996, 483) whose bigness is defined by one’s liberal criteria, of course there are no restraints in the field.

interpretations as readers, and each of these interpretations is inherently no better or worse than the others or than the interpretations of the "authors" of their own work. The result: a massive proliferation of theoretical ideas, the *raison d'être* of sociological theory in the post-modern age, where the goal is to "keep the conversation going." (Ritzer 1997, 203)

Unfortunately, the conversation of the critically deaf is not only the author's intention (if Ritzer admits at least his own authorship) or his own vision of what "the *raison d'être* of sociological theory" is; one has to submit that it is also a believable narrative about contemporary social theorizing. The distaste, to which Bunge refers, among empirical sociologists for the indecently profuse theory is more than understandable and, given the present situation, perhaps even productive for their work.

II. SOCIAL LAWS AND LARGE-SCALE SOCIAL PREDICTIONS: BUNGE AND RANDALL COLLINS

No known laws explain . . . the collapse of the Soviet Union.

Mario Bunge (1998, 22)

There are . . . some plausible candidates for social patterns, in particular historical laws, that is, laws of social change.

Mario Bunge (1999, 112)

Sociologists suppose that if they had recognized the category when the process began they would have been able to predict its outcome.

Charles Tilly (1995, 1594)

In this section, I point to some difficulties in Mario Bunge's notion of social laws. It seems useful also to discuss it in connection with Randall Collins's understanding of the matter for a few reasons. Bunge shares with Collins the views that social science is nomothetic, that it has produced a certain number of social laws, and that this feature makes sociology (more) scientific, as well as antipositivist. As I show, Bunge is sympathetic with Collins's bold applications of the nomothetic approach to history, even though this counters his other claims. Some of their reasons look similar yet, as I try to show, not verisimilar.

As Collins (1989, 124) writes, "Criticisms of the scientific status of sociology possess some validity when applied against narrowly positivist interpretations of sociological method and metatheory, but do not undermine the scientific project of formulating generalized explanatory models." Contra skeptics, he exhibits as the obvious achievements of sociology a few sociological laws, or principles (to which I return). Bunge (1999, 9) echoes a decade later, "The opponents of the scientific approach to the study of social matter deny the existence of social laws: they hold that the social studies are necessarily idiographic or particularizing, not nomothetic or generalizing. Yet we do know a few social laws." He too provides a "random sample" of ten social laws (and we know also that their number may vary from just a few to not less than hundreds of "both plausible and dubious" laws).¹¹ Some of Bunge's top-ten social laws, however, seem to be as plausible (or analytic) as biological truths can be: "7. Poverty stunts physiological development"; "8. Malnutrition and lack of skills hinder increase in productivity." Law 10 sounds rather like a direction: "Sustained development is at once economic, political, and cultural." He (Bunge 1998, 28) puts it more clearly, "Only integral (economic, political, and cultural) social reforms are effective and lasting," but in this case the qualifier "only" invites innumerable counterexamples of successful piecemeal reforms.

Another law looks especially problematic: "5. Modernization tends to replace the extended family with the nuclear family." Even though this historical tendency is empirically well supported, the statement can be neither a law nor even a regularity: it refers to a single even if complex phenomenon, that is, Modernization.¹² In spite of the existence of many theories of Modernization, a law would have to describe more than just one modernization to make any prediction or at least explanation possible. "Modernization" might refer to the economic conditions, and a set of concomitant new institutions and

11. Bunge (1998, 28-29) gives us up to twenty social laws with a warning that "Social science is a land with many prophets but few laws." Yet on the following page, one learns that "There are literally hundreds of further generalizations of the same kind . . . both plausible and dubious. . . . True, few social regularities are universal or cross-cultural; most of them are local, that is, space- and time-bounded. But so are the laws of chemistry and biology."

12. Popper points to this, still popular, mistake when he shows the impossibility of any laws of evolution in biology or sociology alike; see Popper ([1944-45] 1997, section 27). "Globalization" is among more recent concepts providing today's prophets with bread buttered; for example, the Iraq war is said to be "typical for the transition period of globalization" (Garejew 2004), as if we knew many globalizations and their ways.

values usually associated with the term, but, again, these too are a unique historical constellation of events or processes.

In addition to these sociological, socioeconomic, and biosociological laws, there are a number of economic laws, such as that of diminishing returns, and political ones, such as Tocqueville's—people revolt not when oppression is maximal but when it begins to slacken. So, social science is nomothetic as well as idiographic. (P. 9)

As to the law of diminishing returns, economists place important reservations on the law.¹³ Tocqueville's observation, in turn, no matter how plausible it may seem, counts in the text both as a "social law" and "social mechanism" (p. 59; this aspect is discussed later on). There are a few other socioeconomic laws that look as trivial as credible (laws 2, 3, 4, and 9), but they hardly match Bunge's understanding of the "genuine law statements, [which] unlike empirical generalizations, are theoretical: they are either axioms or theorems in hypothetico-deductive systems such as general equilibrium theory" (p. 10), and they do not conform to theories of the middle range, which he calls for "positivists" to work out (*ibid.*). Perhaps, the expression just quoted from Bunge, *empirical generalizations*, which he opposes to "genuine law statements," would describe more aptly what he and Collins offer as "social laws."

With Collins, however, the problem makes another turn: that easily obtained social laws or "principles," aided by terminological complications, are employed in support of ambitious but misleading conclusions about the abilities of the social sciences. The fact that Collins is never clear about his terms is only a minor difficulty.¹⁴ More perplexing is the way he establishes the "scientific validity" (Collins 1989, 124) of theoretical knowledge. Mentioning a number of sociologists from Durkheim to contemporaries, he finds, "The coherence among these various kinds of theory and research constitutes strong evidence that the interaction-density/solidarity/conformity principles are true" (p. 125). This peculiar move then makes any bunch of views (rumors, false theories, lies, or superstitions) true merely on the basis of the coherence among them. He is convinced that the three princi-

13. See, for example, Samuelson and Scott, who admit that it is an "important, often-observed, economic and technical regularity; but it is not universally valid"; for the restrictions to the law, see Samuelson and Scott (1975, 22).

14. Social laws figure also as "explanatory models," "lawful findings" or "lawful generalizations," "principles," "valid generalizations" (Collins 1989, 124-25); or "general hypotheses" (Collins 1978, 29), and so forth.

ples, which are supposed to encourage sociologists' self-esteem, "are not trivial but lead to sociological insights into a wide range of important questions" (Collins 1989, 127).

Perhaps the most impressive among lawful insights is Collins's self-professed prediction of the collapse of the Soviet Union. This case deserves special consideration not only because of its boldness; it is relevant here also because Bunge both endorses Collins's prediction as successful and thinks it is impossible. He writes that Collins did predict the collapse "from macrosociological conditions" (Bunge 1996, 382); on the other hand, "no known laws explain . . . the collapse of the Soviet Union in 1991" (Bunge 1998, 22). Bunge's views of social laws are thus somewhat ambiguous, whereas a closer study of Collins's treatment of social laws and theory will suggest that he successfully "predicted" virtually any outcome because of the way he had formulated his version of geopolitical theory.

In 1978, Collins outlined an "explanatory theory" whose aim was to "explain the pattern of movement of state boundaries." As a starting point, he adopted Weber's notion of the state as consisting "ultimately of military control over a territory" (Collins 1978, 1-2). Later on, he revealed this technique and, by implication, its potential flaws: "Turning this definition into an explanatory theory meant treating everything in it as a variable; the result was a theory of the conditions that determine geopolitical rises and falls in territorial power."¹⁵ Thus, Weber's definition is supposed to serve as an explanatory and predictive theory. I hope it will be seen from the following that his solution is another failure of this kind, and the two variables he promises to stick with—namely, military control and territory—are never enough to account for any social processes, not even using his peculiar method.

He does not accept monocausal interpretations of social change and groups multiple causal variables, which determine "the extent of the states," into two main categories or groups of factors: "the organizational bases of the military" and "the territorial configurations."¹⁶ Since the theory is to be "dynamic," he envisages its efficacy in the delineation of *time periods* of state borders' development: the knowl-

15. Collins (1995, 1552). Here again, he finds the sign of the truthfulness of the theory in its affinity with other theories, his time with Theda Skocpol's theory of revolutions: "The convergence between the two theories seemed to me additional evidence that the model was on the right track" (ibid.; cf. Collins 1995, 1559).

16. First, it is the "organizational bases of the military" that comprise "(a) weapons and military structure; (b) economy; and (c) administrative resources, which include both the technology of administration, and cultural resources in the form of religion

edge of the variables determining both border change and its periods promises the theory's "testability," "empirical validity," and the possibility of making predictions (Collins 1978, 3).

Yet, as Collins himself has to admit, the task of finding "the time laws" has failed.¹⁷ He is facing the challenge of all contemporary theorists who call their field theoretical history, or macrohistory, historical sociology, macrohistorical sociology in Collins's version, and so on, and who try to bind up in patterns large-scale social structures with long historical periods. Now, Collins confesses that he has not managed time: "What are the time laws of these processes [of state border changes]? Unfortunately, there is no simple numerical pattern" (p. 19). A pessimist might think that the lack of a conceivable periodization is a failure of the whole undertaking, but Collins does not dwell long on this hurdle; he moves further and establishes the principles "determining the territorial power of the states." He handles both space and time by stressing—as he is doing throughout his works—the importance of cartography: "These principles of external relations are largely inductive, derived above all from the analysis of historical atlases in conjunction with topographic maps, supplemented by narrative stories" (p. 4). The output of the inductive study are "seven main geopolitical principles" (p. 8). For considerations of space, I focus on what seems the most important in the principles, in their use and abuse.

The outstanding feature of all the principles is the number of limitations and qualifications they have. Every time Collins finds in maps and records inconvenient examples, he resorts to additional explanatory subprinciples and taxonomic subcategories. Take the first, seemingly trivial principle: "*States based upon the largest and wealthiest heartlands tend to dominate the smaller and poorer ones, all else being equal*" (p. 8). Very soon it turns out that

clearly this principle has limitations. Empires not only expand, but contract . . . sometimes the larger and richer states are beaten or even conquered by the smaller and poorer ones. In other words, multiple causality holds; the resources of a given territory are only one variable among several. (Collins 1978, 11)

and ethnicity" (Collins 1978, 1-2). Second, "Among the territorial configurations we must consider (a) heartlands; (b) barriers; and (c) the external relationships among heartlands and the states that are built upon them" (p. 2).

17. The attempts to find any stable temporal sequence in the development of state borders produce the dispersion of figures from fifty to fifteen hundred years (Collins 1978, 19-20; see also pp. 28-29).

The second principle says, "*Marchland states have a power advantage over more centrally located states*" (p. 11), but it is not clear how it matches the first principle and the finding that heartlands are more easily "accessible to military control" (p. 7). The state borders in heartlands according to Collins depend not only on natural barriers but also on population, technology, and the state's size and economy (p. 8); yet so do the state boundaries of marchlands (p. 15). Naturally, "the marchland principle, although simple to apply in many instances, nevertheless contains a number of complexities" (p. 14); to complicate it further, the notion of "internal marchlands" is coined.

The third principle, "*balance of power*," has four subdivisions as well, but more noteworthy is the way the cultural "variable" is rendered. The fourth amendment, or subprinciple, of this principle states, "*Military ferociousness increases near crucial turning points*" (Collins 1978, 21). And, in spite of the failure to find a "simple temporal pattern" (pp. 19-20), Collins identifies "turning points" characterized by a greater brutality. After giving historical illustrations, he comes to explain the affairs in modern Europe as follows:

The ferociousness of the Nazi regime, with its concentration camps and extermination programs (matched to some extent by similar policies on the Russian side of the long-disputed territory of Eastern Europe), may be seen as an example of a similar dynamic, as the long-standing fragmentation of eastern Europe seemed to be entering a showdown between two major powers.

A greater brutality then is to be understood as the function of the calendar and map, not the effect of the irrational and equally idiotic beliefs of the Nazi and Communists. Ethically, the disregard of beliefs, ideologies, and in particular of the responsibility of individuals in this analysis has suspect overtones; such an "explanation" invites us to employ the tactics used by the Nazis and Stalinists themselves, namely, to explain their atrocities by the familiar impersonal historical forces, this time of the geographic and temporal kind. Methodologically, this disregard is in conflict with his repeatedly expressed intention to pursue multiple causality.

