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Theory Trouble: The Case of Biopolitical Science¹

In den USA gewinnt eine neue „biopolitische“ Wissenschaftsprogrammatik an institutioneller Wichtigkeit. Sie materialisiert sich zugleich als ein genereller Vorwurf an die Politikwissenschaft, die in ihrer Konzeption „untheoretisch“ sei. John R. Hibbing und seine KollegInnen bauen auf den früheren Arbeiten der soziobiologischen Politikwissenschaft auf, lassen dabei aber den Rational-Choice-Ansatz von Robert Axelrod vollkommen aus. Axelrod leistet jedoch eine bedeutende interdisziplinäre Arbeit in der Politikwissenschaft und Biologie und hatte besonders hinsichtlich seines Konzeptes der „Evolution der Kooperation“ mit einem der einflussreichsten Insekt-Ethologen, Evolutionstheoretiker und Soziobiologen W.D. Hamilton zusammengearbeitet.

Warum interessiert sich dann Hibbing für Axelrod so wenig? Ausgehend von dieser Frage wird die jeweilige Praxis der Theorie von Hibbing und Axelrod verglichen. Beide sind Teil der Mainstream-Tradition der amerikanischen positivistischen Politikwissenschaft, sie fassen jedoch die „Theorie“ als solche unterschiedlich auf. Der Vergleich beinhaltet somit drei Aspekte: die auf das 19. Jahrhundert zurückgehenden Wurzeln der aktuellen Konzeption der Politikwissenschaft, Natur/Kultur und des „Charakters“; die Entwicklungen der Genomik und das Potenzial eines neuen Naturalismus; und die in außergewöhnlicher Weise verflochtenen Entwicklungen des Nachkriegs-Rational-Choice-Ansatzes und des Neodarwinismus.

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I.

It is clear that biological approaches to political science have hit the scholarly mainstream in the United States – but researchers remain somewhat nervous about their progress and prospects. A spate of recent publications follows on the successes of a new multi-disciplinary sociobiology, which has gained visibility and legitimacy following its bruising in the 1970s (Segerstrale 2001). Much of the new Darwinian political science, like much of the rest of sociobiology old and new, studies co-operation. The question of co-operation has been a prominent one in American political science at least since the emergence of rational choice theory in the 1960s and 1970s. Political action, which necessarily involves co-operation and conflict, was a given for the older behavioral and institutional pluralisms that dominated political science in the 1950s and 1960s. Collective action was not irrational even for economists and other methodological individualists until models of perfect competition became widely used earlier in the twentieth century (Tuck 2008). Rational choice assumptions, however, made co-operation and even conflict (as distinct from competition) into problems to be solved; paradoxes were identified and solutions rendered, through formulas exploring the interactions of preference-maximizing individuals. In a 2004 article on “The Origin of Politics” two scholars of American politics and political behavior, John

R. Hibbing and John R. Alford, take what they see as a very different approach from rational choice: they argue that “wary co-operation” is a biologically evolved trait. Rational choice, by contrast, “is content to take preferences as given and is not particularly motivated to explore their origins or grounding in reality.” The rational choice approach and traditional behavioralism are really “not theories at all” (Alford/Hibbing 2004b, 707). The rubrics that have dominated US political science in the postwar period are not theoretical, on this view, because what theory should do is supply “ultimate causes” (ibid., 707, 718; Hibbing/Smith 2007, 9). Biology is needed to fill this disciplinary void.

To a surprising degree the new biopolitics² is fuelled not only by the collaborative excitement of a research frontier, or by a robust ability to generate publicity and funding, but by dissatisfaction with the extant practice of theory in political science. This intradisciplinary struggle is also, it appears, a struggle over the meaning-in-use of “theory”. According to the biopolitical theory must provide foundations, and indeed, foundations of a particular kind. One way to read this intradisciplinary struggle is as a new front in the old American political science war between behavioralism and rational choice: with their charge of atheoretical-ness, behavioral researchers newly converted to biology are finally tossing back the grenade long ago thrown by rational choice insurgents, when they accused their behavioral colleagues of doing “mere description” rather than science. Real science, of course, is natural science, especially in the United States. But whereas this earlier charge was brought by colleagues inspired by the *form* of natural science, the biopolitical advance carries with it the additional prestige of content (i.e., it’s more about using natural-science findings to study politics than it is about natural-science method). If this is a new front in a continuing war, though, there is surprisingly little in the way of real confrontation.

I am going to need to stage a confrontation here, because one striking fact about the new biopolitics is the extent to which it proceeds as if prominent mainstream rational choice political science hasn’t already substantially engaged biology – and proceeds as if much of the biology that inspires it isn’t itself structured by a rational choice framework. In order to account for these curious omissions, I will attend in this article to the different practices of theory that give shape and meaning to these sometimes contending ways of doing (bio-)political science. Rational choice theory is also known as “positive theory”. Whether they know it or not it is, I argue, the very positivism in positive theory that the new biopolitical researchers, in their explanatory ambitions, find so unsatisfying as to be unrecognizable *as* theory.

II.

Hibbing is a senior behavioralist scholar of American politics who since 2004 has been involved in a flurry of publications that are bringing naturalist frameworks to political science research; he is arguably at the center of a sociobiological wave in the discipline, placing articles in the national association’s two main journals (Alford/Hibbing 2004b; Alford et al. 2005) as well as in the most prominent of the regional association journals (Alford/Hibbing 2004a; Smith et al. 2007; Hatemi et al. 2008), and publishing “state of the art” introductory review essays (e.g., Hibbing/Smith 2007; Alford/Hibbing 2008). In Hibbing/Smith 2007 the authors cast themselves in an insurgent role, lamenting what they see as the continuing isolation of political research from developments in the biological sciences. They note the importance of natural science research to other social sciences, and contrast it to the loss of the “Politics and the Life Sciences” organized section of the American Political Science Association due to low membership (ibid., 6–7). Alford/

Hibbing 2008 opens with a similar lament: “Original empirical research connecting biology to politics is disappointingly rare in political science.” (ibid., 184; see also Somit/Peterson 1999, 39–40). The field is awash in “incoherent environmentalism” (Hibbing/Smith 2007, 9)

