

BOOK REVIEWS

The Sociology of Science in Europe, edited by Robert K. Merton and Jerry Gaston (Carbondale: Southern Illinois University Press, 1977).

This book covers more than the title indicates, since Robert Merton has used nearly a third of the space for "An Episodic Memoir" of his own impressions, contacts, and activities as the leading sociologist in our time. He has vividly described the origins, the antecedents, the invisible college phase, and the slow institutionalization of this new specialty. His episodes are dramatically arranged around certain pioneers: Gilfillan, Ogburn, Price, Candolle, Galton, Garfield, Sarton, Popper, and Kuhn. Personally, I like best the work on scientists' biographies, initiated by Candolle and Galton, which involved eventually the analysis of entries in, for example, the British *Dictionary of National Biography* and the *Dictionary of American Biography*. Merton tends to say nothing of his own role, as he prefers to act as a participant observer of the specialty's development. He thereby frees himself to present his report as insider's information. He does, however (quite properly), present his own well-known conceptual framework which examines the normative and interaction-pattern features of the social and cultural structures of science.

Merton's memoir is a fine piece of work, and is very necessary in this context because the sociology of science has too often remained confined within national and/or disciplinary boundaries. Thus, his review of the sociology of science in Europe cannot omit a presentation of the international picture and those American scholars who have meant the most to the Europeans in the field. The balance of the book considers the sociology of science in Europe through seven essays on West Germany, Austria, Poland, Britain, France, Italy, the USSR, and Scandinavia. Rolf Klima and Ludger Viehoff, in their discussion of West Germany and Austria, give special attention to recent work such as Plessner's empirical studies of the German university system and the more speculative, historical tradition (Ben-David, for example) which examines the system of science with reference to German society. The discussion of the sociology of science in Poland (by Krauze, Kowalewski and Podgórecki) indicates that in this country the specialty is not yet well-institutionalized. There seems to be more orientation to science as such than to sociology.

M. J. Mulkey's discussion of Britain's sociology of science centers on two broad topics: university research and work in industry and government. There are several useful reports here, such as Burns and Stalker's findings that further document the isolation and underutilization of scientists in business and industry, and Cotgrove and Box's discussion of the differential commitments of scientists which prompt those more committed to salary, promotional, and other material rewards to seek careers in industry. Paul Frank (a pseudonym)

has examined four aspects of the French situation: the development of the research tradition in sociology, the relation between the development of disciplines and their social organization (or their cognitive structures), the organization of work laboratories, and the productivity of researchers. In France, organized social science research is conducted largely in research institutes outside the universities. Reliance on independent contractual support for research tends to create a pattern of small research units investigating narrowly pragmatic topics. Findings are commonly published in technical reports, not in scientific journals, with the result that researchers are deprived of visible prestige documentation necessary for promotion and recognition. Worse still, their research is defined by persons unfamiliar with the fundamentals of the social sciences.

Filippo Barbano's account of Italian work stresses the socialization, specialization, and activity structure features of science. He energetically discusses the dialectical structure of science's historical role in crystal-clear terms which were, to me, incomprehensible. The chapter by Gennady Dobrov on the sociology of science in the USSR focuses on science policy. He reminds us that as early as the first year of the Soviet state, Lenin organized and promoted the development of science, creating State agencies for scientific management for the purpose of its application to social needs (such as electrification). The basic strategic doctrine was the transformation of research into a productive social force. Dobrov presents empirical support for his discussions of the development of scientific establishments and fields, the structure of controls, goals and means. One troubling feature of this article is that, in contrast to the others, it lacks a complete bibliography and, surprisingly, refers only to publications in English (even in the case of texts in Russian journals).

Jamison's discussion of Scandinavia pays special attention to the younger sociologists of science who are pictured as more or less totally integrated into the welfare system, practical minded, and interested in applications. He points out that the few remaining "basic" scientists seem to be more interested in establishing contacts with colleagues abroad than in developing a sense of community in their own countries. This does seem to be a trend throughout Europe where the large number of applied scientists, in contrast to their basic research colleagues, address themselves mostly to the public and their local scientific communities.

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For Science in the Social Sciences by David Papineau (New York: St. Martin's Press, 1979).

Most scientists would no doubt agree that methodology is what sets science apart from other modes of creative activity. Fewer would endorse the proposition that "the scientific method" is not absolute, but a shifting community standard, and only a cynical handful of practicing scientists would agree with Feyerabend that method is no standard at all. Since Kuhn, however, the role of methodology in scientific research has become a legitimate topic of debate,

first among philosophers and historians of science, and now among all who profess “objective knowledge” as their productive goal. That methodology is a *range* of choices instead of a fixed procedure, subject to national, epochal, and social influences, is hardly controversial any more. What remains arguable, indeed controversial, is precisely how the scientific method is interposed between “an articulated reality ‘out there’” (46) and a thinking, manipulating scientist “in here”. For if method safeguards objectivity, then the means justifies the ends. Whatever we do to “measure,” “retrieve,” or “capture” reality determines our representations, that is, our claims to knowledge of it.

This realistic-relativistic view of knowledge Papineau would not dispute. Yet it is a paradox to admit simultaneously a single external reality and multiple or alternative research programs for its apprehension. To adopt exclusively a realist or relativist stance, as shown recently by Elkana (*Social Studies of Science*, 1978, 309–326), is an unnecessary, if not futile, epistemological choice. Seldom do natural scientists explicitly make such a choice; rather, they move silently back and forth between each in the process of doing research. Some social scientists consider this a pivotal process for developing a “sociology of scientific knowledge,” a program that demystifies the work of mathematicians, physicists, and biologists. In so doing, this program approaches science as a problem in “negotiated order”. How do scientists themselves interpret and socially construct reality? What, for example, constitute criteria of proof for a particular research community at a particular time? How do these criteria change, affect the evaluation of knowledge claims, and crystallize the community’s consensus, however fragile, to those claims? Without posing these questions, science seems aloof and impenetrable, an utterly rational system for the production of “privileged knowledge.”

For Science in the Social Sciences is clearly in this genre, although it is largely uninformed by its copious literature. This is disturbing and, at the same time, predictable. Philosophers of science rarely take their cues from sociologists (of whom many are by training, if not by self-identification). Papineau, however, is no stranger to sociology. Perhaps his identification with both disciplines engendered his interest in making the epistemological transition from methodology/objectivity in the natural sciences to methodology/objectivity in the social sciences. His interest, while noble *and* welcome, fails upon execution to convince. Underlying the failure of *For Science* is what one might call a Kuhnian fallacy — an adaptation of the children’s fable “The Little Engine That Could.” The Kuhnian version — the pejorative eponymy may be undeserved, though it acknowledges a debt to the heuristic value of *The Structure of Scientific Revolutions* and the corpus it begat — is that the “little” and “immature” sciences, the social sciences, will experience paradigmatic maturation if they continue to emulate adherence to the canons of logic and scientific method (as exemplified by physics) and objectify their knowledge of what Papineau calls “deterministic causal relationships” within social reality (52, 162). The fallacy, of course, exists in the distinctive subject matter of the social sciences. One can mathematize, construct deductive propositional theories, and explain substantial variance in the social relations of groups, organizations, and less intuitive “structures” or “systems,” and still

lack an understanding and a sense for the predictability of social behavior. Indeed, the myth of positivism is “to measure is to know” (but see 2, 20); if anything, the social sciences of late, particularly the disciplines of sociology and political science, have methodologized their subject matter without (much?) enhancing their explanatory or predictive powers. One social analyst (Ritzer, *Sociology: A Multiple Paradigm Science*, 1975) recognizing this realist-relativist “bind,” has, in the name of Kuhn, consigned sociology to the status of a “multiple paradigm science”. The pessimists among us might interpret this as resignation to an unending dissensus about what the subject matter of sociology should be, much less *is*. As for the optimists . . .

Re-enter For Science. In seven chapters and slightly less than 200 pages, we are reintroduced to “social facts,” “causes and statistics,” and “actions, rules and meanings”. The heart of Papineau’s thesis — “a defense of the view that the social sciences can and ought to conform to the standards set by the natural sciences” (1) — is contained in chapters 2, 6, and 7. Here, the respective discussions of “scientific theories,” “alien belief systems,” and “facts, values and ideology” are provocative and even compelling in spots. The review of Feyerabend, Kuhn, and Lakatos (33–43), while grounded in a Popperian rational man characterization of the scientist that is becoming increasingly difficult to defend, is a concise analysis of the tension between theory-dependent observation and the prescriptive need for generalization and falsifiability. In a way, Papineau is engulfed by this tension, for he vacillates throughout the book between the classic “contextual” dichotomy of discovery and justification. To wit:

Philosophers in the Popperian tradition . . . see themselves as concerned solely with matters of justification, with questions of how scientific theories are to be assessed once they are actually put forward. Where the theories come from in the first place, how they are discovered, is presumed not to be a matter for philosophical evaluation but, if anything, something for social, psychological or psychoanalytic explanation. The idea is that, provided a theory is properly assessed after it is proposed, it is no discredit to it, that it might originally have occurred to its proponent because he found it ideologically attractive, or because it came to him in a dream (43).