All this is staggering considering Collins's talking elsewhere about the necessity for sociology of "ultradetailed empirical research" and "translating all macrophenomena into combinations of micro-events" (Collins 1981, 985). This requirement is at odds with geopolitics: translation of its principles into the language of individual

actors would produce a weird generalization such as "People strive, always and everywhere, for military and ultimately for spatial domination." Methodologically, the above causal imputation of the ferociousness of the Nazis and Bolsheviks is indeed derived from maps: the upshot is that mere geographic location accounts for psychological traits and mass phenomena (a doctrine as old as it is dubious), still the causal connection is not clear. At the same time, the apparently innocuous method of "multiple causality" appears to be used by Collins arbitrarily. Compare, in provision "c" of the fourth principle he finds a place for culture: "*Universal religions and ideologies follow the non-intervening-heartland rule*. A final corollary concerns the cultural sphere" (Collins 1978, 25). It is only here that "ideological content" is used, but only to follow "geopolitical lines, not vice versa" (p. 26). In the sixth principle—"Imperialism follows unification"—one can learn also a bit of geopolitical psychology (p. 27): political leaders "usually operate more upon impulse than calculations," "psychological mood, the energy dynamics . . . are strongly desired by leaders of newly unified states," and "in many cases, they attempt to generate this energy" (recall his concern with the scientific validity of sociology).

Armed with "the seven general hypotheses," supplemented in turn by numerous provisos, stipulations, and conceptual ambiguities, Collins applies them to the world situation of the late 1970s. The readers, depending on their mastery of geopolitical interpretation, can find in the "Conclusion" (pp. 29-31)—a model of prognostic equivocality—almost any possible scenario, where the collapse of the Soviet Union may be seen not clearer than the collapse of the United States and where only two temporal specifications are one hundred and five hundred years (p. 30). The claim that the Soviet collapse was predicted already in this 1978 work is a sheer bluff, for a geopolitical "theory" collapses into manifold ad hoc accounts and reservations, and every past and future event or trend is easily interpreted geopolitically. The predictor thus finds himself in the situation of a sociological Nostradamus, for his prognoses, not to mention retrodictions, are doomed to be true if interpreted rightly. Incidentally, Karl Popper's chief reproach of Marxism and Freudianism was that their irrefutable "empirical validity" was based on mere collecting innumerable and supposedly confirmatory cases. Collins's account (or "general historical interpretations" in Popper's terms) is equally irrefutable for it absorbs any happenings as geopolitically interpreted "confirmations." Geopolitical theory is thus all explanatory and for this reason nonarguable, as every new auxiliary

clause and anticipation of diverse developments only makes it more irrefutable.¹⁸

The geopolitical principles undergo on some modifications later on, but the method Collins employed before is unchanged: to make incessant reservations to the principles and “predict” as many outcomes as possible without temporal specifications. In 1986, already after the advent of perestroika, he wrote “The future decline of the Russian Empire” in *Weberian Sociological Theory* (though the link between Weber and geopolitics is far from obvious). Using the method of predicting multiple, vaguely formulated possibilities, he foresees that “the long-term fragmentation of the Russian Empire would last through the twenty-first and twenty-second centuries” (Collins 1986, 196). In other words, even if the USSR reunited or reorganized, and broke down a few times by circa 2190, the “prediction” would still be correct, and perhaps useful. Since the few-decades-long cold war is understood as a “turning point,” “there are two possibilities of a geopolitical turning point. One possibility is victory of one side over the other, and establishment of a world empire; the second is stalemate” (p. 197).¹⁹

Another predicted scenario claims that “precisely because of its natural resources and its internal instability, the Soviet Union will be strongly tempted to intervene militarily in Iran, Iraq, or the Arabian Peninsula in coming decades. Such an advance would constitute a serious overextension” (Collins 1986, 202). Thus, foretelling diverse outcomes, Collins makes his account almost invincible. “If this hypothesis is correct,” he writes humbly, “the power of the Soviet Union . . . can be expected to go into long-term decline” (p. 202). But how can such a “hypothesis” be incorrect? It for example encompasses three outcomes of the nuclear opposition of the United States

18. Cf. Popper ([1934] 1999, 82-83): “As regards auxiliary hypotheses we propose to lay down the rule that only those are acceptable whose introduction does not diminish the degree of falsifiability or testability of the system in question, but, on the contrary, increases it.” When Michael Hechter (1995, 1526) says that “geopolitical theory makes a set of conditional predictions” and “this sounds very much like Popper’s description of conditional scientific prediction,” he seems to miss Popper’s demand for any scientific statements to be arguable, that is, refutable, which geopolitics is not. That is unexpected, for Hechter writes about “the standard answer” by Popper as to social prediction and prophecy (1522-23), and that Popper, who “hardly was an obscure figure in the social sciences,” anticipated Tilly’s criticism of theories of revolutions by four decades (p. 1525).

19. As a popular jokes goes, the probability of any event is 50 percent—it may or may not happen; sometimes Collins seems to foretell in this way.

and Soviets: a peaceful resolution perfectly fits geopolitical ruminations. Mutual destruction does so just as well: "Such an outcome . . . would nevertheless be entirely in keeping with prior geopolitical patterns." The destruction of the United States and its further occupation by the Soviets would still be followed by "a decline in Soviet power in the long run of the next centuries" (Collins 1986, 206). ("I feel there was nothing immoral about attempting to make a contribution in 1980 [actually in 1986] to surviving the nuclear arms race," he adds later [1995, 1587].) As one can see, only one unlikely end result of the nuclear opposition—the devastation of the USSR with America's survival—is left as a possible historical objection. Examples can be easily duplicated. The question is, What exactly did Collins predict: particular events, trends, or anything?

In 1995, Collins nevertheless boasted retrospectively about the "geopolitical theory's successful prediction of the breakup of the Soviet Union" (Collins 1995, 1552), and went on rhetorically, "How can we differentiate valid prediction from lucky guesses and from post facto pleading?" (p. 1554). He does not answer but instead keeps adding nuances to increase the applicability—or rather hermeneutic potential—of the principles. The first principle of military expansion presupposes now both "peaceful and quasi-peaceful means" (p. 1555). The fourth principle states that cumulative processes bring, over century-long periods, states of "drastic simplification. This simplification," as one can expect now, "may happen in a number of different ways" (p. 1557). The fifth principle—"Overextension brings resource strain and state disintegration" (p. 1558)—posits the existence of "overextension points," the determination of which remains as yet unclear. This time, the previous seven principles are reduced to five—evidence of which was mainly "based on historical comparison among agrarian states" (p. 1559)—and eventually to a single "complex expression."²⁰ And all this conceptual mixture is, he says, "the theoretical base from which I made a prediction about the future of Russian state power" (ibid.).

20. The expression reads, "Formally stated, the five principles may be combined into a single, complex expression. Marchland advantage is weighted by the relative resource levels of adjacent states; overextension is the fundamental principle for stating the relative vulnerability of particular geographical points to states with given resources and logistical loads" (Collins 1995, 1560). The explanatory and predictive capacity of Collins's combined principle is equal to that of the statement that eventually state borders will change.

He continues adding more variables and incorporating kindred lawful theories (by Skocpol and Goldstone) and finally reveals the nature of his method: "The advance of theory is just such a development of a core model, with ancillary models that make it applicable to a variety of historical conditions" (Collins 1995, 1565). As was said, however, implanting a multitude of ancillary models into geopolitics only makes it more and more irrefutable. Ironically, Collins considers irrefutability a virtue—but only for his own theory, for he blames fellow theorists for using the same technique: "the rival explanations of Soviet breakdown . . . have been ad hoc" (p. 1581). Charles Tilly sums up this theoretical ploy: "Fixation on invariant models gives rise to a common but logically peculiar sociological performance we may call the 'improving model.'"²¹

To show the putative success of his prediction, Collins interprets the development of perestroika, that is, the events that happened after the forecast had been made, in geopolitical terms. The smoothness of geopolitical interpretation made after the event suggests its validity to him. To the question What counts as a valid prediction? he nevertheless gives a much different answer:

There is a difference between a sociological prediction and a guess or wishful thinking. A valid prediction requires two things. First, there must be a theory that gives the conditions under which various things happen or do not happen. . . . This standard of theory is more stringent than what sociologists generally mean by the term. It is not a category scheme, nor a metatheory, nor even a process model that lacks observable if-then consequences. Second, there must also be empirical information about the starting points, the conditions at the beginning of the if-then statement. (Collins 1995, 1574)

Given these requirements, Collins's forecast then is not a sociological prediction but "a guess or wishful thinking": the first condition is not met for he has no theory as yet, his principles do not show what cannot happen. To demonstrate the putative validity of geopolitics, he resorts once more to the argument from the agreement among geopolitical theories and the fact that theory development is going on (Collins 1995, 1575) as if these factors were self-explanatory signs of

21. It consists, among other things, in "modifying the model so that it now accommodates the previously exceptional instances as well as those instances that already belonged to its domain. Most often, the crucial modification respecifies a condition postulated as necessary in the model's previous version. Thus improving the model expands the claimed scope of the alleged invariance" (Tilly 1995, 1597).

theoretical validity. New map investigations supply him now with more precise instruments: "From historical atlases, I estimated that geopolitical resources give predictability down to units of about 30-35 years" (p. 1582). The new findings allow him to generate two more geopolitical prophecies. First, the future expansion of South African military power; as a matter of course, there are provisions that will secure a greater hermeneutic plausibility of the forecast in thirty to thirty-five years.²² Second, the former Yugoslav federation "will be militarily volatile, with low regime legitimacy" (p. 1590), though this was the case years before and during the forecast.

As Alejandro Portes (1995) notes, "Collins's vision opens rosy vistas for the future of macrosociology, but alas there are good reasons to believe that this is only a dream. First, even if valid, geopolitical principles are sufficiently vague to lend themselves to contrary interpretations."²³ Geopolitical theory interprets everything too easily, but its explanatory capacity does not go beyond a trivial observation that Collins made in its first version: "A glance at the historical records shows that state boundaries are seldom stable over long periods of time, but expand and contract, combine and fragment" (Collins 1978, 2). One of the most outstanding features of geopolitics is its methodological holism and disregard, or relegation, of individuals. Many historical theorists usefully invoke Fernand Braudel, even though one can draw varying conclusions from him; some of the conclusions may not support macrosociology and varieties of geopolitics. When Braudel discovered *longue duree* structures of world economies, he thought that the very slow changes of these enormous entities in time and space "reveal the presence of an underlying history of the world" (Braudel 1977, 84). Events—that is, actions of individuals—are, in Braudel's words, merely "dust" on the surface of immense world economies, whose slow self-contained motion is not discernable in the fleeting span of human life; tectonic changes take secular units to measure. Yet Braudel provides in *The Perspective of the World* a sort of dialectical ambiguity when he gives instances of monarchic caprices, presumably historical specks of dust, which predetermine

22. "Note that expansion is not necessarily based on overt conquest; it could take a form similar to U.S. foreign power in the post-1945 period: leading coalitions of allies and exercising peacekeeping missions" (Collins 1995, 1589).

23. Portes (1995, 1623) points to other flaws in Collins's geopolitical principles: according to them, China must have experienced state breakdown too (*ibid.*); they cannot predict political decisions, and "an expectation with a 50-year range is not really a prediction about an event, but about a trend" (Portes 1995, 1624).

advantages or disadvantages for long centuries in entire parts of the world.²⁴

Collins faces a similar dilemma when he tries to downplay the role of Gorbachev and the significance of his personality in the explanation of the collapse situation. To consider it as a causal factor would be too “antitheoretical,” he thinks, because historical personalities “have the possibility of world-historical significance only if they are structurally located in a position where their actions have major consequences” (Collins 1995, 1579): note however that from this very formulation one may infer the opposite as well, that it is equally legitimate theoretically to consider personal factors as important as institutional in historical causal imputation. (And again, he does not live up to his multiple-causality promises.) It is plainly true, of course, that were Gorbachev an average Soviet communist, the effect of his personality on the events of the 1980s would not have been perceptible. But such observations are irrelevant here; the peculiarity of the circumstances was just in the significance of the individual traits of the person who would fill the position of a Soviet leader, the necessary personal traits that could make use of the “structurally located position” and make these specific changes for democratization possible in the time.²⁵ The point is that if we take his personal traits into our account of perestroika and the collapse, then the direction of the late

24. When he describes the role and change of dominant cities in the world economies in *The Perspective of the World*, Braudel seems to “blurt it out”:

When in 1421 the Ming rulers of China changed their capital city—leaving Nanking, and moving to Peking . . . the massive world-economy of China swung round for good, turning its back on a form of economic activity based on ease of access to sea-borne trade . . . this choice was decisive. In the race for world dominion, this was the moment when China lost her position in a contest she had entered without fully realizing it.