Hibbing and his collaborators are certainly correct to claim that evolutionary theory and biology have been relatively neglected in the field, and to suggest that many of their political science colleagues are skeptical, if not suspicious, of mixing the life sciences with the study of politics. But there is a curious lapse in their account of the extent to which biology has been absent from the discipline. Even as they take inspiration from a number of writings of non-political scientists, they virtually ignore the evolutionary work of a very prominent rational choice political scientist: Robert Axelrod of the University of Michigan, Macarthur (“genius”) award recipient and recent past president of the American Political Science Association. Axelrod, the author of *The Evolution of Cooperation* (1984), developed his “tit-for-tat” game-theoretical argument in collaboration with the biologist W.D. Hamilton. Hamilton was not just any biologist: he was the insect ethologist and mathematical gene theorist who participated in, and did more than anyone else to inspire, the first wave of sociobiology in the 1960s and 1970s.³ In his article co-written with Axelrod for *Science*, also titled “The Evolution of Cooperation” (1981), the subject is clearly biological evolution and co-operation as a biologically evolved trait. Considering this biopolitical scholarship, plus a continuing career of work in biology and biological political science (including Axelrod et al. 1990; substantial parts of Axelrod 1997; Riolo et al. 2001 and 2002; Cohen et al. 2004; Axelrod et al. 2004; Hammond/Axelrod 2006a and b; Axelrod et al. 2006; Pienta et al. 2008), it is curious indeed the extent to which Axelrod is a non-presence in the citations of the biopolitical insurgents.⁴ Axelrod is a figure who could hardly be more prominent on the US political science scene, and who collaborated a generation ago with a figure who could hardly be more prominent on the Anglo-American sociobiological scene.

In this article, I do not aim to evaluate the new bio-political science or, much less, pronounce judgment on its sociobiological precedents. Nor will I compare the relative merits of Axelrod’s conditional cooperation with Alford and Hibbing’s closely related wary cooperation. Nor is this any kind of comprehensive comparison of the work of Axelrod and Hibbing. My aim here is merely to examine this curious moment in the institutional and intellectual history of American political science – this apparent disconnect between a rational choice and a behavioral biopolitics – and to think through some of what it means about contrasting views of theory and social science. Axelrod and Hibbing are both products of the dominant “hard” positivist tradition in American political science. They are also, I argue, both firmly in step with the 19th century origins of Anglo-American social science as a science of government. But when it comes to theory, Hibbing is engaged in a surprisingly non-positivist search for the ultimate foundations of social and political life. Axelrod, on the other hand, is a positivist theorist exploring the scientific potential of a particular heuristic. Although it overlaps the economics of consumer behavior, this rational choice heuristic at its most rigorous takes interaction seriously as a generative site for study. In the final analysis, Axelrod’s is a baldly instrumental policy science, and Hibbing’s a metaphysical quest (at least from a positivist point of view and, methodologically if not theoretically, Hibbing is very much a positivist). The frustrations of this quest have already produced significant, but relatively unmarked shifts in Hibbing’s orientation. These shifts indicate a kind of faltering in Hibbing’s work that Axelrod’s never suffers.

What do these different practices of theory mean for a philosophical and historical comprehension of political-scientific practice? I understand Hibbing’s struggle and Axelrod’s relative

ease through a long view of the discipline that begins from premises that will strike some readers as controversial. The project of political science is not about representing a political world that somehow exists apart from it. Instead, like all social science, political science is part of what it studies and even, to a certain extent, performs what it studies. Social science remains what it was quite explicitly for John Stuart Mill, contemporary positivism's most important nineteenth-century forerunner: a moral science, and a major development in the art and science of government (Mill 1974 [1843], 875–878). As Timothy Mitchell writes, too many practitioners and critics assume “that the work of social science is to represent a material world external to itself”, when we should suppose instead that social science “operates from within the sociotechnical world, not from some place outside it” (Mitchell 2007, 244). Social science is central to projects of techno-politics (Mitchell 2002).

What do I mean by techno-politics? The British nineteenth-century science of government frequently analogized itself to the anatomy and physiology informing medical practice, serving an art that attempted to supplement or substitute for political agency by refiguring as patient the medieval metaphor of the body politic. Techno-politics is this new utilitarian and therapeutic art of government.⁵ The turn to scientific government with its rule of experts informed not only the rise of the social sciences but what we now call the life sciences as well (these sciences were continuous in the work of Mill, Charles Darwin, Alexander Bain, Herbert Spencer, and their contemporaries). What these sciences began by sharing was a certain vision of human materiality: a nature, culture, or nature/culture that makes up the identities of the people who populate it, that underlies their actions and interactions, and that is either more or less subject to manipulation and improvement based on a systematic understanding of its causes and effects. Hibbing and his co-authors, in their attraction to political psychology, take this monadic nature-culture for granted. What is remarkable is the extent to which Axelrod, despite his techno-political orientation, doesn't. And it is this rigorous parsimony, finally, that makes his biology so alien to the new biopolitical scientists. One significant consequence of this is that, even as Axelrod wants to displace politics with governance, he resists the further anti-political seductions of the raciological trope of character.

Tim Ingold effectively captures this raciological trope in his critique of the dualisms that haunt his own discipline of anthropology, which has most fully developed the conceptions of “biology” and “culture” that live on in biopolitical and other sociobiological accounts, and in the work of some of their critics. He identifies what each side of the biology/culture division shares with the other: each posits a kind of essence, transmitted genealogically through descent on the one hand, or social learning on the other (Ingold 2008; see also Ingold 1995). This is true in all of the phases of anthropology's vexed relationship to biology and culture, down to the latest sophisticated “interactionism”, which simply posits two channels of genealogical transmission and maintains the nature/nurture divide it purportedly transcends by pushing it underground. What is valuable about Ingold's account for my purposes is how it identifies a problem that runs through much social science, and is particularly pronounced in the work of Hibbing and his colleagues: the problem of a dogged focus on character. For J.S. Mill, “Ethology [i.e., the science of character] [...] is the immediate foundation of the Social Science” (Mill 1974 [1843], 907). Character is both the essence of an individual (or group) and a genealogical product; it grounds the social and is itself to be studied as an effect of various causes for the possibilities and limits it poses for an improving art of government. What follows is a reflection on the different ways that Hibbing and Axelrod relate to theory, techno-politics, and this continuing feature of especially Anglo-American social science.