Like other intellectual divisions of labor decried as disciplinary ethnocentrism or parochialism (King, *History and Theory*, 1971, 3–32), the separation of discovery from justification as topics for social analysis is anathema to me. Because science is a symbolic social activity, the origins of ideas — as well as their negotiation, confirmation, rejection, and/or obsolescence — must be appraised *together*. Papineau eventually concedes this point, too: “There is little doubt that in actual practice theoretical frameworks are not always given up when scientific standards finally call for them to be abandoned. Their attractions to powerful social groups will often encourage their persistence well past this point. But, again, to say that existing practice falls short of what the canons of scientific methodology require is not to say that ideological influence *must* obtrude on scientific requirements” (175). Finally, seemingly contradicting his thesis, Papineau admits that in the social sciences “ideological pressures will almost invariably be present. By the nature of their subject matter the claims made by theories in the social sciences cannot help but be about

the empirical connections between various states of affairs that different groups will be interested in preserving or preventing” (176). If we consider these juxtaposed statements in further detail, we are assured that “science is still essentially a *critical* activity. Scientists recognize an obligation to account for anomalies in non-*ad hoc* ways, by producing explanations which are themselves further testable” (155).

In general actually *adopting* (as opposed to merely professing) beliefs which lack evidence can only be self-defeating. For one’s actions best lead to the satisfaction of one’s ends if they are based on accurate beliefs about what is in fact conducive to those ends. . . . A belief, even if adopted for ideological reasons, is a claim as to the facts, and as such needs to be assessed for correctness against the empirical evidence (172–173).

Ultimately, what is missing from Papineau’s anthropological account of science is a recourse to alternative *realities*. Having persuaded the reader that “alternative rationalities” and “different belief systems” are marks of conceptual, linguistic, and cultural distinction, the next step toward a thoroughly relativistic recognition of alternative (interpretations of) realities would be expected. Instead, Papineau retreats:

. . . in the end there are real obstacles to an *entirely* value-free social science. But it should not be forgotten that, as before, the issue is essentially an *empirical* one: we are asking what conditions, if any, will as a matter of practical fact prevent ideological influences illegitimately distorting *accepted* views of social reality. And so, even a pessimistic answer does not affect the normative point that it would still be better for people to accept those views that most accurately represent reality rather than those views which unnecessarily distort it. In particular; anybody who is serious about disputing the accepted *orthodoxy* on some matter will still have an obligation to vindicate his scepticism by producing an alternative which can in time actually be shown to be scientifically superior (177, italics added).

It is indeed fitting that the final section of *For Science* (which immediately follows this quotation) is entitled “Science as Ideology”. (It comes, for me, at least one paragraph too late.) The italicized words above are meant to indicate the value-laden character of Papineau’s own views. Like pregnancy, value-freedom in social science is not a fractional state; *social science is not value free*. Similarly, the “issue” is epistemological not “empirical”; the “normative point” – so familiar to sociologists of science like myself – is that what is “better” is sufficiently unorthodox so that fulfilling the “obligation to vindicate” skepticism is tantamount to heresy. In short, the reigning ideology, clothed in theoretical and methodological prescriptions, determines what is “scientifically superior”. Only a sociology of science that *rejoins* a realistic, skeptical sociology of knowledge will reveal the alternative realities distorted and obscured by specific scientific theories and methods. This is *my* epistemological fable. It underscores what is missing from Papineau’s account: a comprehension of what “Critical Theory” (181) is and can do. “Technical interests” and humanity (including human subjectivity) need not be incongruent; there is an intellectual and thereby human failing – no worse than Papineau’s in *For Science*. As you read the book, consider it a reminder that

we all could do better and that – relativistically and realistically – some of us scientists are trying.

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The Revisionists Revised: A Critique of the Radical Attack on the Schools by Diane Ravitch (New York: Basic Books, 1977).

In *The Revisionists Revised*, Diane Ravitch attacks and attempts to discredit the work of several scholars who have questioned the efficacy and potential of schooling to advance egalitarian and progressive ends within the American capitalist order. Ravitch contends that “radical” or “revisionist” critics, including Michael Katz, Samuel Bowles, Herbert Gintis, Clarence Karier, Joel Spring, and Walter Feinberg, advance alien and dangerous political values, apply crude and simplistic analyses, and selectively choose from and distort otherwise scanty evidence. *The Revisionists Revised* should be soundly rejected on some of these very same grounds.

Ravitch presents a simplistic, schoolbook version of American history and social change; she misreports and misunderstands much from the works she reviews; she selectively uses social science evidence drawn uncritically from secondary sources; and she betrays an appalling lack of academic ethics. Through literary device, caricature of competing ideologies, and outright distortion, Ravitch attempts to place radical scholarship outside the boundaries of acceptable discourse and to taint radical scholars with undemocratic, even totalitarian sympathies. *The Revisionists Revised* is not primarily an exercise in serious scholarship and argument. It is ultimately addressed to beleaguered government officials, educational professionals, and citizens whose frustrations and low morale Ravitch is attempting to alleviate. In pursuit of this end, Ravitch adopts tactics that should be repugnant to those who hold to the liberal values she pretends to defend.

Radical interpretation, in Ravitch’s view, is an assault on institutions and values which she prizes. She subtitles her book, “A Critique of the Radical Attack on the Schools.” Chapter one is called “The Democratic-Liberal Tradition Under Attack.” In this chapter, Ravitch contrasts a presumably open and responsible politics characteristic of the United States with the supposed radical alternative. Radicals, she says, demonstrate an “exclusive preoccupation with ends as contrasted with means and ends in relation.” In chapter three, recent radical scholarship is introduced with an attack on 1960s New Left activism, characterized as indifferent to means and approving of violence. Quite apart from their validity as descriptions of radical politics, Ravitch’s characterizations are irrelevant to the validity of critical interpretations of the American past, and are unfaithful to the work and personal commitments of the authors under review. Conceding that none of the books she examines “can be said to be a systematic application of New Left thinking,” the context and manner in which Ravitch situates her subjects make unmistakable the intended connection between radical analysis and undemocratic behavior.

Ravitch's attempt to taint radical scholars with undemocratic affections is clearest in her treatment of Samuel Bowles and Herbert Gintis. Ravitch writes that the authors of *Schooling in Capitalist America* "call their revolution 'socialist' and 'democratic', but their referents are never socialist democracies like Sweden or Israel. Instead, they hail the revolutionary 'socialism' of such nations as the Soviet Union, North Vietnam, Cuba, and China. . . . [A]nyone acquainted with the societies lauded by the authors must feel some skepticism about the claims of greater personal freedom in Marxist 'democracies' [O]ne might wish that the authors showed some slight appreciation of the democratic-liberal values that preserve their freedom to publish a call to revolution against democratic liberalism." Ravitch is correct that Bowles and Gintis do not advance Israel and Sweden as exemplars. They also believe that progressive forces have been unleashed by violent revolutions, including those in France, Russia, China, Cuba, and the United States. But Bowles and Gintis plainly and forcefully reject nondemocratic societies as objects of emulation, a fact which Ravitch omits to report.

Not only does Ravitch distort the political values of her subjects, but also their substantive analyses. She alleges that a fundamental contention common to the authors she examines is that the form and outcomes of American education are to be attributed to the unworthy intentions and motives of individual educational reformers and members of economic élites. "[T]he radical historians," Ravitch writes, "assert that the liberal's promotion of schooling was *intended* to divert attention from more salient issues" (original emphasis). Revisionist scholars share a "belief that schools were *consciously* designed by liberal reformers as undemocratic instruments of manipulation and control," and they "share the opinion that American schools have been an *intentional, purposeful* failure" (original emphasis). These assertions are largely untrue. Following Ravitch's footnotes and examining at length several of the books she reviews, one finds little evidence that malevolent or deceitful intention on the part of reformers or élites plays an important explanatory role in the revisionists' accounts. Instead, one generally finds either explicit disclaimers to the contrary, or arguments which impute underlying systemic purpose to recurring and enduring patterns of outcomes, but which eschew placing blame on individuals.

Ravitch's mistaken emphasis on the question of intention and motivation derives from her adherence to a superficial and trivial conception of social class analysis. For Ravitch, class analysis reduces to correlating individual actions with the clear-sighted pursuit of economic self-interest on the part of self-conscious members of a rigid stratification order. Because she believes that social mobility is the norm, that position in the United States is defined by multiple and cross-cutting dimensions, that Americans almost universally regard themselves as middle-class, that overt class consciousness and antagonism are minimal, and that economic self-interest is not readily apparent, Ravitch concludes that class analysis is of little value in understanding American education.

Class analysis of the sort Ravitch defines is, indeed, of limited value. It is also

not the form of analysis usually applied in the studies under review. For example, Michael Katz, in *The Irony of Early School Reform*, stresses the ambivalent and uncertain perceptions and responses of social élites to ante-bellum industrialization, and highlights the non-economic sources of tension underlying the protests of striking shoemakers. In *Class, Bureaucracy, and Schools*, Katz rejects the argument that business élites were directly responsible for transforming early twentieth century urban schools, and argues that while “[t]he politics of organization-building was the politics of value clash, . . . the nature of that clash is not described by conventional categories of economic or class division” (original emphasis). Even Colin Greer, whose book *The Great School Legend* is the weakest of the works Ravitch treats, rejects the direct pursuit of profit as the force behind educational change, and Clarence Karier takes pains to note of the contributors to *Roots of Crisis*, “none . . . accept an economic deterministic position.”