Philip II conquered Portugal in 1580, and elected residence, with his government, in Lisbon for a period of almost three years. Lisbon thus gained immeasurably. Looking out over the ocean, this was an ideal place from which to rule the world. . . . So to leave Lisbon in 1582 meant leaving a position from which the empire’s entire economy could be controlled, and imprisoning the might of Spain in Madrid, the landlocked heart of Castile—a fateful mistake! (Braudel [1979] 1992, 32)

25. Collins (1995, 1579) evokes another stratagem: “Gorbachev’s visit to China in May 1989, in an effort to reduce geopolitical confrontation, was the catalyst for the mass demonstrations at Tiananmen Square. The failure of that uprising shows that individual charisma alone is insufficient to produce structural change in the absence of the factors listed in state breakdown theory.” How this is relevant to the structural changes in Russia, which took place under Gorbachev, is unclear. More instructive in Collins’s example could be rather the personal role of Chinese leaders in social, political, and eco-

Soviet Union makes more sense; it is better explained, that is, theoretically more satisfactory, as Collins demands, for this factor specifies the initial conditions without which the laws we use in our retrodictions are useless. Apropos, James Coleman, notwithstanding his general sympathy for the project of macrosociological predictions, stresses the view that “Had Andropov been succeeded by someone other than Gorbachev, the East European governments and the Soviet Union might have remained intact for some time beyond 1989. There is an inherently lower predictability of one person’s actions.”²⁶

If Collins specified the conditions of the collapse of the Soviet Union well, and even if he guessed its terms more or less correctly, it would still be a poor explanation. The collapse was not an event, but a chain of heterogeneous events. It was impossible without perestroika and its unintended consequences, which in turn had been brought about by the particular leadership of Gorbachev. His leadership implied his individual features such as relative open-mindedness (especially compared with past Soviet leaders), his ability and willingness to listen to dissenters and the West, and his capacity to reconsider his own principles. Collins makes therefore also a sociological mistake: he neglects the tradition in the Soviet state leadership—a cultural “long-run structure,” by the way—that, from Lenin to Gorbachev, the personality of the party leader meant all too much for

nomic reforms in modern China; one will have a hard time to show a greater relevance of geopolitical “principles” over the personalities of Deng Xiaoping, Jiang Zeming, and Hu Jintao for the recent course of China. Illustrating significance of political leaders’ personalities (characters, idiosyncrasies, individualities, etc.) is fraught with infinite regress; yet as a brief illustration, it may be mentioned that the positions of quite a few world leaders on the impending war in Iraq had been determined to a great extent by their own views, principles, and ideals—not merely by internal and external pressures. To deny this personal factor would mean to perpetuate blind political mistrust and promote instead the geopolitical cynicism (often called realism) based on the irrefutable formula: whatever you do, you are guided only by self-interest and the current situation, never by principles.

26. Coleman (1995, 1618). For a most recent discussion of the role and intentions of Yuri Andropov in that historical situation, one may look at the memoirs by his associates in the Central Committee of the Soviet Communist Party, Arkadiy Volski, Alexandr Yakovlev, and Nikolay Ryzhkov, which commemorated the twentieth anniversary of Andropov’s short rule. Volski and Yakovlev explicitly endorse the idea that a longer leadership of Andropov would have meant preservation of the Soviet Union at least for the time he would be alive (*Moskovskiy Komsomolets*, November 20, 2002, in Russian). I am grateful to Igor Goncharov (York University) for bringing to my attention this source.

the whole course of the Soviet state.²⁷ All important, including geopolitical, decisions had been made either by the leader single-handedly or by the key figures within the politburo of the Communist Party. Gorbachev's personality therefore has to be a factor in our analysis.

Collins is trivially right that social changes are "cumulative processes"; that factors such as "overextension," the weakened economy, military expenses, and so on, matter in this story; and that even certain calculations may be useful.²⁸ Still, this is only a partial explanatory picture at best. In a causal genetic account of the collapse, these institutional pressures are as important as two groups of factors that, however, cannot be accounted for by these pressures: first, a lucky combination of numerous circumstances that moved precisely Gorbachev to the top of the Communist Party hierarchy; second, his whole biography that made him what he was in the 1980s. These are the circumstances that made possible and triggered the consequent upheavals that no equations can possibly calculate. In addition, the political decision to break up the country was made by the three leaders of then Soviet Russia, Ukraine, and Belarus without consulting their voters: the fact of importance is that a few months earlier, the vast majority of their citizens ("structures") expressed in a referendum their wish to continue living in the "renewed and democratic Soviet Union." Finally, it is worth recalling Popper's objection that a prediction taken into practical consideration is likely to alter, change, or cancel the predicted happening; the prediction fails. This objection is well suited for Collins's predictive geopolitics that cannot strictly speaking work for this reason either.²⁹

If we now look back at his point of departure, we see that none of Collins's hypotheses or predictions follow from the definition of state that he picked, namely, one of Weber's characterizations of the state by "military control over territory." He misses also that any other rea-

27. It was a sort of "Soviet Tsarism," though there was no immediate continuity between the Tsarist and Bolshevik versions of strong power centralism. Some point at the continuation of the tradition in post-Soviet Russia, too.

28. Collins (1986) uses throughout complex formulas supposedly showing the degree of overextension of Russia, China, and so on.

29. For instance, the leader of a great power may intrude, preventively or preemptively, in some region on the basis of his geopolitical advisers' long-term prognoses and change the whole situation in advance (or just in case, as the current American president has shown). Thus, the geopoliscientist capitalizes on the mere impossibility of testing his theories (appeals to ignorance, in other words). Unfortunately, the advice of this kind of theorist is taken in some governments today as respectable science.

sonable definition of the state emphasizing its different aspects (and Weber himself had different ways to describe the state) will have as little predictive power and as much capacity to find verifications as his choice has. Two variables, military control and territory, are a little too few to turn “this definition into an explanatory theory” as he meant to do. The military-territorial definition of state can be explanatory, though in a rather uninteresting, tautological way, but it is not in any way predictive. This is why Collins introduces more and more variables and ad hoc qualifications to account for pervasive counterexamples and new happenings. There cannot be a final version of geopolitical theory; it is simply a strategy of modifying and multiplying initial rules. Anybody who sells this strategy as a scientific approach and predictive theory makes false promises. It will suffice to say only that Collins’s theorizing about border change has a very low degree of refutability, that is, a weak grasp on empirical evidence.³⁰

To the question Did Randall Collins predict anything? one can answer as follows: he predicted nothing—precisely because he predicted anything by adopting the methodological policy of searching for confirmations and a hermeneutic approach to theory. Collins combines them with his eagerness to make sociology more scientific and believes that we are experiencing a genuine “Golden Age of macrohistory.”³¹ He links the maturity of sociology with its law-based forecasts, and his liberal way of making predictions might suggest therefore that sociology is immature or nonscientific. It would be a mistaken conclusion. The issue with the search for social laws seems not to be about the absence of ringing results, the laws as regular as physical ones; after all, we have no reasons to expect to find such regularities in human behavior. It rather lies in the tendency to exhibit any uncovered social regularities, unsettled lawlike generalizations, and putative theories as the yardstick of the scientific character and maturity of the social sciences. Such expectations misled some theorists to burden sociology with unrealistic tasks. The scientific character of any science, however, cannot and should not be determined by

30. Collins’s vision of the abilities of the social sciences is almost word for word Popper’s definition of what he calls “historicism”: “The ability of sociology to make valid predictions is a sign of the maturity of the discipline” (1995, 1588). Cf. “I mean by ‘historicism’ an approach to the social sciences which assumes that historical prediction is their principal aim, and which assumes that this aim is attainable by discovering the ‘rhythms’ or the ‘patterns,’ the ‘laws’ or the ‘trends’ that underlie the evolution of history” (Popper [1944-45] 1997, 3).

31. See “Introduction: The Golden Age of Macrohistorical Sociology” in Collins (1999, 1-18).

the existence of the alleged laws it uncovers; what is peculiar for science is not merely its findings but, in the first place, the way we ascertain their validity. Idiographic or historical sciences are as scientific as the natural sciences insofar as they are intersubjectively tested by experience; that is, they rely on the principle of intersubjective testability (and more generally, the intersubjective criticism approach; see Popper [1934] 1999, [1945] 1996). Depending on one's attitude toward this approach, the current theoretical blossom can be seen as a theoretical Golden Age and as a "Puberty of Historicism" as well.

One cannot rely on the contents of scientific knowledge to claim its scientific character, for there is a visible asymmetry between the ever-changing character of the contents of science and its stable methodological principles. The intersubjective criticism approach is necessary and sufficient for the purposes of attaining objective social knowledge, even if the results seem to us too modest so far. Unfortunately for declared social laws, this approach usually does not endorse them. It is widely surmised that scientific sociology emerged as an attempt to make sense of the social changes brought about by modernization. We live in a world as dynamic as the nineteenth-century positivists did, but we have probably little grounds for their optimism, shared by Collins and Bunge, in regard to uncovering historical laws. The social sciences, to the extent that they are scientific, rather lend more weight to nomothetic skepticism.

III. SOCIAL MECHANISMS AND SOCIAL SYSTEMS: SYSTEMISM VERSUS HOLISM IN BUNGE

Individualism and holism may be regarded as components or projections of systemism.

Mario Bunge (1999, 5)

Bunge seems to think that the mere mention of a supra-individual entity such as "the market" or "unions" inevitably gets the methodological individualist caught up in self-contradiction.

Alex van den Berg (2001, 94-95)

Given Bunge's disbelief that an event such as the collapse of the Soviet Union may be accounted for by any known laws (Bunge 1998, 22), it might be surprising that he accepts elsewhere Collins's prediction "from macrosociological conditions" (Bunge 1996, 382). It would be less surprising if one were acquainted with his own explanation

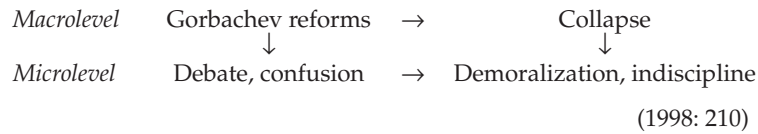
of the phenomenon that similarly neglects the factor of Gorbachev. He (Bunge 1998, 202-11) puts forward, as he does in other works, a systemist approach as an alternative to both methodological holist and methodological individualist attempts to explain major social changes. The collapse is explained by the systemist approach as follows:

The USSR had been suffering from severe systemic malfunction for decades, to the point of having become structurally unstable—a touch-and-go situation. It broke down for a number of interdependent causes of various kinds operating simultaneously on both macro- and micro-levels. (Bunge 1998, 205)

He groups fifteen long-term structural elements of the systemic malfunction into “three clusters: political, economic, and cultural” (Bunge 1998, 205). The failure of political scientists to anticipate and understand the collapse lies, thinks Bunge, in the fact that “political science is not yet scientific enough. In particular, it fails to integrate political analysis with economic, cultural, historical, and psychological analysis; and it focuses too often on either personalities or political systems.” The impression is nevertheless that the proposed strategy called “systemism” is to be placed at the limit “structures” on the scale structure-agency, or otherwise, at “holism” on the scale holism-individualism. The Soviet leader is present in Bunge’s explanation only to show that “The progressive reforms introduced by Gorbachev and his team in 1985 came too late, when the regime was already gone” (Bunge 1998, 208). A remarkable thing about this conclusion is its familiar Marxist and holist flavor: long-term social structures, or wholes, or systems had done the job themselves, while the role of Gorby & Co. was probably just to “alleviate birth pangs” of the impending breakdown. As we will see, Bunge admits at the same time (but at another place) such a factor as “the intervention of the right person at the right place and time” in major social change (Bunge 1999, 27).