III.

Hibbing has built a career as a rigorous practitioner of empirical social science. He has combined a range of methodological approaches, but they all tend to center on the careful analysis of data; admirably, he and his collaborators usually acquire their own data rather than rely on sets prepared by others. A scholar of Congress and US public opinion, his work immediately prior to the turn to biology treated public attitudes towards government. In *Stealth Democracy*, he and Elizabeth Theiss-Morse (2002) present a study of American political attitudes that reveals a broad distaste for politics and apathy about policy outcomes, with people's limited interest and involvement provoked mainly by perceptions of process, and in particular by the resentful perception that political elites are taking advantage of them (see also Hibbing 2002). In its firm final sentence, the book illustrates its place in the mainstream of the tradition of scientific government described above: "American politics will not be improved by pretending the people are something they are not; it will be improved by first determining the people's preferences and then initiating the delicate process of molding democratic processes to suit people's preferences while simultaneously molding people's preferences to suit realistic democratic processes. (Hibbing/Theiss-Morse 2002, 245)

Hibbing's first biopolitical collaborations are with John Alford. In two 2004 articles they introduce "wary cooperation" by way of evolutionary psychology, an enterprise based in psychology departments that has proven to be the most widely popularized of the new sociobiologies. Evolutionary psychology nicely fits the bill for Hibbing's search for a theory that could give him an account of the "ultimate causes" of the attitudes and preferences that he found as a student of American political behavior. Its emphasis on adaptation puts it squarely in the mainstream of neo-Darwinism, but its focus on the mind (a rather un-biological object) and its universalism (when natural selection presupposes variation) are less mainstream. Evolutionary psychology approaches the human mind as a kit of adaptations, many of which were acquired during the long hunter-gatherer Pleistocene era; it is thus a largely speculative endeavor that then looks mostly to laboratory tests of sometimes counterintuitive predictions for its empirical confirmation. This is the script that Alford and Hibbing follow in their first biopolitical publication; although largely successful, the authors are forced to acknowledge that their findings could of course be due to causes other than the ones they specify (2004a, 73). Hibbing's dilemma is on display: he wants theory, by which he means an account of ultimate causes; but the more "theoretical" he gets, the farther he is forced to stray from his moorings in empirical science. And this accounts for his turn to a very different approach in subsequent work: to the twin studies methodology of behavioral genetics.⁶

What kind of ultimate causes of political phenomena do Hibbing and his colleagues seek? They seek the genesis of human nature, or character, in all its similarities and differences. Even as they consistently deride concerns about genetic determinism, noting that "(g)enes leave plenty of room for a shifting environment (or developmental stages) to change phenotypic behavior" (Alford/Hibbing 2008, 193), they remain convinced that biology takes us "deeper" (ibid., 191), that biopolitical research into these co-determinations is about assessing "the fundamental links running from genes through the brain to behavior" (ibid., 186). In the deep, material causes of similarities and differences in individual character, then, lie the secrets of politics. Convinced in the heady beginning that they had discovered the characterological origins of politics in "wary cooperation", a near-universal human trait complete with evolutionary/genetic etiology (Alford/Hibbing 2004a, 2004b), Hibbing and his colleagues have since become more cautious. Now the

phrase is dropped and people are evidently more different (Smith et al. 2007), and it appears that such a characteristic might be demoted in relevance anyway if as an “interpersonal” or “social” temperament it is not found to correlate with “political” attitudes (Alford/Hibbing 2007). Another route to getting closer to those political attitudes might reside in finding a common genetic foundation for them with the physiological responses with which they do appear correlated (Oxley et al. 2008). This work is underway, with “promising” preliminary results, using the techniques of molecular genetics to find the chemical basis for politics qua phenotype (Alford et al. 2008b, 795). Since his biological turn, Hibbing’s motivation in every study appears to be to try to make progress in getting down to the genotypical bottom of observed phenomena.

In 2005, Hibbing, with John Alford and Carolyn Funk, published “Are Political Orientations Genetically Transmitted?” (Alford et al. 2005). Here the authors did not generate their own data, which is understandable, given that twin studies rely on highly specialized samples.⁷ In what is primarily a contribution to the literature on political socialization, they compare monozygotic and dizygotic twins on a number of indices to conclude that “genes”⁸ are almost as responsible for political attitudes (as opposed to political affiliation) as “environment”. The article produced significant controversy, much but not all of it centered on the reliability of the twin method (see Charney 2008a and 2008b; Beckwith/Morris 2008; and Alford et al. 2008a and 2008b).⁹

Behavioral genetics’ analysis of variance is, remarkably, a research program with some interesting parallels to behavioral political science,¹⁰ down to its vulnerability to accusations of being, in Hibbing’s formulation, atheoretical. If theory is supposed to be all about causation, then the approach seems particularly ill-suited to Hibbing’s pursuit (see Lewontin 1974). And there is nothing in the method that tells us what kind of thing is being inherited with a high heredity score: it could just as easily be another kind of developmental resource besides a sequence of DNA. To their credit, Hibbing and Alford acknowledge this latter weakness of twin studies – that they “provide no information on the identity of the specific genes or biological systems leading to a given phenotype” (Alford/Hibbing 2008, 194) – but even as they do they remain confident that such research is the first step on the path to findings that are supremely relevant to political science. Alford et al. 2005 begins with “modern behavioral genetics”, referring to research developments in the biochemistry of behavior (*ibid.*, 154), but the authors proceed in their exchanges as if the decidedly correlational story in their (quantitative-genetic) study will “seamlessly” (Alford et al. 2008a, 323) map onto the more plausibly causal story in others’ (molecular-genetic) laboratories. And they proceed as if these laboratories might not take us even further away than twin studies already do from figuring out anything about politics.