In Ravitch’s perspective, domination arises solely from the efforts of one group to knowingly subjugate and exploit another, never out of well-intentioned responses to dislocation and change, nor out of unquestioned social arrangements. Class struggle does not exist unless it is recognized and named as such. The apparently universal faith in schooling precludes the possibility that schooling is an instrument of class imposition. That individuals seek more schooling because systemic constraints restrict alternative avenues to economic security is not a possibility Ravitch recognizes. The assumptions Ravitch associates with class analysis deserve comment, for, even if true, they do not warrant her dismissal of class as a central concern. The overwhelming majority of Americans who work do so as employees. This means that most bosses themselves have a boss, that many workers exercise at least some supervisory authority, and, in large firms, all are governed by “company policy”. Consequently, few Americans have direct contact with capitalists, and the immediate interests of workers at differing levels often conflict. The invisibility of the capitalist class and the internal stratification of workers may help to explain the relative absence of explicitly class-based party politics in the United States. If this is so, one should want to inquire into the role of schooling in contributing to the invisibility of the capitalist class and, as Bowles and Gintis do, into the role of schooling in preparing students for jobs and careers in a highly stratified and fragmented work structure.

That most Americans regard themselves as middle-class should be a source of some puzzlement. According to sociologists Richard Coleman, Lee Rainwater, and Kent McClelland, in their recent book *Social Standing in America*, Americans tend strongly to equate social class with a continuum of social statuses defined principally by income levels and their associated standards of living. (This is not the definition of class marxists employ. The reasons that, in Coleman et al.’s words, most people’s “interest is mainly in who gets what and only derivatively in *how* they get it” require investigation.) Even if we agree with Ravitch that there has been some movement toward more equal family incomes in the post-World War II period, that movement most certainly has not been great. Ravitch exaggerates the degree of relevant change by adopting the questionable results advanced by the economist Morton Paglin,

and by adopting official government definitions of poverty, which ignore the effects of income gains among the nonpoor in perpetuating the relative disadvantage of low income recipients. "Relative poverty" shows no decline during the War on Poverty period. Despite large, continuing inequalities in income, Coleman et al. report that Americans imagine the middle classes to be increasing in size and drawing closer to those at the upper levels. The frequency of middle-class self-identification Ravitch notes is consistent with this finding, and like it, requires investigation and explanation.

Ravitch assumes that the validity of social class analyses is vitiated in the face of widespread upward changes in occupations across generations. As Ravitch notes, such changes may be the result of the disappearance of "bad" jobs and the creation of more "good" jobs. She does not note that this kind of mobility can occur *even when* social origins strongly influence occupational destinations and when changes bring no relative gains. Indeed, Mary Corcoran and Christopher Jencks, two of the co-authors of a new study of the determinants of economic success, *Who Gets Ahead?*, estimate that the correlation between a man's occupational status and combined measurable aspects of his background (e.g., father's occupation, parental education, race) in the United States today may be as high as 0.64. If Ravitch is remiss in neglecting empirical inequalities in life-chances, an even more serious omission in her work is the neglect of the conceptual ties between rates of social mobility and the nature of social class relations. In this, she is joined by those she criticizes. While the study of the dimensions and mechanisms of social mobility is relevant to understanding the replenishment and reproduction of existing hierarchies, its relevance to understanding the causes of changes in the forms of hierarchies, in social relations of production, and in the composition and strength of antagonistic classes is uncertain and largely unexplored.

Schooling plays a major role in beliefs about social mobility. Not surprisingly, Ravitch devotes considerable effort toward portraying education as an effective and accessible means for undoing the disadvantages of lowly origins. The picture she presents is exaggerated and biased. For example, she cites William Sewell's follow-up studies of Wisconsin high school students as showing that socioeconomic factors explain only a small proportion of individual differences in educational attainment. Ravitch fails to note that the Wisconsin sample began with only high school seniors. Because early school leaving is related to background factors, these data inevitably underestimate their effects. Recent nationally representative data, on which Ravitch draws for other purposes, show that among males, socioeconomic background is at least twice as important in explaining educational differences as the Wisconsin data would imply.

Ravitch emphasizes the importance of schooling for promoting economic equality between races. She notes that a college education returns higher benefits to young blacks than to young whites, and that most of the occupational gains made by blacks between 1962 and 1973 occurred because of increased educational attainment. From facts such as these, Ravitch confidently concludes that "a democratic society can bring about effective social change, if there are both the leadership and the political commitment to do so." Less

optimistic interpretations are possible. The reason that college is more valuable to blacks than to whites is that whites who have only a high school education are far more likely than similar blacks to hold decent, well-paying jobs. The severe disadvantage of the black high school graduate, not the special advantage of the black collegian, explains the value of a college education to blacks. Moreover, reflecting on similar findings, David Featherman and Robert Hauser, in *Opportunity and Change*, warn that "if younger blacks follow the pattern of their predecessors, the apparent gains on their white counterparts may prove temporary." The fact that the majority of occupational gains made by blacks over a decade's time occurred through lengthier schooling suggests the importance of continuing racial disparities among men with the same amount of schooling. This is especially important when earnings are considered. Joseph Schwartz and Jill Williams, also co-authors of *Who Gets Ahead?*, have shown that giving nonwhites the same amount of schooling as whites could be expected to reduce the racial gap in earnings by at most 30 percent. Simply paying whites and nonwhites with the same schooling equal amounts might reduce the earnings by as much as 70 percent. Contrary to Ravitch's claims, education is a rather weak antidote to racism in the labor market. Her treatment of income inequality, social mobility, and the determinants and consequences of schooling reveal Ravitch to be an amateur in the social sciences. She is captive of the researchers upon whose work she draws, and is unable to discern complexities or to introduce methodological and interpretive qualifications of her own. In its weaknesses, Ravitch's treatment of these issues is of a piece with the remainder of her book.

To reject Ravitch's work is not to uncritically endorse the works she reviews. Critics from within the revisionist camp itself, such as Carl Kaestle and Marvin Lazerson, raised serious questions about some of their colleagues' work long before Ravitch took up her task. (Curiously, on the strange grounds that "[w]hile the work of these historians includes some of the radical themes, each has criticized central elements of the radical analysis," Ravitch exempts Kaestle and Lazerson, as well as David Tyack, from her own consideration. It would appear that Ravitch refuses to risk lending the revisionist school any legitimacy by including within it scholars whom she deems respectable.) Certainly, the effort to develop a critical history and sociology of education would benefit from the more explicit and systematic application of well-established theoretical frameworks. It would benefit as well from a less overt emphasis on de-mythologizing popular beliefs about schooling. This orientation leads to exaggeration and to quarrels over nonessentials. It should not be necessary, for example, to refute the liberal's model of a "meritocracy" in each of its empirical particulars in order to validate a marxist perspective. This is part of the effort Bowles and Gintis undertake, and their results are not persuasive. Finally, the revisionist perspective would benefit from heightened sensitivity to the distinction between functions and causes. Too often, the revisionists appear to forget Durkheim's observation that "[t]he need we have of things cannot give them existence, nor can it confer their specific nature upon them." The real shortcomings in the revisionists' work cannot, however, justify the slander, polemics, and thin analysis offered in *The Revisionists Revised*.

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The Political Economy of Human Rights by Noam Chomsky and Edward S. Herman, 2 vols. (Boston: South End Press, 1979).

Dependent Development by Peter Evans (Princeton: Princeton University Press, 1979).

Whither Brazil, and whither its analysis of Brazil? Routinely mentioned now in conventional media and social science as the most advanced and promising of the so-called emerging nations, Brazil's trajectory in the last fifteen years has been held up as an example of what capitalism can do, for better and for worse. Some observers focus on the astonishing economic growth rate, some on the deepening of the accumulation process, some on the state's role in that process, some on the new expansion into the Amazon, some on the continuing wretchedness of the Northeast. Some focus on the widening gap between rich and poor, some on military and police repression and the reemergence of civilian politics, some on the expanded Brazilian role in world politics and trade. No work I know of has captured all of these aspects in what seem the right proportions, and I am not about to attempt such a synthesis here. Rather, I consider two new (and contrasting) treatments of the Brazilian situation, focused on different aspects, but each in its own way calling into question the practice of conceiving of Brazil as a model for other Third World countries. The first volume of Chomsky and Herman's *The Political Economy of Human Rights*, entitled *The Washington Connection and Third World Fascism*, is more valuable politically than as social science, and its portrayal of Brazil as an example of "Third World fascism" or "client fascism" is one reason for that opinion. Evans's *Dependent Development*, subtitled *The Alliance of Multinational, State, and Local Capital in Brazil*, works out some of the institutional economic arrangements that distinguish Brazil and a handful of other semi-peripheral countries from the relatively weaker and poorer periphery. Evans is thus much clearer both about the limitations of his theoretical model in accounting for Brazil's dependent development and about the limitations of Brazilian actuality as a model for other countries.

Before discussing these works in detail, two general issues require comment: (1) the two usages of the term "model"; and (2) the dual purpose of social science. As suggested above, the term "model" is used in two quite distinct ways in macrosociology. On the one hand, "model" refers to concrete examples in the phenomenal world, examples which might be followed by others: the Prussian road, the Soviet model, the Chinese model, and so forth. On the other hand, "model" refers to abstract depictions (usually partial) of that phenomenal world. Insofar as the Brazilian experience since 1964 has been an empirical model, it has been a failure. Over the past seven years, the Pinochet regime in Chile has attempted to recreate the so-called Brazilian miracle by combining terrifying repression and open invitations to multinational capital. The result has been far short of miraculous, even from the regime's standpoint: industrial stagnation, dynamism only in the primary export sector, exodus of skilled and professional workers, low levels of foreign investment. The story in Uruguay since 1971 is much the same. A model that cannot be copied is not a model, but a phantasm. As for abstract depictions of Brazilian development, quite a number have gained currency in the last decade, all of them capturing

aspects of that development. O'Donnell's "bureaucratic authoritarianism," Schmitter's "corporatism," Fernandes's "bourgeois autocracy," Cardoso and Faletto's "associated dependent development," and Marini's "sub-imperialism" all contribute to a sophisticated picture. But whereas Chomsky and Herman add only "the Washington connection" to O'Donnell by their focus on repression, Evans enriches the Cardoso/Faletto depiction in two ways. First, he fleshes out the structure through which multinational firms, Brazilian enterprises, and state agencies antagonistically cooperate. Second, by moving a good part of the way toward applying Wallerstein's concept of "semi-peripheral states" in his concluding comparisons of Brazil with Mexico and Nigeria (although unaccountably not with Argentina), he specifies and delimits the applicability of his own model of the triple alliance.