Although the above account sounds rather holistic, or purely structuralist, Bunge grants that it somehow secures relevant individual (micro) factors of the collapse, too:

The following Boudon-Coleman diagram summarizes in an oversimplified fashion the myriad macro-micro processes involved in the last act of the Soviet tragedy:



To make sense of the way the diagram explains the collapse, or fails to do this, we focus on the central notions in Bunge's social philosophy, namely, "social mechanisms" and "social systems," and their bearing on the micro-macro link, even though understanding of mechanisms and systems is burdened with abundant definitions and classifications throughout his works.

Social mechanisms. Bunge constantly stresses the importance of studying mechanisms, shows dissatisfaction with some uses of "mechanism" in the literature and in book indexes (e.g., Bunge 2004, 192), and promotes a "mechanismic" kind of explanation. Social mechanisms, and mechanisms in general, are characterized by a great many features. "Mechanisms" refer to the way real things work (1999, 17-18). They are processes (pp. 18-22): "I stipulate that a *mechanism is a process in a concrete system*" (p. 21); though not all processes are mechanisms: "Every mechanism is a process, but the converse is false" (p. 24). A concrete system, in turn, is "a bundle of real things held together by some bonds or forces, behaving as a unit in some respect" (p. 22): molecules, stars, families, and entire societies are examples of concrete systems. The latter are not to be confused with structures because "every structure is a property, not a thing" (p. 23).

Mechanisms are of diverse varieties: causal, probabilistic, or mixed (p. 26). They are also classified into "Type I, or involving *energy transfer*, as in manual work and combat; and type II, or involving a *triggering signal*, as in giving an order to fire a gun or an employee" (p. 27). The peculiarity of the latter is that

[In] Type II processes the effect may be "disproportionate" to the cause: that is, a very small cause may trigger a process ending up in a catastrophic effect—such as the proverbial shout in a canyon that triggers a landslide. This is particularly the case with unstable systems, such as social systems relying on a strong but, alas, mortal leader, as well as with unpopular governments that rely only on coercion. . . . Perhaps all major (that is, structural) social changes involve tangles of causal arrows of both types, enhanced or weakened by "accidents" or interfering circumstances, such as bad weather, . . . or the intervention of the right person at the right place and time. (Bunge 1999, 27)

It is an important point, for one can see that this strategy, unlike the account in Bunge (1998), potentially admits Gorbachev's personality—that is, his actions and endeavors—as a causal factor in the collapse discussed.

Mechanisms' ontological aspect is specified as follows: "It may be conjectured that causal mechanisms of both types exist on all levels of reality" (p. 27), and they are rendered as an "ontological category" (p. 65). Accordingly, a proper (causal) explanation is one that points to causal mechanisms (pp. 33-47). Further discussion of social mechanisms is particularly challenging because of numerous definitions and the introduction of additional, if not superfluous, elements, classifications, and constant parallels with the hard sciences. Bunge brings in, for example, the notions of "social forces and powers" whose scope and application are quite cryptic.

Though the existence of a force implies that of a mechanism, the converse is not true. For example, voting, public debate and mass mobilization are mechanisms for democratic political change (or stasis), but they are not forces. On the other hand public opinion, coercion, graft, and lobbying for special interest groups are political forces because they alter the mechanisms of a democratic polity. (P. 41)

The division of the two groups of phenomena into forces and nonforces seems rather optional; in a more familiar sociological slang, they all may well be categorized as "institutions," which, respectively, do or do not alter "the mechanisms of a democratic polity." The use and status of "forces and powers" is still less clear given that "to explain social change one need not always invoke social forces or powers—unless these actually exist and are well-defined, which is seldom the case" (p. 41).

Bunge points out the vagueness of the concept of social mechanism and says, "The system/mechanism distinction may seem subtle and is somewhat obscure."³² His own usage, however, does not make it any clearer. To give an "intuitive grasp" (p. 55) of social mechanisms, he offers a list of ten examples with corresponding empirical finding; a couple of examples are as follows:

32. See p. 58. Cf. "The concept of a social mechanism is somewhat vague because it has been insufficiently analyzed and theorized" (Bunge 1999, 55).

<i>Empirical finding</i>	<i>Hypothetical mechanism(s)</i>
2. All social systems decline unless overhauled.	Decreased benefits, intensification of internal conflicts, unresponsiveness to environmental changes
8. The Soviet Union crumbled in 1991.	Greater freedom of dissent, economic stagnation, ethnic conflicts, lack of mechanisms to implement perestroika

We are ready for a formal definition. We define a *social mechanism* as a mechanism in a social system. (Bunge 1999, 56)

Unfortunately, the definition is not very informative (it has the form “*x is x in y*”; note also the “lack of mechanisms” in the mechanisms column). My intuitive grasp of the items in the right column suggests that they should be more properly called “empirical findings” and placed therefore in the left column (not in the right column under “mechanisms”). Another of Bunge’s ten examples of social mechanisms (pp. 58-59) contains Tocqueville’s observation about the dependency between the degree of oppression and rebellions, but it figured earlier in the text as a social *law*, not a mechanism (p. 9). Furthermore, to save his approach from the inconsistencies that he discerns in rational choice theories and among “holists,” Bunge iterates that he holds

the systemic view, according to which agency is both constrained and motivated by structure, and in turn the latter is maintained or altered by individual action. In other words, social mechanisms reside neither in persons nor in their environment—they are a part of the processes that unfold in or among social systems. . . . Mechanism is to system as motion is to body, combination (or dissociation) to chemical compound, and thinking to brain. (Pp. 57-58)

The upshot is that Bunge constructs an intricate social ontology of intertwined systems, mechanisms, laws, structures, processes, and individuals.

All mechanisms in general are described as “system-specific” and as belonging to four kinds: physical, chemical, biological, and social, as well as to “hybrid” kinds (pp. 59-60). Our understanding of human behavior might be facilitated therefore by the knowledge that social changes are “like chemical reactions in that the mechanisms operating in both cases consist in the making or breaking of bonds or ties. And the competition between two firms for a given item resembles

the competition between two chemicals reactants for a third" (p. 60). Yet, after having mastered the gist of these analogies, the reader stumbles over the following caution:

To be sure, all the above are just formal and therefore superficial, if bold, generalities garnered by gutting and collecting particular cases. Consequently, hypergeneral hypotheses or theories of growth, decline, selection . . . or any other generic mechanisms, can explain no particular facts—let alone predict them. (P. 60)

In his most recent attempt to clear up the nature of mechanisms, Bunge says also that they can be nonessential and essential, the latter being "the specific function of a system" (Bunge 2004, 193). He posits that there are "metamechanisms" (p. 185) and specifies one of his older classifications stipulating now that they can be causal, random, and perhaps chaotic (p. 196); on the top of all varieties of mechanisms, he attributes to them human qualities: "Military aggression, protracted dictatorship, and terrorism . . . are by far the most destructive, divisive, and irrational, and therefore also the most *barbaric and immoral*, of all political mechanisms."³³

Terminology aside, another major difficulty found in Bunge's account of mechanisms is the repeatedly stated idea that a mechanistic causal explanation is somehow superior to the nomological model of explanation. He concedes that mechanism and law "can be uncoupled only in thought" (2004, 198), and further on it becomes evident that they cannot be, even in thought: satisfactory scientific explanations "resort to law statements. So, mechanistic hypotheses do not constitute an alternative to scientific laws but are components of deep scientific laws."³⁴ Bunge's jargon needs a word of caution again: the above expression, "mechanistic hypotheses," can be replaced in all cases simply by "hypothesis" or "law" without loss of meaning; this follows at least from his previous and further assurances such as "no law, no possible mechanism" (2004, 207), which make the expression "mechanistic" redundant. Yet, he needs it to support the thesis that there is such a thing of its own kind as mechanistic explanation.

33. Bunge (2004, 185-86 [italics added]). Furthermore, he speaks of "conservative mechanisms" (p. 208). The talk about immoral mechanisms is convenient for aggressors and dictators themselves: it is not they but social mechanisms that are to blame; we will see similar consequences to Bunge's systemism, too.

34. See pp. 199-200. Some scientific laws are so deep indeed that they allow Bunge to make a sort of scientific value judgment about, say, "the cultural poverty of contemporary Islam" (p. 192).

It is easy to show nevertheless that the difference between the nomological and mechanistic explanations is merely verbal by using Bunge's own examples. First, he finds two big troubles with a nomological (covering-law, D-N, etc.) explanation that is attained by subsuming a particular under a generalization: (1) it is all "true, but it does not elicit understanding"—he, however, is perfectly silent as to why it does not, or what the conditions for proper understanding are; and (2) it "fails to capture the concept of explanation used in the sciences, because it does not involve the notion of a mechanism" (2004, 202). He gives then some examples of mechanistic—supposedly not nomological—scientific explanations:

For instance, one explains the drying of wet clothes exposed to sunlight by the absorption of light, which increases the kinetic energy of the water molecules in the wet cloth to the point that they overcome the adhesive forces. . . . Unemployment of a certain kind is partly accounted for by the spread of labor-saving devices, which in turn is driven by the search for decreasing waste and increasing profits. (P. 202)

To summarize, the idea is that, to get an authentic scientific explanation, it is not enough to state a generalization and then subsume under it the event to be explained. Two new conditions are (1) to refer to some mechanism, that is, to use "mechanism," and (2) to expand on the premises.

In an "unscientific" form, Bunge's (scientific) graphic example of the drying clothes might go as follows: "Heat makes water evaporate: Sunlight produces heat: Sunlight makes water evaporate (from the clothes)." This does not lead to understanding so far, Bunge insists; one has to explain first the mechanisms of the involved laws and initial conditions; hence, "the kinetic energy of the water molecules," and so on instead of the mundane (and allegedly less understandable) "evaporation." Then where do we stop in our explanation? A consistent mechanistic tells us that Bunge's explanation is incomplete also, and it does not make sense: we do not actually get why clothes are getting dryer, unless the mentioned mechanisms of absorption of light, adhesive forces, and molecules' motion are further explained by respective laws or mechanisms. If some unemployment is explained by labor-saving technology, and this in turn is explained by "the search for decreasing waste and increasing profits," then the latter generalizations too have to be explained in turn by other laws or mechanisms; otherwise, following Bunge's argument, we still do not have a proper explanation or understanding of unemployment. His

idea amounts to the proposal to ban, for example, the use of tacit laws and to decipher all information contained in the explanatory premises; simply, to make a short story long—endless, indeed; this idea is plainly impracticable at least because it leads straight to infinite regress.

That the mechanistic explanation is merely a complication of the nomological model is seen also from Bunge's admission that "In all such [explanatory] cases, *to explain is to exhibit or assume a (lawful) mechanism*" (2004, 203), as well as it is seen from Charles Tilly's attempt to use Bunge's approach. Tilly sets out to apply Bunge's "program of mechanistic explanation" to his study of the formation of social boundaries and argues that

identification of relevant causal mechanisms will produce superior explanations of boundary-involving social phenomena . . . than could any likely invocations . . . of covering laws in the form "All boundaries ____." This article, however, makes no efforts to prove that sweeping claim. (Tilly 2004, 215)

The irony is that in his effortless way, Tilly shows just the opposite, that he does stick with the nomological model though, unlike Bunge, he does not make it unwieldy. In the section "Mechanisms That Cause Boundary Change," he offers five types of such mechanisms: "encounter, imposition, borrowing, conversation, incentive shift" (p. 218). Let us not forget also that mechanisms are inseparable from laws—or put simply, consist of laws—as Bunge reminds us, and as both he and Tilly show. Tilly gives then a long list of illustrations of the five types of mechanisms where each illustration is a lawlike statement; that is, having exactly the form "All X are ____," which he just set aside.

Encounter. When members of two previously separate or only indirectly linked networks enter the same social space and begin interacting, they commonly form a social boundary at the point of contact. . . .