At first blush, the complaint of Hibbing and his colleagues couldn’t be clearer. Naturalist explanations are consistently pushed out of political science – because of fear, prejudice, or ignorance – and they want to bring them in. And any reader who has considered the untenability of the nature/culture divide and related dualisms can certainly sympathize. But a closer look raises questions. Is the problem that political science engages exclusively in “environmental” explanations that are, by definition, only part of the story? Or is it that political science doesn’t explain at all, because it lacks the “theory” that (only?) the biological can provide? As cautious defenders, Hibbing and his colleagues take the first position, ever ready to promote the latest sophisticated interactionism when talk turns to “genes v. environment”. But as caustic critics, they question whether anyone who isn’t talking about biology can be explaining anything – even part of anything (these interlocutors lack theory). This slippage feeds the suspicion that they are problematic reductionists of some kind. And in the two forum exchanges on “The Genetic Basis of Political Attitudes” this is a theme voiced by their critics.

Reductionism per se is not my main concern, but it is related to my questions about Hibbing's views of theory, social science, and character. And so a couple of brief questions and comments related to reductionism are in order. First, how is it that a repeated critical assumption of this biopolitical work even makes sense: that a non-biological account of politics, whether more or less explanatory, is somehow "environmental"? Applied to staples of political science explanation, the genes/environment dichotomy from behavioral genetics merely appears to be a kind of category mistake. This is particularly clear when we consider prominent non-psychological accounts of political phenomena. How, for example, does Duverger's Law (plurality voting rules favor two-party regimes) constitute an environmental *or* genetic understanding of party systems? No one denies that parties are made up of people, and that people carry their genomes with them when they do what they do. There is a strong whiff of what Eric Turkheimer (1998) calls "strong biologism" in the biopolitical literature; as Charney (2008b, 341) analogizes, rejecting a quantum-mechanical etiology of heart disease as unhelpful doesn't make one a dualist; this rejection is perfectly consistent with a materialist understanding of the heart. What Richard Francis (2004, 94–95) calls the "misguided materialism" that marks much sociobiology also seems present here; the biological is implicitly conceived as a kind of more really real substrate, even though studies showing dramatic physiological effects (including genome-level effects) from, say, changes in social status might suggest that we should attribute a profound materiality to social relations. Misguided materialism, which is exhibited whenever Hibbing and colleagues lapse into metaphors of depth or finality, ironically reintroduces the very dualisms that sociobiologists say they are trying to eschew.¹¹

One peculiar moment in their exchange with critics is Hibbing and collaborators' insistence that a social response to an individual's physique is still genetic, because genes shape the physique and the physique shapes the response (Alford et al. 2008a, 322). Critics challenge this as a "Geneticization of Environment" (Beckwith/Morris, 790f.); but this critique might only reinforce, by declaring misapplied, a presupposed dichotomy. We should note instead what is actually most telling about this move, i.e., how it makes interaction – what one would think would be the very stuff of politics – into a characterological effect, and an effect emanating from one party at that. When Hibbing and his colleagues talk about the distinctiveness of politics, and they do, it is always to specify it as a dependent variable, distinct from, for example, the psychological and the social. They note that behavioral geneticists and others have so far only focused on the foundations of economic and psychological, but not political, phenomena (Alford et al. 2008a, 326). We still always need on this view to look to characterological origins, because everything people do has such origins and only such origins, whether "genetic", "environmental", or both. But what about the importance of politics as an independent variable (in e.g. Duverger's Law)? Such a variable, it appears, either has no place, or could only be superficial. And an explanation relying on it wouldn't really be theoretical, according to the dogma that theory looks to ultimate causes, and that ultimate causes are found in the depths of character. We will see that the rational-choice scholar Robert Axelrod throws into relief just how un-political Hibbing's approach to theory and social science is.

IV.

Axelrod's early work leaves little doubt as to his techno-political orientation. In his contribution to a workshop on "The Place of Policy Analysis in Political Science", Axelrod explicitly invokes

“the utilitarian conception of policy science”, beginning with his title, “The Medical Metaphor.” The first sentence reads: “Policy science is aimed at improving human welfare, just as medical science is aimed at improving health.” (Axelrod 1977, 430) In this short piece, Axelrod puts forward a concise manifesto: policy science must be interdisciplinary; it must engage in fundamental as well as applied research, but studies can still often be gauged as more or less useful; science aimed at improving welfare is no less scientific for it, as it demands objectivity to determine what works and what doesn’t; and policy science should aim at prevention as much as at demanded cures. The biggest difference from medicine comes in the form of much less consensus about goals. Here one might take the opportunity to acknowledge a place for democratic politics: for consulting with experts over means, but for popular disagreement, deliberation, or struggle over ends. Axelrod’s approach is not, however, to let this contingency stand. Instead, the analogical failure between medicine and government subverts a second group of experts: “analysis of policy must include not only a scientific study of the consequences of alternate choices, but also a humanistic study of what should be sought and why.” (ibid., 432)

In Axelrod’s first book, *Conflict of Interest*, the contours of his own contribution to policy science and expert rule can already be seen. And we find already here a very different understanding of theory from Hibbing’s. Although Axelrod nods to an ideal of the integrated explanation of behavior, he is clear that this would involve multiple *theories*; theory in the singular is a mode of proceeding that necessarily makes *ceteris paribus* assumptions about what it studies (Axelrod 1970, 8). Theory is never confused with any foundational way of the world; instead it is a particular way of seeing, a particular way of making sense of things that might be more or less generative. Although the proximate tests of that generativeness are formal, the ultimate tests are empirical, and finally practical. In keeping with the rigorous twentieth-century positivism of rational choice’s self-styled scientization of behavioral political research (Riker 1962, 6–7), Axelrod believes he is doing formal work for the purposes of producing testable, falsifiable hypotheses (Axelrod 1970, 6, 189). On top of this, it is clear from his opening discussion of a scene from Mark Twain’s *Tom Sawyer* that there is a practical upshot to the work: mutually destructive conflict of interest can be reduced by molding the preferences of one or more parties to it (as Tom does when he makes fence-whitewashing attractive to his friend Ben).