This leads to the second general point, the two intellectual tasks of social science. The first task is to identify essences, to name, define, and characterize material and social relationships. The second is to explain variation. Most social science necessarily does some of each, although routine social science, in Kuhn's sense, simply assumes through its use of terms and concepts that the essences have already been properly identified. Conversely, most ideological struggle among social scientists focuses precisely on questions of essence rather than variation (e.g., status versus class, norms versus material structures). Stinchcombe thinks that identifying essences is either ritual totemism or irrelevant baggage, and that explaining variation is all that matters, all that distinguishes strong social science from weak. But in my view the two tasks are connected, because the better one's identification of essences, the more likely one is to be able to account for variation. This is particularly true for routine social scientists. And insofar as social science is inescapably political, getting the names right is half the battle. Chomsky and Herman's book is limited because they are not interested in variation, but only in documenting an important essence of Third World reality, namely brutal repression with US aid, connivance, and misrepresentation. Evans by contrast cares *both* about an essence — industrial development — *and* about variation — *more or less* industrial development in different countries. This interest in explaining variation enables him to define clearly the configuration he finds in Brazil's economic institutions.

Chomsky and Herman's basic thesis in their first volume is that "under frequent U.S. sponsorship, the neo-fascist National Security State and other forms of authoritarian rule have become the dominant mode of government in the Third World" (8). An essence is posited, with two aspects: terrorism by the state in the form of torture and genocide, and a massive US presence in promoting, supplying, condoning, and downplaying or misrepresenting that terrorism. Numerous instances of this essence are then described: East Pakistan, Burundi, Paraguay, Brazil, East Timor, Indonesia, Thailand, the Philippines, the Dominican Republic, Chile, Argentina, Guatemala, Nicaragua, and the various Indochinese countries. There is a basic truth here, a monstrously important truth, usefully catalogued and righteously denounced. In the post-World War II period the United States has been the major purveyor of terror in the world; in Chomsky and Herman's language, "*Washington has become*

the torture and political murder capital of the world" (16, italics theirs). This is amply documented by Michael Klare, whose indispensable labors the authors amply draw upon. Furthermore, no satisfactory holistic account of the recent Brazilian trajectory is possible if it slights the use of repressive violence against the left and the use of genocidal violence against the indigenous population. Nor would such an account be complete without exploring Brazil's role as what Chomsky and Herman call the "torture-aid subcontractor" for Latin America. Finally, there is no gainsaying that state-initiated repressive violence "*has a functional relationship to investment climate*" (54, italics theirs). Insofar as there is any economics in Chomsky and Herman's "political economy of human rights," this is it.

If one, however, is interested in variation as well as essence, Chomsky and Herman are not particularly helpful. They organize much of their book around a distinction between "benign" and "constructive" terror, the difference presumably being Washington's degree of approval. These contrast with "bloodbaths" and other heinous forms of Communist terror which so preoccupy US officials and journalists. Chomsky the linguist quite properly turns on its head the everyday usage of a term reserved for the Palestinians and Vietnamese. But there is no consistent analytical thread here: both anti-communism and the desire for investment and trade opportunities motivate the US in supporting and/or justifying all this benign and constructive violence, and the authors attempt neither to connect different sorts of US interests to different kinds and amounts of terror, nor to connect these latter to different political and economic outcomes. One could plausibly assert, for example, that in large measure the relative *absence* of US economic interests led to the wanton devastation of Laos and Cambodia. One could add to such an account Franz Schurmann's subtle analysis of bureaucratic trade-offs in his sadly neglected *Logic of World Power*: the unrestricted Air Force and Navy bombers had a green light denied them over the more politically sensitive Vietnamese terrain. Because they are interested in essences rather than variation, Chomsky and Herman's rendering of Brazilian "subfascism" is superficial and in places misleading. First, they place such emphasis on the US role in subverting Brazilian democracy in the early 1960s that they slight the activity of the Brazilian right in general and the military in particular. This gives an impression of US imperialist omnipotence, when in fact the Brazilians have used the US as much as the other way around, if not more. Second, the characterization of Brazil's "denationalized client fascist elite" as "devoid of any economic ideas of their own," as engaged in "dogmatic adherence to free enterprise," as "economically illiterate" (25), is simply far-fetched. Diemists and Mobutuists, perhaps; Brazilian generals and technocrats, hardly. Or again, to say that those generals are "visionless creatures of U.S. imperial policy aping their masters" (103), is to miss the consciously nationalist self-strengthening activities that would enable the Brazilian government to tell President Carter in 1978 to peddle his human rights pieties elsewhere. Third, there is no dynamism in Chomsky and Herman's model, no tension or contradiction, but rather a super-abundant moralizing. They speak of an "expanding empire of violence" (9) when events in Iran and Nicaragua, not to mention Zimbabwe or the US defeat in Indochina, suggest that there are strong counterforces at work in the world. Not the least

of those forces exist at the present time in Brazil, where the conjuncturally specific job of repressive violence – I am not one of those who thinks it greatly inappropriate to call it fascist – has quite possibly been done. In fact, the time may be ripe for further openings toward a more democratic politics as one way out of the current stagnation.

Chomsky and Herman have written a painful political tract about “the sun and its planets” (frontispiece), the US and its clients. Its strength as an outraged correcting of the official and journalistic rhetoric is at the same time its weakness as social science: it largely remains at the level of discourse used by the authors’ political opponents. It names and defines an essence all too frequently missing from current social science analysis of the Third World. But in so doing, it fails even to suggest reasons for the variations in “fascism,” as exemplified in its failure to provide more than an extremely rudimentary model for comprehending Brazil.

Evans’s *Dependent Development* is by contrast attuned both to essence and variation. It thereby provides a useful model, not of Brazil as a whole, but of the organization of the three major economic actors: multinational firms, local capital, and state actors. An introductory discussion of dependency theory from Baran and Furtado to Amin and O’Donnell introduces that trio theoretically, recapitulating the idea that military rule is necessary for the repression of the previously mobilized and organized urban working class, which is in turn necessary for moving beyond easy import substitution to heavy industrialization. A long second chapter discusses the transition from periphery to semi-periphery, in Evans’s language, from “classic dependence” to “dependent development”. Then follow several case studies of the competition and bargaining among the three factions of capital in various sectors: the multinationals and the national bourgeoisie in pharmaceuticals and textiles; the multinationals and the state in research and development; all three in steel, petrochemicals, and metals. A final chapter attempts to put the whole model in perspective, in part by comparing Brazil to Mexico and to Nigeria. Two questions thus animate Evans’s book: what are the origins and limits of dependent development, and what are the institutional mechanisms through which it works? To take the second question first, Evans shows convincingly that “the global rationality of the multinationals” detracts “from their natural [sic] contribution to local accumulation,” but that this “contradiction” is “resolvable by bargaining” (276), bargaining which cumulates over time. The outcomes of such bargaining vary “by industry and by issue” (277), but local capital – the famous national bourgeoisie – is by no means dead, having decided advantages “in situations where integration with the local social structure [is] the key to business success” (281). “The weakest of the three partners” (280), local capital has yet shown considerable vitality and entrepreneurship. As for the state, its central role in promoting accumulation is amply documented, but the case studies also show that because state enterprise managers share conventional corporate ideology, their activities are not always consistent with overall state policy. The case studies and especially Evans’s discussion of their implications (275–290) repay careful attention, including his penultimate warning that the “vulnerability” of Brazilian accumulation to “disruptions in the international economy constitutes the most obvious limitation of the triple alliance” (290).

Evans's answer to the first question, about the origins and limits of dependent development, also makes an important contribution, by specifying the applicability of his triple alliance model to semi-peripheral countries. Here I find Evans caught half-way between the conventional "developmentalist" perspective that still holds sway in most studies of "national" industrialization (marxist or otherwise) and the emerging world-systems perspective. To judge from the organization of the book, he began his work as a developmentalist and came to see the conceptual advantage of the semi-periphery as he was finishing it. This is highlighted in his concluding comparison of Brazil both with Mexico, where the parallels are great, and with Nigeria, where the petroleum boom has paid for an increasingly strong state now in the process of orchestrating a triple alliance. Although he is not explicit about it, Evans's account of Brazil's transition from periphery ("classic dependence") to semi-periphery ("dependent development") also exemplifies the advantages of a world-systemic view, especially as his data contradict the "developmentalist" theoretical statement with which he begins: "Classic dependence . . . created forces of production and social groups that eventually transformed it into a very different kind of political economy" (56). The linchpin of his argument about the transition is the shift from Great Britain to the United States as the major source of foreign capital (76): US capital was never much invested in Brazilian primary exports and went heavily into industry. Furthermore, US-German rivalry before World War II gave the Vargas government leverage to gain financing for state-owned steel works (88–89), and both rivalry with and imitation of Argentina – Evans somewhat downplays this aspect – pushed the Brazilian military to pressure for both steel and petroleum production (90). To this one may add such better-known facts as the world depression-induced spur to import substitution, the World War II boom, and the outward push of US capital. Thus it is incorrect for Evans to claim that classic dependence was a "self-transforming" system (94): his analysis shows quite clearly that Brazil was and is part of a larger, truly self-transforming system, the capitalist world-system.