Imposition. Authorities draw lines where they did not previously exist, for example distinguishing citizens from noncitizens, landowners from other users of the land, or genuine Christians from insufficiently pious persons.

Borrowing. People creating a new organization emulate distinctions already visible in other organizations of the same general class, for example, by instituting a division between hourly wage workers and employees drawing monthly salaries. (Tilly 2004, 218-19)

Further on, Tilly explains that these types of mechanisms often work jointly, have their opposites and counterparts, contain “more microscopic” mechanisms within them, and have other taxonomic subdivisions. Keeping focus on our specific problem, we cannot see in his discussion in what sense explanatory mechanisms are different from mere laws (generalizations, regularities) and why we cannot mean by “mechanism” what it usually means, namely, a group of related laws. Bunge and Tilly do not show, as they promise, that mechanistic explanation is in any way deeper than or superior to—or even any different from—the more familiar nomological form of explanation. At one point, Bunge makes this conceptual detour still more evident when he writes, “No law, no possible mechanism; and no mechanism, no explanation” (2004, 207). The “middle term” in this formula—mechanism—may not be superfluous only if it serves as a shortcut for grouping laws for the sake of economy; otherwise, for the purposes of explanation it is an excess. It is hard to miss that mechanism-ism still remains a conceptual confusion, and some participants of the Symposium on Bunge acknowledge that it was confusing even without Bunge’s effort;³⁵ while mechanistic explanation is an overloaded version of the good old nomological model and adoption of this proposal can only make explanatory work cumbersome and unmanageable.

Bunge (2004, 208) anticipates that the headway of mechanistic explanation may not be very quick and gives the delay accordingly a mechanistic explanation: “Who said there is no progress in philosophy? It may be slow because of the operation of conservative mechanisms—such as neophobia, willful ignorance, obscurity worship.” Insofar as the pernicious mechanisms stand for some well-corroborated laws, as Bunge requires, and they do apply to the disagreement with his ideas, this is of course a good explanation.

Social systems. Bunge’s *The Sociology-Philosophy Connection* is permeated by the author’s plea to adopt “systemism” as the most if not the only adequate method of social scientific explanations.

The concept of system is central to sociology because every person is part of several “circles” (systems), and behaves somewhat differently when acting in different systems. The latter, in turn, are affected by their components. In short, no agency outside some system, and no system

35. See contributions by Renate Mayntz (2004) and Colin Wight (2004).

without agency. . . . Hence individualism and holism may be regarded as components or projections of systemism. (Bunge 1999, 5)

Some systems are spontaneous (e.g., families, circles of friends, local markets, most towns): "Designed systems and their corresponding mechanisms are usually called 'organizations.' An example of an organization is the law-enforcement system, a social control mechanism," and so on (p. 61). Quite unexpected is the following rendition of society as consisting of "organizations," with the exclusion of spontaneous systems: "Assuming that every society is made up of three artificial systems—the economy, the polity, and the culture—leads to distinguishing the corresponding types of social mechanism" (p. 61); as startling seems the proposal to assume that economy, polity, and culture are "artificial" systems.

Bunge repeatedly gives reasons to interpret him as methodological holist and individualist at once. A puzzling passage contains two contradictory statements: first, there are "individuals belonging to different levels"; second, "any given social fact is ultimately a result of individual action" with a submission that "this is the true component of ontological and methodological individualism" (p. 62). The way out is found as follows:

[All] social relations hold within or among social systems; and, wherever there are systems, at least two levels must be kept in mind. These are the microlevel, or level of the system components (such as persons and social subsystems), and the system (or supersystem) level. Any number of intermediate levels may of course have to be interpolated. (P. 62)

There are two (macro and micro) problematic assumptions: the existence of social systems and of their multiple levels. Moreover, the latter claim is difficult to reconcile with another of his beliefs that "there are no degrees of existence—save in certain theologies" (p. 75), unless one considers systemism a theology. Bunge enters into illustrations of the micro- and macrolevels with the help of three Boudon-Coleman diagrams without saying, however, how all those arrows and dispositions work and explain, let alone how they are supposed to certify the existence of multilevel systems. They also do not show that the alleged failure "of both individualism and holism suggests that the adequate alternative to both is systemism" (p. 66).

What seems to fail is rather Bunge's attack on individualism. The view, and methodological principle, "according to which every

whole is nothing but the collection of its parts" is mistaken, Bunge thinks. This is why: "An army is not just a bunch of soldiers: it is a social system held together and organized by relations of command and cooperation. A disorganized mob of soldiers is not called an army but a ragtag of ex-soldiers" (p. 89). The evidence of this system's existence is nevertheless merely verbal: one type of relations among many people is defined as "army" and "social system" and the other type is not. The holist or systemist may rejoin, but there *are* the structured relations. True, the relations, structures, and systems do exist—moreover, Bunge unwittingly shows at another place that they exist in full accordance with individualism: "social relations pass through the heads of people" (p. 62). Pretty much enough. Relations, structures, and systems of course exist as our shared beliefs, thoughts, values, attitudes, expectations, and knowledge of rules: they, collectively usually called ideas, make a bunch of soldiers behave in an organized and well-coordinated fashion. "Army" in turn stands for their multiple and uniform behavior.

Bunge illustrates then the existence of social systems by means of making any agent a spontaneous sociologist or philosopher of the systemist/holist kind (incidentally, such an attribution to agents of theoretical qualities is one of his reproaches to Popper):³⁶

Certainly, individual actions sustain or undermine social networks and formal organizations. But they can do so only provided the individual recognizes the existence of such supra-individual entities and adapts to them at least to some extent. Even someone intent on undermining an organization must start by admitting its existence, particularly if he intends to fight it from within. In so doing he jettisons whatever individualist philosophy he may uphold in theory. (P. 89)

Bunge assumes (wrongly) that mere use of verbal shortcuts amounts to the recognition of the existence of corresponding "supra-individual entities." (As was noticed earlier, mere usage of "mechanisms" was his reason for advancing "mechanismic" explanation.) If the individual sees in the army what she sees—namely, people who share certain norms, views, rules, and like structures in their minds, and in "army" sees first of all a word—she will not need to get rid of the individualist philosophy she may uphold to undermine an organization. The abandoning of individualist philosophy is most likely to make one's

36. Discussing Popper's view that "If the method of rational critical discussion should establish itself, then this will make the use of violence obsolete," Bunge sarcastically remarks, "In other words: Let's all become intellectuals" (Bunge 1999, 115).

efforts counterproductive, for they will be based on weakened explanatory and hence instrumental grounds; the practical implications of the systemist outlook may have certain affinities with political sentiments à la Foucault to fight pernicious “systems,” not to handle particular issues.³⁷ Furthermore, even if most people happened to believe in the entities’ independent, this existence would show merely the existence of their beliefs, not of the entities. Bunge goes on to find an ally in the system-oriented individual who presumably “confirms the view that there is no agency without structure and conversely: agency and structure are just two sides of the same coin.” This nonetheless is poor evidence that the coin is something more than individual behavior and thought. The author’s arguments for the existence of powerful and self-contained systems one reminds of insinuations of the age-old type: “Behold, Behemoth . . . his strength in his loins, and his power in the muscles of his belly” (Job 40:15-16)—but never presents “Behemoth.” This is the reproach to Bunge made recently by Alex van den Berg (2001, 94-95):

Bunge seems to think that the mere mention of a supra-individual entity such as “the market” or “unions” inevitably gets the methodological individualist caught up in self-contradiction. But this is only so if the market or unions *necessarily* contain features that are irreducibly supra-individual. This is not self-evident. Bunge’s several exemplary systemic explanations of various phenomena such as the rise of Peronista populism and the collapse of the Soviet Union make liberal use of supra-individual entities and events, but it is not immediately clear to me, nor does he explicitly try to demonstrate, that any of them are so irreducibly “emergent” that they could not be described as aggregates of individual actions.

In his reply to van den Berg, Bunge’s main evidence consists rather in the existence of other theories using systemic concepts (Bunge 2001, 404-406). He elaborates the sociosystemic ontology that becomes pan-systemic: it is maintained that “everything is either a system or a component of one” and “all systems have universal emergent properties” (p. 406). In the realm of the social, “social order, political stability, and national development are properties of whole societies” (ibid.). Bunge returns to the example of the army and invokes Leo Tolstoy, “who had fought in two wars [and] knew that an army is not the same

37. As Michel Foucault instructed at a meeting with high school students, “[We] can’t defeat the system through isolated actions; we must engage it on all fronts”; “Reject theory and all forms of general discourse. This need for theory is still part of the system we reject” (Foucault [1971] 1977, 230-31).

as the collection or amalgamation of its constituents" (p. 407). The authority of Tolstoy is meant to support the following view: "What distinguishes a regular army from any other collection of individuals is the possession of such systemic properties as hierarchy, command chains, specific missions. . . . All these features submerge on demobilization—submergence being of course the dual of emergence" (ibid.).

Tolstoy, too, seems to be a systemist in the sense that he believed in the decisive role of the whole societies' and peoples' "properties," but he at least admitted that the de/mobilization was the result of the commander's orders as well. If Bunge mentioned this plain fact, he would probably have to continue that army and command chains (i.e., countless singular interactions) work through the chief to the subordinate individuals to the subordinate individuals, again, as their individual acceptance of the rules, readiness to obey orders, and actions. We naturally use "hierarchy" as a label for this complex process, but this has nothing to do with the whole army's alleged properties or moreover its autonomous existence, and such labels do not explain the army's behavior unless they are translatable into individual behavior.

Bunge's systemist ontology is reflected in the systemist method. He is not only convinced that systemism explains adequately the Soviet breakdown but also that this approach is a reliable alternative to other methods of making predictions. Bunge blames, for example, rational choice theorists, who uphold individualism, for their failure to predict the outcome of any major international conflicts such as "the nuclear confrontation [which began before the invention of that theory] and the American intervention in Vietnam" (1999, 91). As was said earlier, however, these expectations cannot be addressed to the social sciences in general unless they employ as relaxed methods as those Collins uses. No science can possibly possess relevant information about the future "myriad macro-micro processes," as Bunge (1998, 210) puts it, to predict such events; this is why one at best can "summarize in an oversimplified fashion" (ibid.)—not explain or predict them—with Boudon-Coleman diagrams and arrows. Tilly (1995, 1602) points to this link between social ontology and methodology:

If the social world actually fell into neatly recurrent structures and processes, then epochal theories, invariable models, and the testing of deductive hypotheses would become more parsimonious and effective means of generating knowledge. Because the social world does *not* con-

form to that prescription, we need other programs on both ontological and epistemological grounds.

Bunge is trying to show also that systemism is a routine method: "In sum, a practicing social scientist can be neither a consistent holist nor a consistent individualist. Whether or not he knows it, he is a systemist, that is, someone who studies social systems" (1999, 166; cf. Bunge 1996, 264). In a similar way, Weber treated ideal types as a commonsensical method,³⁸ and Popper took for granted his concepts of the logic of situation and piecemeal social technology.³⁹ It is far from evident, nevertheless, that Bunge has made a strong case for systemism as a self-evident method.

In conclusion, Bunge's puzzle seems to consist in a discrepancy between his two explanatory strategies. It is the method of studying social mechanisms that—in spite of numerous terminological and taxonomic complications—still sounds commonsensical and plausible; it translates into the language of regularities or laws, and also leaves a certain gap for individuals and thus makes more sense. And it is his systemist approach and ontology that are in conflict with the strategy of studying social mechanisms; this approach fails, as Collins does, to give individual factors their proper significance and eventually ends up as a variety of holism.⁴⁰ On one hand, Bunge (1999, 22) believes that pervasive mechanisms are not merely "pieces of reasoning but pieces of the furniture of the real world"; "Type II [i.e., individual-friendly] mechanisms are particularly conspicuous and important on the biological and social levels" (p. 27). The systemist approach, on the other hand, seems not to allow him to carry out the

38. Raymond Aron interprets Weber's rejoinder to historians like this: "You in fact do exactly what I have just described" (Aron 1967, 194). As Hans Gerth and C. Wright Mills wrote, "By using this term ['ideal type'], Weber did not mean to introduce a new conceptual tool. He merely intended to bring to full awareness what social scientists and historians had been doing when they used the words like 'the economic man,' 'feudalism,' 'Gothic versus Romanesque architecture,' or 'kingship'" (Gerth and Mills [1946] 1958, 59). Cf. Anthony Giddens: "In setting forth the formal characteristics of the ideal-type concepts, Weber does not consider that he is establishing a new sort of conceptual method, but that he is making explicit what is already done in practice" (Giddens [1971] 1994, 141).