Hibbing and colleagues make much of the assertion that rational choice theory assumes self-interested characters and takes preferences for granted; they want to use biology to ground, modify, or refute self-interest, and to explain the reasons why actors have the preferences they have. It is certainly true that some rational choice theorists take a kind of ontological approach to their theories, making them fully representational of decision-making processes. But we can see already from his earliest work that this is not the case with Axelrod. What he is interested in is a heuristic, and in applying his heuristic not to action but to interaction.¹² Thus, the “basic theme” of *Conflict of Interest* is not the behavior of elements qua elements; it is instead structural and relational: “the amount of conflict of interest in a situation affects the behavior of the actors and thus the outcome.” (Axelrod 1970, 5)¹³

In his study of games and systems Axelrod has been interested, at least since 1981, in biological as well as social systems. Richard Dawkins – neo-Darwinist evolutionary theorist and perhaps the world’s most famous supporter of sociobiology – takes credit for suggesting that Axelrod get in touch with W.D. Hamilton, who was, unbeknownst to Axelrod, a colleague at the University of Michigan (Dawkins 1989, 214). Axelrod was inviting submissions of decision rules for one of his iterated prisoner’s dilemma game-theoretical tournaments. The result of their collaboration was “The Evolution of Cooperation” (Axelrod/Hamilton 1981); Axelrod’s book-length

treatment of his tournaments, results, and analyses incorporated and took the title of this prominent article (Axelrod 1984). And Axelrod has since published a substantial amount of other work that moves between politics and the life sciences. But what kind of work is this?

According to the E.O. Wilson's *Sociobiology: The New Synthesis*, "altruism is the central theoretical problem of sociobiology" (1975, 3).¹⁴ Briefly, it is no coincidence that some of the most prominent *biologists* in this tradition¹⁵ are students of insect behavior, as insects are among the most social of animals (Hamilton and Wilson were insect ethologists).¹⁶ The reason that insect ethologists in the 1960s and 1970s were puzzling over altruism is the following: altruism in biology has a very specific meaning – the sacrificing of one's reproductive fitness to the advantage of others – and, from the perspective of neo-Darwinism, it was hard to make sense of the eusocial behavior of many insects. This was because neo-Darwinists had come to firmly reject the group selectionism of earlier theorists, convinced that to do this was to renew the rigors of Darwin's own theory of natural selection; according to them, the "struggle for existence" – which, after all, persists not only among but within species – did not allow for individuals' sacrificing traits to be adaptive.¹⁷ Hamilton was part of a neo-Darwinian movement that shifted perspective from the individual organism to the gene (which was of course unknown to Darwin), allowing "kin altruism" (where kin are defined as sharing genetic material) to be adaptive for this "immortal replicator" (see Dawkins 1989 [1976]).

In its empirical applications, this theoretical work can articulate with the same traditions of quantitative genetics and population biology that inform the twin studies methodology that Hibbing and his collaborators have relied upon for their most prominent work. The reason that such research could be thought to confirm or disconfirm sociobiological theory is that the instrumental genes of studies of living populations are unproblematically identified with the molecular genes of bio-chemical assay of individual organisms; the latter are said to "code for" phenotypes (in what is increasingly allowed to be a "complex" or "interactive" process) of the sort that the living populations exhibit. On the biochemical side, then, essentialisms similar to Hibbing's are often assumed: here a too-robust informational metaphor allows the gene to be both sovereign and immortal. Its code is said to command, whether the commands are successful or not. Thus context is allowed its profound effects, but code's very construal as text lends it a kind of unsupported ideal reality and centrality even absent its context.¹⁸

Genes conceived in this way are the units of natural selection for the stream of neo-Darwinism that made altruism the central problem of sociobiology; also important to this stream is an insistent adaptationism that starts from the presumption of the functionality – sometimes even the optimal functionality – of selected phenotypes and thus their corresponding genotypes. Game theory proved to be a particularly fruitful way to "reverse engineer" the presence or absence of traits in populations. Hamilton's kin altruism could account for the presence of traits allowing substantial cooperation, but not for enough of what seemed to be apparent in many living populations. Following the work of the sociobiological anthropologist Robert Trivers (1971), the success of tit-for-tat in Axelrod's iterated prisoner's dilemmas suggested the potential for significant cooperation to evolve among unrelated players; this conditional cooperation could represent an "evolutionarily stable strategy" (Maynard-Smith 1982). The sociobiological lesson of this game-theoretical research is that a significant tendency to give costly aid to potential reciprocators could be fully adaptive (when linked to a tendency to shun non-reciprocators); co-operators would not be overwhelmed by defectors. Axelrod's subsequent research has gone farther down this road, with particular attention to the generation of computer models that simulate the evolution of stabilizing emergent properties in increasingly complex games of multiple

players (see Axelrod 1997). We learn that modes of cooperation can even survive in one-off, rather than iterated, encounters. Here the capacity of “tags” (arbitrary tokens of similarity that stand in for something like ethnicity) to suffice in generating cooperation without reciprocity has been explored (Riolo et al. 2001; Axelrod et al. 2002) and, apparently, discriminatory altruism has successfully evolved once tags have been combined with “viscosity”, which shapes the interaction topology to introduce a degree of localism in encounters (Axelrod et al. 2004). Whereas the initial work on evolution was in a fully individual selectionist mode, the subsequent, more advanced biological game theory has demonstrated that possibilities of a kind of group selection can emerge even from individualist, agent-centered models when rendered with sufficient complexity (see Sober/Wilson 1998, 55–100).

Throughout, Axelrod’s work has remained consistently and unabashedly techno-political in orientation. *The Evolution of Cooperation* builds to section IV, “Advice to Participants and Reformers”, the separate chapters of which, “How to Choose Effectively” and “How to Promote Co-operation”, precisely map the territories respectively of classical utilitarian ethics and politics (compare Bentham 1983 [1834] and Bentham 1970 [1789]). Just as in *Conflict of Interest*, policy promotes welfare by transforming individual interests; policy works to “change the payoffs [...] to make sure that when individuals do not have private incentives to cooperate, they will be required to do the socially useful thing anyway” (Axelrod 1984, 133). In more recent research, Axelrod has explored the barriers posed by conceptions of the sacred to this kind of technopolitical manipulation, and possible ways to overcome them in pursuit of peace (Atran et al. 2007).