This brings me back to the present, to the limits of the current arrangements in Brazil, to the questions of essence and variation, of Brazil as a model, and of models of Brazil. Evans is quite clear that dependent development has reached a critical point, especially given the world economic slowdown, with capital and intermediate goods imports rising faster than exports, creating mounting indebtedness. He usefully points to the three Brazilian strategies of export expansion, all of which in combination will help somewhat: renewed classical dependence through primary exports, subimperialism through exports of manufactured goods to the periphery; and export platform manufacturing for the core. The competitive success of these exports requires that Brazilian wages be kept low, yet to increase the size of the Brazilian national market would require that *some* workers' wages be raised. Unfortunately, Evans does not explore the political possibilities through which this structural contradiction will or will not be overcome, concluding merely that for dependent development to continue core capitalists will have to keep up *their* end of the multinational alliance against the potential resistance of neo-protectionists and endangered workers. Evans's "triple alliance" model then is a helpfully

accurate representation of Brazil in part because his interest in both variation and essence leads him to see Brazil itself as a model for only a few Third World semi-peripheral states, rather than as an instance of some fascistic essence. Perfectly aware that accumulation has thus far required exclusion of the mass of the population from welfare and politics, Evans, like Chomsky and Herman, has perhaps underestimated the chances for greater political and material inclusion of parts of the working class. For the members of the triple alliance there are obvious risks in such a change, hence the hesitancy with which democratic openings have been broached in the last few years. Unfortunately, Evans so concentrates on economic institutions that one cannot learn from his book how united or cohesive the Brazilian bourgeoisie really is, how likely it is to try to combine internal with export expansion.

Chomsky and Herman call attention to numerous examples of repressive and genocidal states in the Third World, all supported in one way or another by the US. The absence of a political dialectic, the lack of any serious economic analysis, and their innocence of comparative method greatly detract from the scientific value of their work, a political compilation which must not be ignored. Evans's sociological analysis of economic institutions in Brazilian industry and his comparisons of Brazil to other semi-peripheral states make a scientific contribution with political lessons for those who wholesale this or that model of revolution as promiscuously as conventional social scientists wholesale this or that model of modernization, dependence, or class struggle. One wishes for more books like his, but with more explicit attention to the political aspects of world capitalism as well.

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20th Century Gothic: America's Nixon by Sherri Cavan (San Francisco: Wigan Pier Press, 1979).

Evil, visions of evil, and illusions of evil reverberate throughout this ambitious study of Nixon as cultural phenomenon. Professor Cavan, in publishing *20th Century Gothic* herself, demonstrates that at least one kind of evil, the corporate stranglehold on publishing, can be bypassed if not defeated. Other kinds of evil, real or illusory, although not defeated, are here incisively scrutinized and evaluated. Nixon, Cavan asserts, was a development of broad cultural forces and local events, who in turn came to dominate those same cultural forces through impression management and the manipulation of a collective vision of evil. This vision, or "gothic imagery," is, according to Cavan, "a vision of life in which good and evil are locked in mortal combat, winner take all," and is at the heart of twentieth-century American culture and politics. Because it is a *collective* vision, and because Nixon was able to use and abuse that vision according to his desires, American society shares in a collective guilt for his success. We created the conditions and forces within which Nixon germinated and blossomed, and we gave birth to and nurtured our own version of the "gothic," so we must then bear, however uncomfortably, the guilt of our complicity. Cavan arrives at this conclusion through a wide-ranging analysis of both Nixon and his cultural milieu, relying upon a variety of

social sciences. Her achievement is both instructive and unsatisfactory. Cavan views Nixon from a number of perspectives, but the insights we receive are marred as we are never quite sure where precisely these perspectives coalesce.

In the wake of bewilderingly rapid technological innovation and change arises a cultural version of paranoia, Cavan says. Condensed into a “gothic vision,” this fear ascribes extreme malevolence and power to dark forces of unmitigated evil. Only in a kind of Zoroastrian or Manichean war between Good and Evil, relentlessly fought to the death, lies any hope of salvation. Nixon, as a product of the culture that created this image, shared in the collective vision and used it to his advantage. Allying himself with the virtuous forces of the Good, he perceived his enemies, both real and those he created, as absolute Evil. *Ecrasez l'infâme* became his ceaseless cry for assistance in the universal battle against those “out there” who intend to eradicate us all. In the politics of fear he thrived in and perpetuated, Nixon was uniquely skillful in his ability to appropriate, assimilate, and manipulate the “gothic image” through impression management and illusory scenarios. “He was,” Cavan notes,

a man who had a deep conviction that forces of evil were loose in the world and would destroy all that was good unless they were countered by powers equally ruthless. The dramatic clash of good and evil resonated in all he said throughout his entire career. This gothic imagery provided the leitmotif of the world view he propagated, and ultimately it transformed party politics from thesis and antithesis to warfare and sabotage (155).

Nixon was our most successful conman of illusions, our very own “gothic huckster, the evangelical salesman whose only product was himself” (266). Yet the conman or the huckster can only succeed if someone buys his wares. Herein lies the collective complicity in Nixon’s rise, Cavan says, for “quite simply, the conman can take the mark only if the mark himself has larceny in his heart” (224). The American public was willing to believe the illusions Nixon created, and to swallow the solutions he proposed. Nixon succeeded as well, Cavan theorizes, because he was at the vanguard of twentieth-century thought. His peculiar brand of self-serving logic, wherein all of his own actions were sophisticatedly rationalized, reflected the ultimate influence and assimilation of relativity and enlightened self-interest turned to evil ends. Such a perversion of these endearing American values was natural for Nixon, a man well accustomed to betrayal and deceit. Cavan catalogues Nixon’s betrayal of his parents, his family, his religion, his compatriots, his co-conspirators, his party, and the trust of his nation. The legacy of Nixon, Cavan says, closely resembles the legacy of Hitler. The cultural strain in America in the 1950s was a New World version of that same strain in Germany in the 1920s, and both Nixon and Hitler can lay claim to being the true “gothic heroes” of the twentieth century.

Cavan arrives at these and other striking and often outrageous conclusions by bringing to bear upon her study of Nixon the insights of various social sciences. Her work is a confusing blend of history, political science, sociobiography, sociolinguistics, and anthropology. Thus she scrutinizes at a distance Nixon’s inner sanctum as a cultural anthropologist would observe an alien tribe. From

this perspective, Bebe Rebozo seems to function as co-wife, and H. R. Haldeman as office husband and mother. Through analysis of the taped White House conversations, Cavan ascertains that the conspirators spoke a macho language heavily weighted with the metaphors of gangsterism. *20th Century Gothic* is brimming with such insights, as Cavan shifts from one perspective to another, allowing the juxtaposition of divergent approaches to imply and infer connections and crossings that have been missed in more narrowly focused studies. The scope of the book is sweeping; we are carried along in almost one breath from gargoyles on the parapets of Medieval cathedrals to mushroom clouds, and from the Renaissance chivalric code to popular novels of evangelical hucksterism. We are propelled into the broad forces of historical and cultural change, finding ourselves immersed in Nixon's world. And so we must be, if Cavan's theory is correct, for Nixon's world must perforce be *our* world. His illusions, fantasies, and fears are interchangeable with ours. He is, was, and ever shall be "America's Nixon".

There is, however, a central methodological difficulty with Cavan's book, which mutes the force of her arguments. Juxtaposing social scientific approaches and theories is enlightening, but their relationship with one another is not wholly or satisfactorily clear. The anthropological perspective is suggestive, but the First Family is not an alien tribe; rather, according to Cavan's own thesis, Nixon and family are, as Marlowe said of Lord Jim, "one of us". Cavan herself seems startled by some of her conclusions, for while she notes the gangster language of the Oval Office, she notes also that not everyone in the society uses such language, even though our culture "*is organized in such a way that what was once considered criminal is now common practice*" (251, her emphasis). In other words, there was something fundamentally different about the Nixon circle, which Cavan does not quite capture. He may not in fact have been "one of us". Too often the cultural forces Cavan describes are too broad to be of any particular use in ascertaining Nixon's uniqueness, and the localized events in his life point more towards his own quirkiness than to his place in the mainstream of American culture. As well, Cavan's section on sociolinguistics is her weakest; by editing the Nixon transcripts ("to facilitate reading"), Cavan robs herself of the opportunity to strengthen her case. The "hems" and "haws," the circumlocutions, the redundancies, and the long pauses that she has edited out are of utmost importance in any study of Nixonian language. They demonstrate how deeply ingrained were obfuscation and deliberate ambiguity in all aspects of the presidential coterie. Silences are especially communicative; consider, for example, how much is communicated through the gaps in a Chekovian dialogue. These shortcomings – of intermingled perspectives and condensed protocols – do not destroy Cavan's arguments, but they unnecessarily weaken them. *20th Century Gothic* is more suggestive than convincing.

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Accounting for Genocide: National Responses and Jewish Victimization during the Holocaust by Helen Fein (New York: The Free Press, 1979).