39. "The best historians have often made use, more or less unconsciously, of this conception [logic of situation]" (Popper [1944-45] 1997, 149); Popper says that the approach of piecemeal social technology "might indeed be called the classical one" (p. 58).

40. Bunge (1996, 265) writes that there is a "legitimate grievance" that systemism is "so vague as to be trivial and indistinguishable from holism." The following discussion (pp. 265-81) seems only to make the grievance more legitimate.

promise of studying mechanisms. Systemism claims to assimilate well both individualism and holism (which are “components or projections of systemism”) but does not reconcile them, which can be seen in his social explanations. It seems that the chief problem lies in Bunge’s social ontology overloaded with systems, processes and mechanisms, and individuals; but at any rate, his approach is overcrowded terminologically. As a consequence, systemism has much in common with what is often called holism and collectivism and, as I show in the next section, leads Bunge to holistic views on social planning. Since Bunge invokes Tolstoy, it may be interesting to note that the way Popper describes that writer’s historicism aptly characterizes systemism as well: “In his [Tolstoy’s] version of historicism, he combines both methodological individualism and collectivism; that is to say, he represents a highly typical combination—typical of his time, and, I am afraid, of our own—of democratic-individualist and collectivist elements” (Popper [1944-45] 1997, 148).

IV. BUNGE’S SYSTEMIST CRITICISM OF POPPER’S SOCIAL PHILOSOPHY: SOME SOCIOLOGY-PHILOSOPHY DISCONNECTIONS

The chapter begins with Bunge’s recollection of his accidental encounter with *The Open Society and Its Enemies* in the late 1950s, and with his admiration for the book. The following correspondence with Popper ([1934] 1999, 103) “sparked off a friendship that lasted a quarter of a century” and led Bunge to edit in 1964 *The Critical Approach to Science and Philosophy* in Popper’s honor, where he praises Popper’s influence, clarity, range of subjects, criticism, empiricism, and realism (Bunge 1964, vii-ix). *The Sociology-Philosophy Connection* too contains some references and eulogies to Popper, but the chapter on his social philosophy ends up with quite an odd conclusion. It has become almost commonplace to think of Popper’s philosophy as systematic, whereas Bunge (1999, 127) finds it “though extremely interesting . . . fragmentary (unsystematic) and rather shallow.” And it may not be extremely interesting, for Popper “made no lasting contributions to social explanation” (p. 106), and “has had nothing original, let alone constructive, to say about any social order” (p. 125).

Before entering into details, a remark is in place. It is not my present task, of course, to trace the development or continuity in Bunge’s commitments, or to find the reasons for his ambivalent treatment of

Popper's philosophy; my focus here is the way Bunge tackles different aspects of his social philosophy. The standard problem arises from the outset when these aspects are cut off from the more general context of Popper's philosophy; then the connections among the aspects are established anew, that is, in the way Bunge himself sees the connections. Some of the reestablished connections, however, are different from those made by Popper, or are in addition to Popper's, and some are just missing. As a result, the whole philosophy looks less systematic than it might. Put simply, Bunge's interpretation is not perfectly accurate; Popper's social philosophy can reasonably be seen as a more coherent picture than the random collection of fragments Bunge makes it out to be.

He singles out seven "main pillars of Popper's social philosophy: rationality, individualism, libertarianism, antinomianism, negative utilitarianism, piecemeal social engineering, and a sunken pillar—that of the desirable social order" (p. 103). Having isolated them, he concentrates on their respective defects, though some of the defects appear (1) due to the very isolation of the "pillars," and (2) due to the fact that the number of the pillars is quite arbitrary.⁴¹ A rather common reading is that, notwithstanding certain difficulties in Popper's philosophy, its different elements—including the seven pillars pertaining to his political views and philosophy of science (epistemology)—look pretty much interconnected and resolved along the lines of the main and systematizing principle of his philosophy, namely, fallibilism.⁴² The disregard of this principle deprives many of those ideas of their meaning. (Given Bunge's disposition toward a systemic vision, one might expect a more systemic treatment of Popper's philosophy.) The way Bunge interprets the selected pillars, and links political with epistemological ideas, leads him to the conclusion

that the pillars are there all right, but they are shaky and do not support a construction so profound and consistent, as well as ample and detailed, as to deserve being called a substantial social philosophy, let alone one capable of inspiring any social activists or politicians. This may explain why neo-conservatives, classical liberals, and democratic socialists have claimed that Popper is on their side. Thus, Popper's legacy is no less ambiguous than Hegel's or Marx's. (P. 103)

41. If Bunge borrowed the simile from Lawrence's *The Seven Pillars of Wisdom*, someone else, inspired, say, by Hemingway, might call up "five columns," and Boccaccio would have produced still richer analysis.

42. It is not a defensive interpretation, but the point made repeatedly by Popper himself and his numerous commentators.

There are a few suspect suppositions in the fragment: that Popper ever had an intention of creating an ample, detailed, and substantial social philosophy, and that the sign of such a philosophy's strength is its inspiration of any social "activists" (though Popper targets very specific political teachings). There are also two contradictory assumptions: that Popper inspired nobody and that the most influential political forces (except totalitarian) assimilated his ideas. The fact that neoconservatives, classical liberals, and democratic socialists accept some of Popper's ideas may suggest rather affinities among these ideologies, not necessarily incongruities in Popper. Finally, the remark that "Popper's legacy is no less ambiguous" than Marx's can be taken, politically, as a compliment, and, intellectually, as a result of multiple misinterpretations. It may sound audacious to say that Bunge contributes to the misinterpretations as well; still he constantly gives reasons to the reader to do so.

Bunge stresses the negative aspect of Popper's notion of rationality, which is similar both in politics of the open society and in science: rationality entails critical discussion that is as necessary for freedom and democracy as it is for science (p. 104). Now, "What about efficient action?" Bunge asks,

Obviously, negative rationality won't help us here. Efficient action calls for practical rationality because we need to know whether a given practical issue does call for action and, if so, which action is to be taken. But what is practical or instrumental rationality? Popper did not tackle this question in any detail in *The Open Society*. (Pp. 104-105)

Popper's failure to elucidate this issue, maintains Bunge, consists also in his antiessentialist approach that in particular does not allow him to produce good definitions. Popper's ([1967] 1985) later attempt in "The Rationality Principle" "gave rise to a modest but thriving academic industry" (the present discussion avoids contributing to the industry) but does not give a stable meaning of "rationality" and understanding of its methodological status (Bunge 1999, 105). The vagueness, fuzziness, and inconsistencies of Popper's use of "the rationality principle" therefore, prevent him, Bunge thinks, from answering the question, Which action is to be taken?

Furthermore, since Popper's rationality principle is empty, so is his situational logic, which he claimed to be able to explain human actions

and social facts—even though he never sketched it. Thus Popper made no lasting contributions to social explanation. Moreover, he could make none for the following reasons. First, agency without social structure is a figment. Second, scientific explanation proper . . . involves exhibiting or conjecturing some mechanism—that which makes individuals or social systems “work” or “tick”—not allusion to some nondescript “situation.” (P. 106)

This representation suggests a few objections. To begin with, Popper does connect his notion of rationality with rational action, not, however, with the prophetic question “Which action is to be taken?” but above all with “What actions should we not take?” Bunge rightly emphasizes Popper’s negativism, and it is unexpected that he omits the apparent connection between the two big topics in Popper’s philosophy—that is, rationality and the prohibitive character of technological and piecemeal engineering—topics that Bunge studies separately. It is not that this connection is suggesting itself, it is rather explicit in Popper: “It is one of the most characteristic tasks of any technology to *point out what cannot be achieved*” (Popper [1944-45] 1997, 61).

It is unclear also why the concept of “instrumental rationality” had to produce trouble for understanding Popper; why one cannot read it in accepted terms of means-ends rationality. It is true that Popper’s rendition of the rationality principle is far from impeccable; it remains so even in the later revised version, “Models, Instruments, and Truth” (Popper [1963] 1996). The impression is, however, that Bunge finds a too easy prey here. The subject might deserve his greater attention for it could reveal, for instance, important similarities between Popper’s explanatory model construction and Bunge’s “mechanismic” explanations. A closer look at situational logic can show at the same time that it is more efficient exactly where systemo-holism fails: once more, recall the attempts to explain the Soviet collapse of which systemism is able to produce a mere “summary,” not a plausible explanation. The claims that Popper “made no lasting contributions to social explanation” and fails to give “scientific explanation proper” are still less warranted on factual grounds. The deductive analysis of causal explanation is often associated with the name of Hempel, but he acknowledges Popper’s *Logik* to be one of the sources of his model.⁴³

43. See, for example, Hempel (1965, 251). Yet both Popper and Hempel are almost silent about their historical predecessors upholding similar (D-N) views of scientific explanation.

Popper's views have been familiar and influential among social scientists, especially in economic methodology, for quite a long time.⁴⁴ The impact of his ideas on contemporary sociology has been shown recently as well.⁴⁵ And, returning to the issue of Popper's impact on politics, even though it is rare that a professional philosopher has any effect on politics, clearly Popper has been known among statesmen.⁴⁶ Bunge hardly is unaware of these facts; the fact is that he for some reasons omits them in his appraisal of Popper's social philosophy.

"Individualism" is the element that Bunge finds especially deficient in both the political and scientific philosophy of Popper. The substance of his counterarguments, however, is not terribly clear. Bunge, once more, merely postulates the existence of social systems ("every human being is part of several social systems") and their emergent properties (p. 107). This time, his evidence is found in running a corporation, which means "to make or sell commodities that neither of its individual components could handle." This is the mark of all social systems: "They have supra-individual features stemming from the division of labor, the cooperation, and the conflicts among their members" (p. 107). Let us extend this line of reasoning. Consider a small grocery store run by two, a supermarket, a chain of supermarkets, a transnational corporation, a regional economy, and the world economy. All of them are then to be called "social systems": at all levels—levels of *analysis*—we find respective division of labor,

44. On Popper's influence on economic methodology, see the economists Bruce Caldwell (1991, 1998), Neil De Marchi (1988, 1992), Wade D. Hands (1992, 1993), Richard Langlois (1986), and Deborah Redman (1991, esp. pp. 111-16).

45. Peter Hedström, Richard Swedberg, and Lars Udén (1998) studied the impact of his ideas on contemporary sociology and demonstrated it quantitatively by means of a content analysis of references in leading sociology journals from 1960 to 1996. It turns out that Popper was the most frequently cited philosopher and he outdid in this respect—especially in Europe—Kuhn, Hempel, and Wittgenstein: "[It] is obvious that Popper's ideas have made deep inroads into the discipline of sociology" (Hedström et al. 1998, 343).

46. The issue of Popper's impact on politics perhaps deserves a special study; for the present discussion, a few remarks are in place. I am grateful to Professor Jeremy Shearmur for his suggestions on the subject, as well as for the observation made in e-mail correspondence: "Popper was, I recall, at one point favoured by all three main German political parties (this was before the rise of the Greens)." A similar remark is found in Bryan Magee: "Progressive cabinet ministers in both of the main British political parties, for instance Anthony Crosland and Sir Edward Boyle, have been influenced by Popper in the view they take of political activity" (Magee 1973, 2). (These observations may be seen also as a reply to Bunge's suggestion that Popper was reputed by different political forces to be on their sides.) See also the former politicians' accounts on the issue: Magee (1995), Roger James ([1980] 1998), and Edward Boyle (1974).

cooperation, and conflicts. What is surely “supra-individual” in this progression is the growing number of the agents and the amount of their interactions and of produced goods. Does the existence of social systems emerge out of these? Big numbers themselves are a scarce argument for the systemist ontological claim. Bunge cautions,

The systemist thesis, that society is a system of systems, should not be mistaken for holism or collectivism. Whereas the latter is irrationalist, systemism holds that only an analysis of a whole into its components and their interactions can account for it. . . . Hence, it is not touched by Popper’s [1944-45] devastating critique of holism. (Ibid.)