And in some of his latest work, Axelrod crosses back from medical metaphor to medicine. His articles on cancer advance a reconceptualization, based on game-theoretical predictions about cell organization and behavior, that raises exciting possibilities for new therapeutic approaches (Axelrod et al. 2006; Pienta et al. 2008). Quite reasonably, Axelrod himself sees this latest interdisciplinary work as bringing insights *from* political science *to* biology (Axelrod 2008). But perhaps, to some degree, he has been doing this all along with his version of biopolitics. It is here, I think, and in his understanding of theory and eschewal of character, that we can begin to see why it is that our new biopolitical scientists have so much trouble recognizing him as a fellow traveler.

V.

Why do Hibbing and his collaborators give so little recognition to Axelrod’s achievements? One reason is simply their naming of rational choice as an adversary. We have seen, through the discussion of neo-Darwinism, that there is a great deal of irony here. After all, there is a significant extent to which contemporary evolutionary theory *is* rational choice: an extent to which these are not two different things, but in fact the same thing. There is, however, something else going on here. Axelrod himself says that what he is doing differs from sociobiology. He writes: “Sociobiology is based on the assumption that important aspects of human behavior are guided by our genetic inheritance [...] Perhaps so. But the present approach is *strategic* rather than *genetic*.” (Axelrod 1984, ix-x, emphasis in original) In his work, Axelrod is exploring the various potentials of a heuristic. He is not making foundational claims.

We saw that early on Axelrod defined theory, and model-building, in a rigorously positivist way. His statement on explanation expressed a quasi-Weberian ideal: at best, we would take the results of different theories and make them additive, knowing that each approach is partial and

perspectival, bringing certain things into view and obscuring others. Investigations must not proceed with either assumptions of a naive realism, or with the assumptions of a two-world frame, as if good investigators are those who pry off the phenomenal to reveal the noumenal. Theory, for Axelrod, is a pattern or way of seeing that organizes facts and tries to generate successful predictions. It can quite reasonably be argued that his particular way of seeing is not only limited, as all are, but distressingly repetitious, even as it might prove productive for his purposes. And one can imagine a rigorous adherence to the rational choice approach getting in the way of what Hannah Arendt, in the preface to the first edition of *The Origins of Totalitarianism*, calls comprehension: “the unpremeditated, attentive facing up to, and resisting of, reality – whatever it may be” (1968 [1951], viii).

Hibbing’s complaint about rational choice is a very different one. The problem is that it is not a *theory*. And Axelrod’s positivist restraint illustrates why it is not. For Hibbing, patterns limited by ways of seeing are not the path to understanding, nor is Arendt’s call to try to suspend some of our patterns of thinking in the presence of events, to discard the premeditations with which we too often organize the facts, and to face them anew. Instead, Hibbing is tired of facing facts only to discern patterns that he thinks themselves demand an explanation. His studies of public opinion gradually disclosed something to him about the people he was surveying. But interpreting these results, generalizing from them, and telling us about them – even with nuance, insight, and the careful deployment of statistical methods – was no longer satisfying. His conclusions, as much as they relied on an expert judgment in discerning patterns and relationships, came to seem to him like mere description; they needed themselves to manifest a deeper pattern, and it is this essential reality that he sought, and found, in sociobiology.

Part of the problem with Hibbing’s quest is identified by Axelrod in his critique of a third author. In Hibbing’s search for ultimate causes “he tends to imply that any model that fails to explain everything explains nothing [...] This unsound rhetoric leads to trouble” (Cohen et al., 50). Part of the problem lies in the mode of explanation Hibbing has been seeking all along: he seems to be oriented in all he does by the framework of political psychology. In the work of Axelrod, we have seen that the turn to psychology is not entailed even by strictly individualist, agent-based models of explanation. Such explanations might look, for example, to the variable structures of situations, rather than to the variable characters of individuals, for answers. Put differently, in the hands of its most sophisticated practitioners rational choice is not, as it is for Hibbing and for many critics, a psychological theory *manqué*. To his credit Hibbing doesn’t seem to be driven to sociobiology, as some are, in order to account for findings that only appear anomalous from the standpoint of certain economic assumptions about behavior (see e.g. Ostrom 2000). And to his credit the sociobiology that most attracts him is a reasonably sophisticated one: the multi-level selection version developed by Sober and Wilson (1998), among others. But Hibbing seems rather blithe about the theoretical challenges faced by such an approach. And it’s clear that, despite the “multi-level” in multi-level selection, he is focused on looking deeper into individuals for answers, and what he expects to find at the deepest levels are genes, or maybe proteins.

I might have placed “proteins” in quotation marks, because I’m not sure what Hibbing and Alford are talking about in their initial breathless presentation of multi-level selection theory and wary cooperation. “The Origin of Politics” appeared in 2004 in the new journal *Perspectives on Politics*; it is questionable whether such an article could have found such prominent publication before this disciplinary opening. Highly speculative, it presents an evolutionary landscape of collectives within collectives of entities subject to conflicting selection pressures, including, in

what must be a slip, “the complex proteins that make up genetic material. At this deeper level, it is the complex proteins that are selfish, and their survival machines – the genes – may behave in ways that seem highly inconsistent with selfishness” (Alford/Hibbing 2004b, 708). This “Russian doll analogy” continues all the way up to political parties and nation-states, in an only occasionally clear approximation of multi-level selection theory.¹⁹ The modal human product is the wary cooperator. Wary cooperation is “drawn from the work of leading scholars in evolutionary psychology and experimental economics. [...] Humans are cooperative, but not altruistic; competitive, but not exclusively so.” (ibid., 709)

The authors are quick to turn to the policy implications of their research. Their suggestions recall Hibbing/Thies (2002). “Reformers would do well to realize that people do not wish to be in control of the political system; they only want those who *are* in control to be unable to take advantage of their positions.” Thus: “Current American foreign policy might be improved [...] if decision makers realized that, like Americans, people in Afghanistan and Iraq do not crave democratic procedures. Kurds simply do not want to be dominated by Sunnis; Sunnis do not want to be dominated by Shiites; Uzbekis by Tajiks; and Tajiks by Pashtuns. People [...] really just want to avoid being victimized by a more powerful group.” (Alford/Hibbing 2004b, 713)²⁰