No single event has engendered more fervent or emotionally laden research

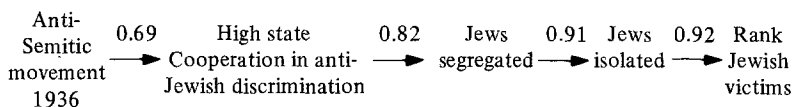
than the Holocaust. Research has been dominated by conventional historical exposition of what happened, when, where and why with the aim of assessing responsibility. Previous studies have usually focused on Nazi policies, the extent of domestic collaboration, and Jewish non-resistance. Recently, however, the sociologist Helen Fein has broken out of the confines of traditional historical analysis which emphasizes the uniqueness of these events and their underlying causes. In *Accounting for Genocide* Fein uses comparative and statistical analyses to reevaluate the impact of these and other factors. She synthesizes previous work into an original theoretic framework that goes beyond the assessment of blame, beyond the particulars of Europe's darkest hour toward a general understanding of political cruelty. Fein's major theoretic interest is the structural conditions of genocide. She asserts that groups excluded from a polity's "universe of obligation" (i.e., lacking solidary bonds to the politically dominant group) are vulnerable to genocide. However, genocide can be implemented only if the international setting insulates a state from external interference and if, internally, the vulnerable group cannot forge alliances which will enable it to evade capture.

She begins by comparing the Turkish massacre of Armenians during World War I and the Nazi extermination of European Jewry in World War II. Her comparative analysis is remarkably similar to recent work on world-systems theory. (1) Both victimized groups comprised a middleman minority in states at the periphery of the world system. Their economic role in these states made them particularly vulnerable to competition from rising groups. (2) Both groups were desired as middlemen precisely because they lacked a native political base. From the start they were excluded from the national universe of obligation. (3) In both cases genocide was part of the creation of the state order being forged by a new political elite. Wars provided the opportunity to act against a vulnerable group by insulating the Young Turks and the Nazis from external interference. Interestingly, Fein generates this analysis from a Durkheimian value-consensus theory emphasizing universes of obligation, whereas world-systems theorists begin with a marxist-weberian framework emphasizing structural relationships. Thus despite her interest in the structural conditions of genocide, Fein focuses on the inter-group attitudes that emerge during the formation of nation-states rather than on the relative power and resources of different groups. Nevertheless, like analysts such as Wallerstein and Skocpol, Fein stresses that the international setting affects opportunity structures impinging upon various groups acting within smaller political structures. Moreover, Fein also notes that a state's position in the world system affects inter-group relations within it. But whereas Wallerstein and Skocpol emphasize structural relations between groups, particularly class relations, Fein focuses on group attitudes and solidary bonds.

This cross-case comparison, however, illuminates only the conditions affecting a group's vulnerability, and some of the conditions allowing a state to implement genocide policies. An in-depth study of the Holocaust allows for a cross-state comparison of the conditions for *successful* implementation of genocide. The Holocaust occurred with the cooperation of political structures that varied across Europe and with differing degrees of success. Rather than assuming

that Nazi policies were the exclusive determinant of the Holocaust, Fein examines the characteristics of the political communities which implemented them. She notes that the percentage of Jews “victimized” (i.e., killed directly or indirectly, or interned in concentration camps) varies from over 95% (in Poland) to less than 1% (in Finland). Using regression analysis, she sets out to explain this variation and thus to determine the conditions under which genocide can be successfully implemented. Her study encompasses all of Nazi-dominated Europe (22 states and political units) except the Soviet Union (for which suitable data were unavailable) and countries with small prewar Jewish populations (e.g., Luxembourg). Her major finding is that where Nazi control was strongest the victimization rates were highest. But elsewhere the victimization rates were determined by the level of prewar anti-Semitism. These two causes alone explain most (86%) of the variation in Jewish victimization. Commonly offered explanatory factors such as local variation in Nazi directives, Jewish responses, and domestic collaboration were also affected by these independent variables and are analyzed as intervening variables.

Fein’s model of the process of genocide therefore provides a powerful framework for reevaluating and synthesizing previous work. Exclusion of Jews from the national universe of obligation, indicated by the presence of successful prewar anti-Semitic movements and the absence of rigorous government responses to protect the civil rights and liberties of Jews, accounts for high levels of state cooperation in implementing Nazi policies of anti-Jewish discrimination. Indeed, the Nazis relied upon this cooperation – except in those areas subjected to total control. Hence, high state cooperation led to the *de facto* social segregation of Jews. Once this was accomplished it was relatively easy to physically isolate Jews, and once Jews were isolated their fate was virtually sealed. This is represented in the following diagram, showing zero-order correlations between the variables (354):



This analysis calls traditional interpretation into question. First, Jewish victimization is related to a polity’s willingness to segregate and isolate Jews – and not necessarily to its desire to get rid of them. Second, Nazi domination created new opportunities for anti-Semitic states to take even harsher actions against the Jews than they could before the war. Thus the importance of Nazi policy extends beyond its impact on Nazi actions; it also catalyzed state cooperation. Third, factors that inhibited state cooperation in discriminating against Jews increased their life-chances. Church protests and steps on behalf of Jews by resistance movements or by exiled leaders could lower the victimization rate (except where Nazi control was most intense). The effect of intervening factors is clearly demonstrated in Fein’s treatment of the two exceptional cases, the Netherlands and Rumania. Despite the low level of anti-Semitism in the Netherlands the victimization rate was almost 80%. There was a high degree of cooperation by the Dutch civil service bureaucracy which continued mechanically to implement orders from above. Neither the Dutch

Reformed Church nor the government-in-exile exerted their leadership to encourage non-compliance. Moreover, Dutch social institutions initially accommodated the Nazi occupiers. This response was mirrored in the Jewish community because successful Jewish social defense movements required the cooperation of non-Jewish social defense networks, which were either non-existent or poorly organized until after half the Dutch Jewry had already been deported. Anti-Semitic Rumania, on the other hand, began liquidating its Jews even before explicit Nazi orders were given. Nevertheless, as Germany's defeat became imminent Rumania tried to extricate itself from the Axis and reversed its policy of state cooperation. This belated resistance was supported by both the state and the Church in order to minimize the expected postwar backlash from the apparent victors. The victimization rate in Rumania was under 60%.

Fein's sociological model of a process occurring over time explains both the general pattern and the exceptions because she accounts for variation in the crucial intervening processes. This strength is further highlighted in her analysis of church actions. She finds that church protests could indeed reduce the victimization rates. In those areas with the least amount of Nazi control, positive church statements deterred collaboration and instigated cooperation with Jewish social defense movements. Fein extends her regression analysis to explain the various responses by the dominant church in each state to anti-Jewish policies. Whether the dominant church in a nation protested against the deportation of Jews was, according to Fein, a function of religion, the extent of Nazi control, the political history of a state, and the extent of anti-Semitism. Where Nazi control was exerted through indigenous institutions, Roman Catholic churches were less likely to protest than Protestant or Orthodox churches. On one hand, there was a high correlation between anti-Semitism and the predominance of Catholicism. On the other hand, Catholic states were frequently those states created after World War I, or, in the cases of Croatia and Slovakia, created by the Nazis during World War II. Thus non-Catholic churches in older states where prewar anti-Semitic movements were the least successful were most likely to protest. Church protest was totally absent in nations subjected to the most intense Nazi control and in other predominantly Catholic states where anti-Semitism was high (and which were also formed since World War I). Moreover, Fein assesses how church protests affected victimization rates. They reduced the extent of state cooperation and facilitated the extension of social defense networks to Jews. Because Fein goes beyond an examination of any particular church and analyzes the effects of different historical and structural characteristics of states on church actions, she significantly contributes towards resolving this historical controversy. Her research design does not, however, allow her to examine the importance of the anti-Semitism net of the political peculiarities of the newly formed Catholic states. Nevertheless, she raises this as an important question for future research. Conventional history could not even specify the nature of this problem.

Fein also contributes to the debate on the effect of cooperation by the *Judenräte* (Jewish Councils) on the fate of the Jews, which has centered

around the validity of the Arendt thesis. To implement the Final Solution, Arendt argues, the Nazis required the cooperation of the *Judenräte*, which administered and policed Jewish communities and frequently rounded up Jews for deportation to the death camps. Critiques of Arendt's work have focused upon her factual errors, or have tried to explain why the *Judenräte* cooperated, either by tracing the long history of successful accommodation by Jewish communities to hostile governments, or by demonstrating the "rationality" of certain decisions given the knowledge of circumstances and alternatives available to them. None have been able to assess the net effects of *Judenrat* actions upon the ultimate fate of the Jews. Fein analyzes the causes and consequences of *Judenrat* cooperation in deporting Jews within the context of how Jews became victims in each polity. First, non-Jewish resistance decreased the likelihood of a *Judenrat* being established. Second, *Judenrat* cooperation was most likely where Jews had been isolated and consequently virtually all alternatives were closed. Where a *Judenrat* was established and the state did not cooperate or resistance movements aided the Jews, *Judenrat* accommodation was less likely. From these findings, Fein estimates the effect of *Judenrat* cooperation and separates the effects of the extermination process from its causes. *Judenrat* cooperation in the deportation of Jews led neither to the isolation of the Jews nor to greater victimization. Rather, isolation was a cause of both. Fein shows that Arendt correctly ascertained the necessity of local cooperation for the implementation of the Final Solution, but that she mistakenly stressed the role of Jewish institutions. Implementation was facilitated primarily by non-Jewish cooperation in the early stages of the genocide process. Jewish responses were largely determined by non-Jewish responses.