Yet it is difficult not to mistake systemism for holism, or not to see that Popper’s critique applies to systemism by implication. “Whoever denies the existence of social systems,” continues Bunge, “is bound to either smuggle them in or invent surrogates for them. Popper was no exception” (p. 107). Here he makes another unsupported connection: for the individualist, conceptual surrogates stand for big numbers, and the use of shortcut terms does not oblige us to hypostatize them—but it seems to compel Bunge.

He resorts once more to his favorite argument from the military: “The fate of the troops depends not only upon the decisions of their officers but also upon such suprapersonal items as transportation and communication” (pp. 107-8): How much less irrationalist is systemism than holism? His sincere disappointment in Popper’s individualism leads Bunge to make two conjectures, political and sociological, about the reasons that prevented Popper from believing in systems.

Why did Popper uncritically [*sic*] adopt the individualist social ontology inherent in traditional liberalism and anarchism? The simplest answer is that he did so in reaction to the holist (or collectivist) conception of society that underlies the thought of Plato, Hegel, Marx, and their heirs. . . . And why did Popper fail to sketch an original or even consistent ontology of the social? I conjecture that the reason is that neither he nor his best interlocutors, the members of the Vienna Circle, were interested in metaphysics (or ontology). (Pp. 108-9)

Bunge extensively attacks in the volume the excesses of the sociology of knowledge and social constructivism theories (see chapters 8 and 9), but resorts here to their pet arguments. As a result, the lost connection is the intellectual reason that made Popper an individualist. (It is lost also when Bunge explains the success of *The Open Society*, which

“proposes a weak social philosophy,” by nothing more than its contents and style, and by opportunity [p. 126]). Still another missing connection is Popper’s signature motif on the interdependence of metaphysics and science.⁴⁷

In the cursory discussion of Popper’s “antinomianism,” ambiguities in Bunge’s story seem to reach their peak. On one hand, Popper “had shown that the thinkers whom he misleadingly called ‘historicist’ had failed to exhibit any historical laws: they had only noted some trends” (p. 112). On the other hand, Bunge contends that “There might be objective social laws, in particular laws of social change” (p. 111) (cf. Popper’s description of “historicism”). In spite of the fact that Popper had logically shown the impossibility of such laws, Bunge thinks that the failures of historicists “do not disprove conclusively the existence of historical laws” (p. 112). As some modern theorists of history do, he finds support for this thesis, once more, in Braudel and his concept of *longue durée* (secular economic cycles), but does not show how these trends can be those promised “objective laws” of social change. Moreover, in another chapter he is full of skepticism in regard to the very existence of “‘long waves’ (or secular cycles). . . . So far, most of the studies of this problem have proved inconclusive” (p. 65).

From what he says, it seems next to impossible to extract what his criticism of Popper consists of and what his own position is. One can find no, or opposite, answers to the following questions: What was wrong with Popper’s “antinomianism”? What were Popper’s views on laws at all? Does or does not Bunge uphold his critique of historicism? What after all counts as an “objective social law”? He adds another concept of “quasi-laws,” and gives seven examples of this variety: “(4) All social innovations are introduced by new social groups (E. H. Carr). . . . (6) The institutions of today do not entirely fit the situation of today (T. Veblen). (7) All progress in some regards involves regress in others” (pp. 112-13). These quasi laws are supposed to buttress the following conclusion:

47. For example, “I do not think it possible to eliminate all ‘metaphysical elements’ from science: they are too closely interwoven with the rest. Nevertheless, I believe that whenever it is possible to find a metaphysical element in science which can be eliminated, the elimination will be all to the good. For the elimination of a non-testable element from science removes a means of avoiding refutations” (Popper 1982, 181).

There are then, antinomianism notwithstanding, some plausible candidates for social patterns, in particular historical laws, that is, laws of social change [*sic*]. These have three main sources: (a) we are all immersed in the same biosphere, which "obeys" laws of nature; (b) all humans are animals with the same basic needs, and are willing to do something to meet them; (c) all normal humans are sociable, whence they tend to build or join social systems. (P. 113)

Having identified these quasi laws as plausible candidates for not less than "laws of social change," and having conditioned them upon our environment, basic needs, and the sociability of "normal" humans, Bunge nevertheless admits that "our knowledge of social regularities is still dismally poor. And it won't be enriched unless the antinomianist bias is superseded by both historiographic research and philosophical analysis" (*ibid.*). One may wonder whether his analysis is not conducive to the antinomianist bias.

The analysis of Popper's "Negative Utilitarianism" takes Bunge less than one page. The reason for this economy is probably due to his generalization that may bewilder a Popper scholar: "Popper's moral philosophy occupies all of one footnote (Popper 1945, chap. 5, fn. 6)" (p. 113). Bunge recalls that his first reading of *The Open Society* was "love at first sight," but after a lapse of time its text seems to have become out of sight and barely kept in mind. One is left to speculate about the relations between moral philosophy and political philosophy, to which *The Open Society* definitely pertains, or to count the number of pages Popper devotes to "purely" moral matters in this book alone. Bunge allots another section to Popper's "Libertarianism" (pp. 109-10)—he finds it of course "one-sided and ineffective"—and one has to guess now why he disconnects "political" and "ethical." The discussion of negative utilitarianism is done in this cursory way too; still Bunge manages to cram into this scanty account ideas that Popper never held: that the principles of negative utilitarianism "invite us to treat only symptoms, refraining from removing the sources of evil" (p. 113), and that one may extract from Popper the advice "Do not concern yourself over much with others" (p. 114). In general, negative utilitarianism is interpreted as "selfishness of the considerate and smart kind." It is not that Popperian political and moral philosophy is free from difficulties (see, e.g., Shearmur 1996); these inferences do not follow even from the single note in Popper to which Bunge refers.

Conversely, in the discussion of "Piecemeal Social Engineering," Bunge invites a rather irrelevant connection. He remarks,

Popper has been accused of inconsistency for being against social revolution but in favor of scientific revolution. His defence was as follows: "If the method of rational critical discussion should establish itself, then this will make the use of violence obsolete." . . . In other words: Let's all become intellectuals. (P. 115)

This made-up "defence" attributed to Popper ignores, Bunge believes, that in all societies only a minority is able to think critically, that power tends to suppress peaceful public discussions, and that this suppression leads to rebellion. Bunge thus makes Popper not a critical rationalist but a myopic idealist who, following Kuhn, tends to "call every scientific breakthrough a revolution" (p. 116). He tells us that Popper "never put forth any constructive proposal" for planned social reform (even though the notion of piecemeal social engineering is a theory of social planning and reform), and that Popper

did not examine in detail any of the social technologies, such as normative macroeconomics, city planning, social medicine, the law, or management science, all of which raise interesting ontological and epistemological problems—such as, for instance, the question of the very nature of plans as different from theories. (P. 117)

One can then simply open an index in a random handbook on social planning and policy and go on computing items Popper failed to touch on in his views on social reform. This is, of course, a convincing way to show that Popper is not an encyclopedist, but nobody has this expectation.⁴⁸

There is almost no information on what Popper thought about social technology and planning. Although he gives a few short quotes, the impression is that Bunge analyzes Popper's texts by memory. This produces overtly false claims: "Faithful to his anti-definitionist stand, Popper never clarified satisfactorily what he meant by 'institution,' 'social technology,' or 'piecemeal social engineering.'" Bunge nevertheless reconstructs these ideas "from the context" (p. 117) and finds quite a few historical examples of the

48. On the same grounds, Newton is not either—he "merely" theorized and did not bother to anticipate all possible applications of his theories. Bunge makes similarly strange reproaches to Popper further on: "What does Popper have to say about overpopulation, environmental degradation, gender and race discrimination, or anomie? Nothing. What about the near-omnipotence of the megacorporations, the North-South inequality," and so on (Bunge 1999, 124). Since analysis of Popper's "Social Order: The Broken Pillar" (pp. 120-25) is reduced to this nothing-valuable-to-say handling, I have refrained from discussing this section: the answers are easily found in Popper.

piecemeal social engineering policy from Disraeli to the United States of the 1960s. Having found Popper's ideas not articulate enough, Bunge concentrates on the deficiencies of the piecemeal method of social reforms. The reforms in the United Kingdom and the United States after the 1960s have not been "wholly successful," and led to economic recessions and the growth of relative economic inequality. Bunge singles out two causes of this failure: "local" and "principled" (p. 118). The local cause is that the two countries' "social expenditures had to compete with the insane arms race, tax cuts for the rich, the support of client governments . . . and the Vietnam war"; at the same time, their European reformist counterparts did not sacrifice "social welfare to the Cold War" (p. 118). In other words, the local cause has nothing to do with the issue of the effectiveness of piecemeal reforms (the relevance of the gradual character of the reforms to their putative failure is not being discussed). On the other hand,

the general or principled reason for the failure of all known social reforms to secure freedom from exploitation—a goal Popper shared with socialists of all colors—is that they have been piecemeal or sectoral rather than global or systemic. Piecemeal social engineering is bound to produce at best only modest results, because society is not merely a collection or "sum" of individuals . . . it is a system. And a system, be it atom, chemical reactor, organism, ecosystem, family, or society at large, cannot be altered successfully bit by bit, for all its components hang together. (P. 118)

Bunge makes a few suspect assumptions in quite a brief statement. First, freedom from exploitation, as any other political goal, is irrelevant to the question of efficiency and choice of reform strategy. Popper's emphasis is on the technological feasibility of planning and reforms—it is a "value-free" approach—and exploitation may be exactly the social engineer's aim.⁴⁹ Second, there exist varying readings of Popper's political stance(s), and Bunge offers at least two. Having attached Popper to "socialists of all colors," he says two pages further that "Popper espoused a definite political philosophy, namely

49. Rationality in social planning (i.e., the use of social technology in reforms) alone does not entail any political commitments: "As another example of a social institution, we may consider a police force. Some historicists may describe it as an instrument for the protection of freedom and security, others as an instrument of class rule and oppression. The social engineer or technologist, however, would perhaps suggest measures that would make it a suitable instrument for the protection of freedom and security, and he might also devise measures by which it could be turned into a powerful weapon of class rule" (Popper [1945] 1996, 23).

advanced liberalism" (p. 120). The reader has to choose between the two labels, but as a precaution it may be said that Popper was choosy enough not to support socialists of "all colors," not even in his "Red Vienna period."⁵⁰ Third, "piecemeal social engineering is bound to produce at best only modest results"—this is exactly the aim of this policy; the modesty of the results, therefore, cannot be a reproach to it but rather be a sign of its technological efficiency. Fourth, the connection between success of reforms and metaphysical systemism/holism has to be shown; while the flaw of this claim is seen from the fifth related statement that society "cannot be altered successfully bit by bit," which is a plain empirical mistake. One can see and hear from the mass media at any moment today, in most parts of the world, that virtually any news program is essentially an instrument of pointing at and fighting against "concrete social evils" and that reformation of the society is an ongoing, bit-by-bit process just on the basis of these deliberate social issues. The belief that "*Only* by adopting a systemic or multisectoral approach to social issues can we hope to solve social issues" (p. 119 [italics added]) is not supported by overwhelming evidence of our practice that suggests instead the opposite conclusion. In addition, this systemist belief of Bunge has purely holistic overtones and reinforces the popular mode of thinking observed by Bryan Magee among students in 1968:

"There is something fundamentally rotten about any society in which *x* happens," with *x* standing for any serious social evil. If anything *at all* was seriously wrong, the whole of society was sick: unless everything's perfect everything's rotten. Such an attitude could rest only on Utopian assumptions. And it quite naturally made those who held it receptive to a holistic as well as systematic social critique of the only society they knew. (Magee 1995, 260; cf. note 37)

Bunge (1999, 119) denies the affinity between systemism and holism: "A systemic view of society (as opposed to both the individualist and the holist views) suggests that one can advance gradually provided one does it in all the pertinent sectors at the same time, since they all hang together." This is not a strong case against piecemeal social engineering; furthermore, it looks like a self-destructive argument. Recall that according to Bunge, society is a system of subsystems, characterized by a certain composition, environment, and structure (p. 23). Given his prerequisites, to assess the pertinence of

50. See Popper's autobiography, and Malachi Hacoen (2000).

particular institution(s) for a successful reform, we have to assess the whole system in a way that would tell us, technologically precisely, how the whole system and its components will behave in the course of the reform. This could work only if we had sufficient, that is, perfect, knowledge about the interaction between the whole and "all the pertinent sectors," not only now but in the future as well. We do not, and cannot, have such knowledge. After having made Popper a straw man, Bunge proposes an alternative that sounds as clear as it is practicable: "The proper device should not be 'Piecemeal social engineering' but 'Systemic social reform guided by sociotechnology and implemented with the active participation of all the stakeholders'" (p. 119). This wording serves in turn as the ground for a rebuke to Popper: he has failed to tell us "what we should do" (p. 120).