Hibbing and Alford fill out their picture of the potential of biopolitical research with discussions of twin studies, autism, and molecular genes. Here they make so bold as to say, on the basis of twin studies, that “*a predisposition to conservatism is genetically heritable*” (Alford/Hibbing 2004b, 714, emphasis in original), and they gesture towards “a deeper explanation for the durability of a characteristically southern orientation toward politics in the relatively closed breeding ground of much of the traditional white South” (ibid., 716).²¹ The authors begin their conclusion by disavowing determinism and even biological priority, only to go on to suggest that theirs are “original causes” underlying “more proximate environmental factors”. And finally, they conclude as follows: “Evolutionary theory has the potential to render obsolete our intradisciplinary conflicts over approach, method, and theory. [...] It is a true theory of the origins of behavior and as such provides a basis for bringing together the remarkably diverse and useful ongoing research in political science and beyond.” (ibid., 718)

Hibbing’s search had been for “true theory”, and here he had apparently found it. But, strangely enough, in the 2008 manifesto, “The New Empirical Biopolitics”, theory of any sort is virtually missing altogether: there is almost no talk of natural selection, much less the evolution of politically relevant traits.²² It is unclear whether the incoherent biologism (to coin a phrase) of the 2004 article has been maintained; bits of it appear in recent responses to critics, but there are also signs that Hibbing, in his empirical rigor, is willing to go where his particular findings take him – and that is likely to be away from “a true theory of the origins of behavior”. Although the criticisms are ostensibly leveled at predecessors, “The New Empirical Biopolitics” really constitutes a kind of auto-critique when it suggests that biopolitics founders on grand theorizing, which leaves “insufficient variation in central dependent variables” (Alford/Hibbing 2008, 184). But without a very grand theory indeed Hibbing, on his own definition, has no theory, and his new empirical path runs great risks of leading to all of the frustrations of his old one. The eagerness to link twin studies to “wet” genetics to confirm the discrete characterological bases of political “phenotypes” does, however, remain strong (Alford/Hibbing 2008, 193–195). And because of the faith that “politics is generated”, i.e., ultimately caused, “by the [...] brain and the [...] genome” (ibid., 191), this now hard-won bits-and-pieces truth will for Alford and Hibbing still, slowly but surely, add up to theory, i.e., real and whole truth. The new path has technopolitical upshots – there

is talk of implications of findings for institutional design (*ibid.*, 197) – but it ultimately bypasses them for the goal of understanding (*ibid.*, 195). As Hibbing gets farther from armchair theorizing about character – as he gets closer to the accretion of particular findings that might look to others, if not him, as “just collections of relationships, little more than recapitulations of time tested correlations” (Hibbing/Smith 2007, 9) – his will to govern falters. He begins more to resemble the survey subjects that provoked him to this quest, who are uninterested in rule.

We can see now why Hibbing and his colleagues have not, for the most part, recognized Axelrod’s significant contributions to their adopted field. Axelrod’s rigorously positivist program is in line with their methodological intuitions, but incompatible with their theoretical aspirations. And when Axelrod is doing political science, he’s doing political science – when he is doing biology, he’s doing biology, even if he’s bringing insights from political science to it. Axelrod finds a certain kind of sociobiological synthesis hopelessly un-parsimonious, and he has little or no interest in character. Yet he has found that a rigorous positivism is far from incompatible with a science of government that can inform techno-political practices. The abstract theorist Axelrod proves in the end to be far more worldly, for better or worse, than Hibbing, the self-styled empirical naturalist.

My argument is by no means an endorsement of rational choice theory, far less an endorsement of positivist strictures. It is no surprise or shame that Hibbing would join, consciously or not, with post-positivist protests against Axelrod’s thin, Hempelian idea of explanation as merely prediction after the fact – and that he would value understanding over control. But I’ve shown that Hibbing’s own theoretical practice is simply not coherent: his foundational biopolitics is un-empirical and muddled, and his empirical biopolitics inevitably runs up against all the old frustrations of behavioralism and more regarding everything he values in scientific work – its replicability, generalizability, and explanatory power.

I have presented here a kind of cautionary tale – one that asks for historical sense, reflexivity, and above all a measure of humility. In five short years, the new biopolitics is somewhat adrift from its initial stated ambition to reveal to us the origin of politics. My analysis here is of biopolitics’ curious non-encounter with its rational-choice predecessors. The analysis suggests that the new biopolitics is drifting because of tensions in its practice of theory. Roughly, this research program combines the grand naturalist and governing ambitions of nineteenth-century positivism with the methodological strictures of twentieth-century positivism. Its rational-choice foil, meanwhile, is more self-consistent in its technicism and skepticism. This greater care in theoretical practice better positions rational choice for disciplinary borrowings and crossings. And in spite of the evident constraints of its economic approach to politics, rational choice, at least at its theoretical best, is better able than the new biopolitics to guard against a retreat into character – to guard, that is, against losing what is distinctively political in the study of political science.

But the tale is cautionary in another way as well. Because method is more important than theory in US political science journals, and because characterology carries such authority in a broader intellectual and political culture, the new biopolitics will, despite theoretical trouble and retreat, enjoy continuing success. Its relatively superficial encounters with rational choice will leave both unscathed, focusing on substituting a thick and heterogeneous individualism for an apparently thin and homogeneous one.²³ Through these encounters, each perspective will be able to remain largely unreflective about its historical location in a continuing nineteenth-century project of social science, in which investigations are tied to a presumption of expert rule – and in which, on one side at least, the specter of race lives on.