In addition, Fein's analysis suggests that if *Judenrat* cooperation prior to the deportations (e.g., the administration and policing of the ghettos in accordance with Nazi directives) increased the victimization rate, this was probably because it facilitated segregation. Some *Judenräte* probably prevented the emergence of Jewish leaders and "activist" organizations which might have been able to enlist outside support. In other words, *Judenrat* actions after ghettoization probably had no significant adverse impact upon the ultimate victimization rate. Thus Fein challenges both the Arendt thesis and the majority of other studies that have focused on the *Judenräte* in Eastern Europe. Such studies are certainly valuable for understanding the dynamics of ghetto life, but they are misdirected when trying to determine the causes of Jewish victimization. Fein does more than identify the structural determinants of the various stages of the extermination process. She evaluates some of the hypotheses of those historians upon whose scholarship and archival research she relied. She discounts the importance of the length of warning time available for mobilizing resistance to the Final Solution as an explanation of Nazi success. The most effective strategy for the Jews was evasion of the Nazi net. Because this strategy could be successful only where Jewish social defense movements established non-Jewish allies, warning time accounted for much less variation than factors influencing non-Jewish responses to Nazi control. Different states reacted differently: some adjusted to the new conditions while simultaneously using the available time to help the Jews evade

deportation; others did not use the time available; and still others facilitated the execution of the Final Solution.

Fein also rejects historical interpretations claiming that Jewish demographic characteristics, such as their percentage of the population, absolute size, or extent of concentration in urban areas, are causally related to the victimization rate. Demographic characteristics account for little variation in victimization rates. Jew's social position and the nature of their communal organization were far more important. Specifically, the relationship between Jewish and non-Jewish organizations and communal institutions is the relevant set of variables. First, the willingness of a polity to cooperate in the discrimination of Jews is a function of the type of competition between ethnic and occupational groups which emerges in each society and of the position of the Jews within both the social structure and the organizations involved in the competitive struggle. Thus, for example, in the Balkan states carved out of the Ottoman empire, Jews were only one of several ethnic groups (e.g., Armenians, Greeks, Syrian Christians) overrepresented in middleman occupations and roles. Hence the Jews were less salient and, as a group, less apt to be the primary target of those competing for entry into these social positions. Second, a state's position in the world economy affects this competition and the nature of the resultant discrimination against minorities. This suggests a need to reexamine the effect of the interwar economic crises upon the position of various states (including Germany) in the world-capitalist system and how this affected the nature of inter-group relations within each state — especially the new violent expressions of anti-Semitism. Third, avoiding victimization was related to the ability of the Jewish leadership to mobilize social defense networks in cooperation with non-Jewish allies. Thus the structure of the Jewish community and the characteristics of inter-group relations — rather than demographic factors — were the crucial variables.

Accounting for Genocide exemplifies the contribution sociology can make to Holocaust studies. Fein tested some of the hypotheses of previous research, was able to support some explanations, reject others, and specify how yet others remain problematic. She formulated new explanations derived from her theoretic framework of the nature of nation-states and from statistical inferences about the relationships between several variables. Moreover, her application of statistical controls to factors influencing historical processes affects the agenda for future research. Certainly Fein has demonstrated the necessity for rigorous comparative studies of genocide in order to further explicate the impact of the international setting upon political processes within nation-states that make groups vulnerable to genocide. Furthermore, determinants of competition between political actors — including the state itself — need to be further explained. How does this competition affect responses to the Nazis? Do different forms of competition lead to different consequences under different conditions of Nazi rule? These questions call attention to the limitations of the value-consensus approach. Fein is of course correct to focus on how the characteristics of the different polities affected the success of Nazi genocide. But her theoretic conception of states causes her to overlook many of these. First, as noted above, her Durkheimian ap-

proach ignores the relative power and resources of different groups within the state. But these structural factors are as important as the international setting in creating opportunity structures for implementing genocide. Consequently, Fein has little to say about how anti-Semitism among non-dominant political actors (including both ethnic groups and churches) affected the fate of Jews. France, for example, is classified as a "low" anti-Semitic state (similar to Denmark, Belgium, and Finland) because prewar governments acted against anti-Semitic movements. Nevertheless, the advent of the Vichy government certainly provided new opportunities to traditionally anti-Semitic political actors. Fein cannot, however, estimate how changes in the power relations between groups affected the propensity to cooperate with the Nazis, or the ability of Jews to establish contacts with the resistance, or the likelihood that the Catholic church would actively protest against the deportations. Second, Fein notes that middleman minorities are particularly vulnerable because of the willingness of various groups to gain economic advantages by discriminating against them. Those who might gain from the discrimination against such minorities are not necessarily the dominant groups. Nevertheless, the Nazis could enlist their cooperation, especially if they were strategically placed within part of the state bureaucracy and/or local governments.

A model accounting for organizational competition for power and resources and which relates this to the social and political structure of each state would be less abstract and more powerful than Fein's model which stresses groups' attitudes to each other. Rather than being universes of obligation, nation-states can be seen as arenas for the competition of groups for resources. Such an approach would force us to ask how anti-Semitism led to state cooperation; or rather, to the cooperation of strategically placed groups within the state. It would also force us to explore the dynamics of simultaneous cooperation by some groups and the formation of underground escape networks by others. Fein's model merely indicates some of the causes of cooperative state and church attitudes. Still, Fein's sociological study contributes much to our understanding of how genocide has been implemented in the twentieth century. Fein's study is unique because it merges a theory of how a particular group becomes vulnerable to genocide with an analysis of the international variation in victimization rates. Thus she forsakes the traditional units of analysis – either the perpetrators or the victims – and studies the political communities subject to Nazi pressures. She can therefore examine the interaction of various groups and relate this to the conditions that provided the opportunity successfully to exterminate the Jews. The result is a scholarly and original treatment of the relationships between the characteristics of the social and political structures of states and genocide. Holocaust scholars will have to treat seriously her findings and inferences and thus begin to incorporate the sociological perspective into their work.

Why, however, has it taken so long for a serious and comprehensive sociological treatment of the Holocaust to appear? Is it because the primary sources are hidden in the mysterious corners of archives where historians have staked their territorial claims? Is it because those historians uniquely qualified because of their mastery of several European languages are not trained in socio-

logical methods? Only partly. The exclusion of sociology from Holocaust studies also stems from an unacknowledged fear that the quantitative analysis of theoretical propositions will somehow defile and desecrate this tragic experience (Cf. Irving L. Horowitz's review in *Contemporary Sociology*, 9;4, which expresses this fear). Conventional historians discuss the unique features of the Holocaust, thus separating it from the mundane. This is good conventional history – filled with the pathos of a scholar trying to feel and penetrate a trauma locked behind the walls of time. Fein has shown, however, that sociological methods can certainly be sensitive to the texture of historical events and human suffering. She used statistics to put order into chaos. By critiquing competing interpretations of how and why the Nazis were able to implement their racial policies against the Jews, she has furthered our understanding of how the Jews became victims.

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Freud, Biologist of the Mind by Frank Sulloway (New York: Basic Books, 1979).

The development of Sigmund Freud's ideas has been shrouded in a dense mythology, argues Frank Sulloway in his much discussed *Freud, Biologist of the Mind*, a mythology begun by Freud himself and sustained by orthodox psychoanalysis. Sulloway seeks to dispel this mythology and uncover the "real" Freud hidden behind it. The result is a well-researched, enlightening, but ultimately paradoxical work: Sulloway painstakingly exhumes a "Freud" about whom one can only muse as a historical curiosity and then quickly rebury.

The critical period in Freud's intellectual development ran from 1895 until roughly 1909 – or from the break with Josef Breuer, his collaborator on the early hysteria studies, until Freud's visit to the United States, which confirmed the growing public recognition of psychoanalysis. During this time, Freud put together psychoanalytic theory in its basic form and wrote his classic works on dreams, jokes, the psychopathology of everyday life, and sexuality. According to the familiar orthodox account, Freud labored in isolation against an indifferent and often hostile world to create a fundamentally original theory – a pure psychology that decisively broke with established biological theories of mind. Through careful research and self-analysis, he discovered the existence of infantile sexuality, in all its polymorphous perversity, and constructed his theory of neurosis thereon. Freud thus had appeared as the prototype of the independent, empirical, embattled scientific hero.

Sulloway convincingly challenges this picture of Freud's intellectually formative years on all counts. Freud, he shows, never labored in total isolation; he always had at least a few intellectual intimates, primary among whom was Wilhelm Fliess, that enigmatic figure so readily dismissed by orthodox psychoanalysis as a quack. Sulloway demonstrates that Fliess's ideas were respectable at the time and that he anticipated several of Freud's most important "discoveries". Similarly, the negative response to Freud's work has been

grossly exaggerated: Sulloway calls upon written reviews and the minutes of professional meetings to show that Freud's work on hysteria, dreams, and sexuality all received substantial attention, much of it positive. Psychoanalytic theory, moreover, was *not* a great rupture with the established wisdom of the day. To the contrary, it was strongly rooted in contemporary biology and sexology; even the notion of infantile sexuality, far from being invented by Freud, was quite familiar at the time. As a result, psychoanalysis is not the pure psychology that its adherents (including Freud himself) have claimed. It is a "psychobiology" whose biological dimension is essential.