The conclusions Bunge makes about Popper's social philosophy are distressing: it "lacks a theory about social order because he has neither an adequate theory of society nor a positive moral philosophy"; his theory of society is "sketchy and inadequate . . . because it refuses to admit the very existence of social wholes" (p. 125); and "Popper's views have not inspired a single piece of social legislation" (p. 126). Here Bunge is close to Plato's demand for rulers to be philosophically nurtured. (He could as well reproach Popper for failing to become prime minister.) If, otherwise, success among lawgivers is to be the yardstick of the validity of philosophical ideas, very few, if any, can be called "philosophers." Reading Bunge produces the impression that he has little sympathy for, and finds little significance in, Popper's social ideas (then why bother?), and at certain points, that he has forgotten many of them. These, however, are only subjective impressions, and the present discussion may face the charge that the author has mutilated the ideas of Bunge just as he, in my view, did with Popper's; it is up to the reader to compare and judge.

I want to stress one thing at the end. Keeping in mind Bunge's two central intentions in the book—to show how philosophy can and should be important for sociology, and to make an intellectual purge in academia—his loose exposition of Popper's ideas can only have side effects for both tasks. If one comes to share the view that Popper is "exaggerating the importance of criticism at the expense of creation and analysis" (p. 127), one will not merely misunderstand Popper but, downplaying instead the role of criticism, will lend confidence to those whom Bunge names charlatans with all their unrestricted creativity, which disgusts him.

REFERENCES

- Aron, Raymond. 1967. *Main currents in sociological thought*. Translated by Richard Howard and Helen Weaver. New York and London: Basic Books.
- Beilharz, Peter. 2001. *Socialism: Modern hopes, postmodern shadows*. In *Handbook of social theory*, edited by George Ritzer and Barry Smart. London: Sage Ltd.
- Boyle, Edward. 1974. Karl Popper's *Open Society*: A personal appreciation. In *The philosophy of Karl Popper*, edited by Paul Arthur Schilpp. 2 vols. La Salle, IL: Open Court.
- Braudel, Fernand. 1977. *Afterthoughts on material civilization and capitalism*. Baltimore and London: Johns Hopkins University Press.
- . [1979] 1992. *Civilization and capitalism*. Vol. 3 of *The perspective of the world*, translated by Sian Reynolds. Berkeley and Los Angeles: University of California Press.
- Bunge, Mario, ed. 1964. *The critical approach to science and philosophy*. In honor of Karl R. Popper. London: Free Press; Collier-MacMillan Ltd.
- . 1996. *Finding philosophy in social science*. New Haven, CT, and London: Yale University Press.
- . 1998. *Social science under debate: A philosophical perspective*. Toronto and Buffalo and London: Toronto University Press.
- . 1999. *The sociology-philosophy connection*. New Brunswick, NJ, and London: Transaction Publishing.
- . 2001. Systems and emergence, rationality and imprecision, free-wheeling and evidence, science and ideology: Social science and its philosophy according to van den Berg. *Philosophy of the Social Sciences* 31 (3): 404-23.
- . 2004. How does it work? The search for explanatory mechanisms. *Philosophy of the Social Sciences* 34 (2): 182-210.
- Caldwell, Bruce J. 1991. Clarifying Popper. *Journal of Economic Literature* 29 (1): 1-33.
- . 1998. Situational analysis. In *The handbook of economic methodology*, edited by John B. Davis, D. Wade Hands, and Uskali Mäki, 462-68. Cheltenham and Northampton, MA: E. Elgar.
- Coleman, James. 1995. Comment on Kuran and Collins. *American Journal of Sociology* 100 (6): 1616-19.
- Collins, Randall. 1978. Some principles of long-term social change: The territorial power of states. *Research in Social Movements, Conflicts, and Change* 1:1-34.
- . 1981. On the microfoundations of macrosociology. *American Journal of Sociology* 86 (5): 984-1014.
- . 1986. *Weberian sociological theory*. Cambridge, UK: Cambridge University Press.
- . 1989. Sociology: Proscience or antiscience? *American Sociological Review* 54:124-39.
- . 1995. Prediction in macrosociology: The case of the Soviet collapse. *American Journal of Sociology* 100 (6): 1552-93.
- . 1999. *Macrohistory: Essays in the sociology of the long run*. Stanford, CA: Stanford University Press.
- De Marchi, Neil, ed. 1988. *The Popperian legacy in economics: Paper presented at a symposium in Amsterdam, December 1985*. Cambridge, UK: Cambridge University Press.
- , ed. 1992. *Post-Popperian methodology of economics: Recovering practice*. Boston: Kluwer.
- Foucault, Michel. [1971] 1977. Revolutionary action. In *Language, counter-memory, practice*. Cornell, NY: Cornell University Press. (Original work published as an interview in *Actuel* 14:42-47)

- Garejew, Machmut. 2004. Was lehrt uns Irak? Politische Fehlentscheidungen und militärische Mängel—eine russische Sicht. *Berliner Zeitung*, March 25. <http://www.berlinonline.de/berlinerzeitung/archiv/.bin/dump.fcgi/2004/0325/politik/0026/>.
- Gay, Peter. 2000. Do your thing. In *Historians and social values*, edited by Joep Leerssen and Ann Rigney, 33-44. Amsterdam: Amsterdam University Press.
- Gerth, H. H., and C. W. Mills, eds. [1946] 1958. *From Max Weber: Essays in sociology*. Translated and edited, and with an introduction, by H. Gerth and C. Mills. New York: Oxford University Press.
- Giddens, Anthony. [1971] 1994. *Capitalism and modern social theory: An analysis of the writings of Marx, Durkheim and Max Weber*. Cambridge, UK: Cambridge University Press.
- Ginzburg, Carlo. [1976] 1992. *The cheese and the worms: The cosmos of a sixteenth-century miller*. Translated by John Tedeschi and Anne Tedeschi. Baltimore: Johns Hopkins University Press.
- Hacohen, Malachi H. 2000. *Karl Popper—The formative years, 1902-1945. Politics and philosophy in interwar Vienna*. Cambridge, UK: Cambridge University Press.
- Hands, D. Wade. 1992. Falsification, situational analysis and scientific research programs: The Popperian tradition in economic methodology. In *Post-Popperian methodology of economics: Recovering practice*, edited by Neil De Marchi, 19-53. Boston: Kluwer.
- . 1993. Popper and Lakatos on economic methodology. In *Rationality, institutions, and economic methodology*, edited by Uskali Mäki, Bo Gustafsson, and Christian Knudsen, 61-75. London: Routledge.
- Haskell, Thomas L. 1998. *Objectivity is not neutrality: Explanatory schemes in history*. Baltimore: Johns Hopkins University Press.
- Hechter, Michael. 1995. Symposium on prediction in the social science. Introduction: Reflections on historical prophecy in the social sciences. *American Journal of Sociology* 100 (6): 1520-27.
- Hedström, Peter, Richard Swedberg, and Lars Udéhn. 1998. Popper's situational analysis and contemporary sociology. *Philosophy of the Social Sciences* 28 (3): 339-64.
- Hempel, Carl. 1965. *Aspects of scientific explanation*. New York: Free Press.
- James, Roger. [1980] 1998. *Return to reason: Popper's thought in public life*. Chippenham, Wiltshire: Open Books.
- Langlois, Richard N. 1986. Rationality, institutions, and explanation. In *Economics as a process: Essays in the new institutional economics*, edited by Richard N. Langlois, 225-57. Cambridge, UK: Cambridge University Press.
- Magee, Bryan. 1973. *Karl Popper*. New York: Viking Press.
- . 1995. What use is Popper to a politician. In *Karl Popper: Philosophy and problems*, edited by Anthony O'Hear. Cambridge, UK: Cambridge University Press.
- Mayntz, Renate. 2004. Mechanisms in the analysis of social macro-phenomena. *Philosophy of the Social Sciences* 34 (2): 237-59.
- Miller, David. 2000. Sokal & Bricmont: Back to the frying pan. *PLI* 9:156-73.
- Nagel, Thomas. 1998. The sleep of reason. *The New Republic*, October 12, 32-38.
- Popper, Karl R. [1934] 1999. *The logic of scientific discovery*. London: Routledge.
- . [1944-45] 1997. *The poverty of historicism*. London: Routledge.
- . [1945] 1996. *The open society and its enemies*. London: Routledge.

- . [1963] 1996. Models, instruments, and truth. The status of the rationality principle in the social sciences. In *The myth of the framework*, edited by Mark Notturmo. London: Routledge.
- . 1982. *Realism and the aim of science*. Vol. 1 of *Postscript to the logic of scientific discovery*. London: Hutchinson.
- . 1985. The rationality principle. In *Popper selections*, edited by David Miller, 357-65. Princeton, NJ: Princeton University Press. (First published in French in E. M. Claassen, ed. 1967. *Les Fondements Philosophiques des Systèmes Économiques*. Paris: Payot.)
- Portes, Alejandro. 1995. On grand surprises and modest certainties: Comment on Kuran, Collins, and Tilly. *American Journal of Sociology* 100 (6): 1620-26.
- Quine, Willard V. O. 1981. Mencken's *American Language*. In *Theories and things*, 203-8. Cambridge, MA: Harvard University Press.
- Redman, Deborah A. 1991. *Economics and the philosophy of science*. New York: Oxford University Press.
- Ritzer, George. [1983] 1992a. *Contemporary sociological theory*. New York: McGraw-Hill.
- . [1983] 1992b. *Sociological theory*. New York: McGraw-Hill.
- . [1983] 1996. *Modern sociological theory*. New York: McGraw-Hill.
- . 1997. *Postmodern social theory*. New York: McGraw-Hill.
- Ritzer, George, and Barry Smart, eds. 2001. *Handbook of social theory*. London: Sage Ltd.
- Samuelson, Paul A., and Anthony Scott. 1975. *Economics*. Toronto: McGraw-Hill Ryerson. (Adapted from Samuelson 1955-73 editions)
- Shearmur, Jeremy. 1996. *The political thought of Karl Popper*. London: Routledge.
- Smart, Barry. 1976. *Sociology, phenomenology and Marxian analysis: A critical discussion of the theory and practice of a science of society*. London: Routledge Kegan Paul.
- . 1983. *Foucault, Marxism and critique*. London: Routledge Kegan Paul.
- . 1985. *Michel Foucault*. Chichester, UK: Ellis Horwood.
- . 1992. *Postmodernity*. London: Routledge.
- Smart, Barry, and Carol Smart. 1978. *Women, sexuality, and social control*. London: Routledge Kegan Paul.
- Sokal, Alan D. 1996. Transgressing the boundaries: Towards a transformative hermeneutics of quantum gravity. *Social Text* 46/47:217-52.
- Tilly, Charles. 1995. To explain political processes. *American Journal of Sociology* 100 (6): 1594-610.
- . 2004. Social boundary mechanisms. *Philosophy of the Social Sciences* 34 (2): 211-36.
- van den Berg, Alex. 2001. The social sciences according to Bunge. *Philosophy of the Social Sciences* 31 (1): 83-103.
- Wight, Colin. 1998. Philosophical geographies: Navigating philosophy in social science. *Philosophy of the Social Sciences* 28 (4): 552-66.
- . 2004. Theorizing the mechanism of conceptual and semiotic space. *Philosophy of the Social Sciences* 34 (2): 283-99.

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