NOTES

- 1 I received much help with this article, yet any remaining faults are very much my own. Many thanks to Elyse Conklin, Steve Downes, Colin Klein, Khalid Madhi, Sophia Mihic, and Nora Willi, to Karin Bischof and an anonymous reviewer, and above all to Thomas König for his encouragement and aid.
- 2 A European audience is more likely to associate biopolitics with historical and contemporary struggles over bioethics and biotechnology, the politics of health and longevity, and/or elements of the work of Michel Foucault, Giorgio Agamben, and others. For a comprehensive guide and sustained theoretical reflection see Lemke 2007.
- 3 Alford/Hibbing 2004b mentions Hamilton (711), with no connection to Axelrod. Hamilton is the first neo-Darwinist listed by Somit/Peterson as an inspiration (1999, 44f.); they do not cite Axelrod at all. Hamilton's two-part article on "kin selection" for the *Journal of Theoretical Biology* (1996 [1964]) is widely credited with launching the sociobiological movement (see e.g. Wilson 2006, 315ff.).
- 4 Only Axelrod 1984 is substantively engaged, and only once briefly in an endnote at Alford/Hibbing 2004a, 65. Citation of any other work is limited to Alford and Hibbing 2008, 185, which mis-cites Hammond/Axelrod 2006b as "Axelrod and Hammond".
- 5 Although inspired by Mitchell 2002, this definition does not necessarily match his usage.
- 6 Hibbing and Alford first reject the notion that these are quite different approaches, associating any sameness/difference divide between evolutionary psychology on the one hand and behavioral genetics on the other with dated "popular misperceptions" of "casual observers" (Alford/Hibbing 2006). But in Alford/Hibbing 2008 they write: "biopolitics has to a substantial degree followed the lead of evolutionary psychology in focusing on the role of human genetics and brain physiology in establishing broad human behavioral universals [...]." The problem here is "insufficient variation in central dependent variables": thus an alternative reliance on, for example, the twin studies approach of "behavior genetics" can provide a "decidedly empirical variant of biopolitics" (184–85 and *passim*).
- 7 Hibbing and Alford report that they are currently engaged with collaborators on original twin survey research, in a National Science Foundation-funded study expected to yield data by late 2009 (Alford/Hibbing 2008, 186).
- 8 The word "gene" is used in different ways within different research traditions. There is the instrumental or preformationist gene that is the shorthand unit for heredity-talk, and the material or developmental gene that is the shorthand unit for biochemical-talk (Moss 2003), and there are different ways to conceptualize especially the latter depending on what kinds of investigations are performed (Griffiths/Stoltz 2007). Hibbing and his colleagues write as if all things going by the name of "gene" are one-and-the-same and presumptively foundational, as researchers at the height of classical molecular genetics – quite some time ago, that is – thought they must be. Newer research, from the emergence of epigenetic findings and other developmental challenges to the several ways that expectations have been defied in the mapping of genomes, does more than just multiply sites and agents of interaction (see e.g. Fox-Keller 2000; Snyder/Gerstein 2003; Gerstein et al. 2007). In light of post-genomic molecular genetics, it might make most sense to speak not of genes but of "things you can do with your genome" (Griffiths/Stoltz 2007, 102). One of Hibbing and colleagues' most annoying failings is their continual emphasis on the datedness of others' views. And so they call the criticisms they encounter of twin studies relevant to "the behavioral genetics of a quarter-century ago" (Alford et al. 2008b, 793), in a way that suggests that what they are doing is something different, when, despite excursions into contemporary molecular literatures, the actual *evidence* they present in Alford et al. 2005 (and in the Hatemi et al. 2008 follow-up) is well within the confines of a decades-old quantitative genetic tradition (statistically refined over the years to be sure, but the core of the methodology, its analysis of variance, goes all the way back to 1918 – Griffiths/Tabery 2008, 333). The problem, needless to say, is not that this is old, but that they are giving the impression that criticisms we have heard rehearsed in the past somehow don't apply to their work.
- 9 The important controversies about heritability studies are less technical and more theoretical than the bulk of these exchanges suggests. The findings are real, but what do they mean? See Downes 2004 and Turkheimer 2008 for introductory critical accounts from within philosophy of biology and behavioral genetics respectively.
- 10 Such similarities are acknowledged by Hibbing and Alford themselves; see Alford/Hibbing 2008, 186.
- 11 For more rigorously non-dualist naturalisms, see for example Oyama et al. 2001 and Damasio/Damasio 2006.
- 12 Of course, there is bound to be some slippage from heuristic to representation (Mihic et al. 2005, 489).
- 13 Axelrod did flirt at one point with cognitive maps (1976); but he seems to have long since abandoned political psychology and returned for good to the study of games and systems.
- 14 Note that altruism becomes a problem for 1960s and 1970s biology in a way analogous to how voting becomes a problem for contemporary political science. Behaviors that had on previous assumptions been largely taken for granted are newly puzzling because of common, broadly economic, assumptions.
- 15 Human sociobiologists are for the most part psychologists and social scientists, not biologists.
- 16 Primatologists are also important to the tradition; they have, for more defensible reasons, made significant contributions to human sociobiology.
- 17 The assumption of these neo-Darwinists that Darwin himself did not abide group selection is simply not true. Pas-

- sages in *Origin of Species* are open to a group-selectionist reading (e.g. Darwin 1964 [1859], 235–42 on neuter insects), and *Descent of Man* is quite explicit on this count (e.g., “those communities, which included the greatest number of the most sympathetic members, would flourish best, and rear the greatest number of offspring”, Darwin 1981 [1871], 82).
- 18 See Griffiths 2001 for different meanings of information, and for where this metaphor overreaches.
 - 19 It should be said that the elements and groups here are strangely chosen and ordered, and that Alford and Hibbing’s depth metaphor seems radically inappropriate to the spirit of multi-level selection.
 - 20 Note the easy slide from the priority of interest and institutional constraint to the priority of group identity, the conflation of ethno-linguistic, religious, and other markers of identity, and the elision of “American [...] decision makers” as an interest, identity, or “powerful group”. Presumably the authors think that such imprecision lacks theoretical import.
 - 21 The former claim was altered considerably for Alford et al. 2005, which also eschews talk of breeding grounds. But see Alford/Hibbing 2008, 198, which takes up speculative racilogies that are (re-)emerging in some corners of the North American academy.
 - 22 One exception – a response to anticipated criticism – is a discussion of the relevance of the rather recent appearance, in evolutionary time, of “mass-scale politics” (Alford/Hibbing 2008, 191). Another, interestingly enough, is a rare reference to Axelrod among others, for his modeling of “the implications of evolution for political variables” (ibid., 185). Rational choice has moved from being an atheoretical foil of 2004 to being virtually the only theoretical reference of 2008.
 - 23 Alford/Hibbing 2005 offers a tantalizing glimpse of a more significant encounter, not taken up in subsequent work. It goes beyond thickening and varying to propose a radical de-centering of the rational-choice subject, and it briefly acknowledges the role of formal theory in biology.

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