What is the biological element in Freud? Like many of his contemporaries, Freud advocated a "biogenetic" thesis and a Lamarckian view of evolution. That is, he believed that the development of the individual repeated the evolution of the species (ontogeny recapitulates phylogeny) and that this evolution proceeded through the inheritance of acquired traits. According to Freud, human evolution and the growth of civilization involved renunciation of a series of instinctual satisfactions incompatible with these developments. Originating as psychical defenses, these renunciations somehow became incorporated into humanity's genetic endowment and thus became organic. The psychosexual development of the individual is an organically determined repetition of this phylogenetic process, passing through a similar set of instinctual renunciations. This biogenetic thesis was the bedrock upon which Freud anchored his notion of infantile sexuality, its polymorphous nature, its progression through an ironclad set of stages, and hence his theory of repression and neurosis. It also underlies Freud's work on culture as well as his later theory of the instincts. Those biogenetic ideas that readers of Freud find so perplexing and inexplicable are thus not at all incidental, according to Sulloway; they are a systematic part of Freud's work and do not diminish in his later writings. Although Freud often protested that he was leaving biology behind, he took it wherever he went.

Having exposed the myths of psychoanalysis, Sulloway attempts to explain why they were created in the first place. If Freud was in truth a "psychobiologist," why did he and his followers regularly claim that these theories broke decisively with biology? If Freud was not an isolated, embattled figure in those formative years, why did he (in his autobiography and elsewhere) claim that he was? Sulloway argues that these twin myths lie at "the heart of the epistemological politics that have pervaded the entire psychoanalytic revolution" (488). By picturing psychoanalysis as rooted wholly in a new set of empirical discoveries based on a distinctive psychoanalytic methodology rather than in previous theories of the mind, these myths provided psychoanalysis with special claims to both autonomy from other sciences and a distinctive kind of knowledge unavailable to those without psychoanalytic training. They thus enabled the new science to legitimate itself and to invalidate its critics. Any effort to disprove psychoanalytic ideas or to assimilate them to other disciplines could be met with the argument that these ideas were rooted in a unique theory, methodology, and body of data that the uninitiated were not qualified to judge. Far from being unique to psychoanalysis, Sulloway suggests, this myth-making process is characteristic of

science in general. New scientific paradigms often shroud themselves in an empiricist myth by placing their origins in the work of an isolated investigator who through pure observation (freed from the weight of previous theory) reaches a sudden inspiration that leads to wholly new ideas. As a result, our idea of science is furnished with images of Galileo atop the Tower of Pisa, Newton under his apple tree, and Sigmund Freud discovering his own infantile sexuality. In fact, Sulloway argues, the emergence of new scientific paradigms occurs in a more collective, less abrupt way.

Sulloway's claim that Freudian theory is a psychobiology actually involves two related, but distinct arguments of unequal cogency. On the one hand, Sulloway presents an intellectual biography of Freud, which attempts to identify the historical *roots* of Freudian theory. On the other hand, he provides an interpretation of Freud's texts, which attempts to clarify the *content* of Freudian theory. These two issues, roots and content, are quite different: it is one thing to argue that Freud's ideas originate in certain strands of late nineteenth-century biology; it is another to argue that these ideas themselves are inextricably biological. Sulloway convincingly claims that Freudian theory is *rooted* in certain biogenetic and Lamarckian notions and that the idea of infantile sexuality was not invented by Freud, but was a widely accepted notion. He provides a plausible explanation of why orthodox psychoanalysis and its official biographers would systematically mystify these roots, and the implications of his argument for the historically inclined are clear. His arguments about the *content* of Freudian theory, however, are much less convincing and complete: It is not clear that psychoanalysis in itself — whatever its roots — is simply a “psychobiology”. Sulloway does not adequately explain why the biological content of Freudian theory — whatever its importance — has been so roundly ignored. Finally, he does not spell out the implications of his re-reading of Freud as psychobiology for those who would make use of Freud's ideas.

To be sure, Sulloway's interpretation of the Freudian texts contains an important kernel of truth: biogenetic, Lamarckian, and other biological ideas play a systematic role throughout Freud's work. Their presence is neither haphazard nor limited to earlier writings. Any reading of Freud not recognizing this presence is doomed to incomprehension, and the value of Sulloway's work lies precisely in calling the serious reader's attention to the consistent biological element in Freud. Sulloway, however, argues that this biological element is not only systematic but also central and essential; that biogenetic explanations of sexuality, neurosis, and repression are the only ones Freud offers; and hence that Freudian thought is *purely* psychobiological. Sulloway never supports these more radical contentions: he merely shows that one can do a convincing psychobiological reading of Freud, not that the psychobiological reading is the *only* convincing one. Indeed, he does not confront alternative readings of Freud from the perspectives of sociology, linguistics, hermeneutics, or critical theory. Sulloway sticks narrowly to the orthodox psychoanalytic literature and thus does not consider the most interesting, non-orthodox interpretations of Freud. There is no mention of Paul Ricoeur, Norman O. Brown, Herbert Marcuse, Philip Rieff, Talcott

Parsons, Juliet Mitchell, Jürgen Habermas, or Jacques Lacan. Sulloway thus does not even have an adequate conceptual framework for assessing the relative merit of an exclusively psychobiological reading of Freud. He does not allow us to inquire whether the systematic presence of biogenetic notions in Freud is a necessary foundation or a superfluous scaffolding; or to look at it another way, whether Freud speaks in one tongue or in many.

Freud was not exclusively a psychobiologist – or anything else for that matter. His discourse was ambiguous and open-ended, because he had no one set of concepts that adequately captured what he strove to say. Biogenetic notions are present, but they do not monopolize the discussion. Consider, for example, Freud's attempt to explain instinctual repression in *Civilization and its Discontents*. Searching for the biological element in Freud, Sulloway looks almost exclusively at those footnotes where Freud gives an "organic" account of repression. Freud there argues that the human evolutionary achievement of erect posture required the renunciation of those instinctual gratifications associated with olfactory stimuli, anal eroticism, and so on. Thus sexual repression can be seen as an organic defense against an earlier stage of animal existence. Sulloway's work helps us understand that this biogenetic argument is not simply an inexplicable aside, but reflects certain abiding features of Freud's thought. In focusing on these few footnotes, however, Sulloway ignores the wider argument being made in *Civilization and its Discontents* – an argument that links sexual repression not to biology but to a specific interaction between society and the instincts. There are, then, at least two quite different explanations of repression in this work, but Sulloway sees only one. The ambiguity and open-endedness of Freudian discourse is also manifested in Freud's ambivalence about the biological content of his own theories: even though Freud consistently brought biogenetic notions into his writing, he as consistently denied that this thought had any biological elements. His work is full of protests that he has left biology behind. Sulloway argues that these anti-biological protestations are a tactical maneuver on Freud's part to establish the autonomy of psychoanalysis and distinguish it from previous theories. He thus reduces the contradiction to a simple process of mystification. It seems more probable that the contradiction reflects a real tension within Freudian thought: Freud struggled with his biological concepts, sensed their inadequacy, and sought to transcend them. He never fully succeeded, however, and biological formulations thus co-exist in his thought with non-biological ones.

Putting psychobiology at the heart of Freudian thought (rather than seeing it as one element in that complex set of ideas) is not merely implausible, it also creates a puzzle that Sulloway does not adequately solve. If the biological content is so central to Freud, why has it been so roundly ignored? Sulloway does not realize that accounting for this is quite different from explaining why Freud's biological *roots* have been obscured. The latter is a relatively esoteric issue of intellectual biography and thus can be explained in terms of the epistemological politics of the orthodox psychoanalytic movement, which has dominated the biographical work on Freud. Such a narrow explanation will not do for the issue of content. The interpretation of Freud's writings,

as opposed to the investigation of his intellectual roots, has not been so successfully dominated by the psychoanalytic movement. Freud's texts are there for everyone to read, and just about everyone reads them in his own way. What must be explained therefore is not simply why the psychoanalytic movement has ignored the biological content of Freudian thought, but why a wide spectrum of thinkers, most not influenced by orthodox psychoanalysis, also have done so. Here a narrow focus on the psychoanalytic movement hampers Sulloway again: because he looks exclusively at orthodox readings of Freud, he never thinks to ask why most of the unorthodox and the anti-orthodox have made the same "mistake".

Strikingly, Sulloway's psychobiological reading of Freud seems to lead nowhere; its intellectual implications are nil. Usually when a scholar goes to great length to revise our idea of what some eminent thinker "really" said, he has some wider point to make; he wants to reorient our thinking about the world in some significant way. Surprisingly, Sulloway makes no such effort. After spending 500 pages arguing that Freud is a psychobiologist, he fails to show how this new reading of Freud provides us with fresh and fruitful insights into the psyche. Indeed, he tells us that the Freud he has so painstakingly unearthed is not of much intellectual value to us at all: "Freud's theories reflect the faulty logic of outmoded nineteenth-century biological assumptions, particularly those of a psychophysicalist, Lamarckian, and biogenetic nature. . . . Much that is wrong with orthodox psychoanalysis may be traced directly back to them" (497-498). Those odd notions about ontogeny recapitulating phylogeny and about the inheritance of acquired traits lost their popularity by the 1920s, and Sulloway is certainly loath to resuscitate them. At the same time, however, Sulloway does not want to dismiss either Freud or psychobiology. Freud, he assures us, did have something important to say, and a refined, updated psychobiology is the best bet for capturing it. Sulloway does not tell us, however, what this important something is or why a new psychobiology would fare better than the old. Indeed, he does not convincingly demonstrate what to make of this psychobiological Freud at all.

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