

Christian Smith. *The Sacred Project of American Sociology* Oxford: Oxford University Press, 2014. 212 pp. ISBN: 978-0-19-933713-8

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Christian Smith is an insider of sorts—a product of Harvard with a past at North Carolina, a member of the exclusive Sociological Research Association, who publishes in conventional journals, does survey research, and runs a research center at Notre Dame. He also, however, writes books, some of which articulate his vision of the good life, which go far beyond conventional sociology, including his genre-busting *What is a Person?* (2010), the most wide-ranging and compelling book in “sociological theory” of recent years. He has now written a reflection on sociology that brings to mind the books of the early 1990s, such as Horowitz (1993), Cole (2001), and articles like Joel Smith’s attack on the influence of postmodernism (1995). But Christian Smith has new material to deal with, and a sociological thesis: that sociology is understandable in terms of the sociology of religion as a sacred enterprise, with strong ideological commitments, and that this accounts for its present as well as past intellectual flaws. This sacred project has to misrecognize itself, he thinks, and this misrecognition provides Smith’s opening. He believes that “publicly naming and overtly embracing this sacred and spiritual project would threaten the scientific authority and scholarly legitimacy of academic sociology on which the project itself depends for success” (7). But Smith’s argument is in many ways a peculiar one, because Smith accepts, though in a confused and inconsistent way, the exact kind of committed, ideological sociology and advocacy research he complains about.

The Sacred Project

So what is the sacred project?

We might *start* by saying that sociology is about something like *exposing, protesting, and ending through social movements, state regulations, and government programs all human inequality, oppression, exploitation, suffering, injustice, poverty, discrimination, exclusion, hierarchy, constraint and domination by, of, and over other humans (and perhaps animals and the environment)* (11; italics in the original).

Or more sharply, it stands in the modern *liberal-Enlightenment-Marxist-social-reformist-pragmatist-therapeutic-sexually liberated-civil rights-feminist-GLBTQ-social constructionist-poststructuralist/postmodernist “tradition”* (11; italics in the original).

But this account would only be a start—to understand it the real need is to “uncover the more basic and determining assumptions about philosophical anthropology and normative goods that sociology’s sacred project presupposes” (7).

The philosophical anthropology, oddly, is the opposite of what sociologists have always preached: not our pervasive dependence on society, but individual “autonomy.” He acknowledges that

This may seem strange for a discipline devoted to showing how individual people are powerfully influenced, if not constituted and determined, by their social environments. But sociology’s sacred project finds a way to finesse this tension. The controlling influencer that society is, which sociology reveals, is associated with the

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constraining (oppressive, exploiting, discriminating) part of human experience, which the individual needs to resist and overcome if he or she is to be free, happy, and healthy. (14)

The evidence for these commitments is found clearly in textbooks and elsewhere. As Smith notes, “best-selling sociology textbooks consistently. . . convince—I would say indoctrinate—students into accepting and ideally embracing its particular assumptions, commitments, perspectives, and mission. Far from being merely informative, balanced presentations of what sociology has learned scientifically about society, these introductory textbooks function as recruiting tools and re-socialization manuals for the movement that is American sociology’s sacred project . . .” (86). And Smith finds plenty of additional evidence for the idea of a sacred project in research paper titles, and so forth. But he has an oddly ambivalent, or confused, idea that there is such a thing as “real sociological books and articles” that sociologists would be assigning if they “had any intellectual and moral integrity” (86).

This muddle is at the heart of the book, and is never resolved. Smith seems to be working with a model in which there are at least four sociologies. First is textbook sociology, which has little to do with actual sociology (he makes the good, if obvious point, that every theory textbook repeats some version of the idea that there are three basic theoretical approaches in sociology, which everyone knows to be false). So actual sociology, the second kind, is something else. But when he talks about “what sociology has learned scientifically about society” he seems to be evoking some sort of world of genuine sociological knowledge free from ideological piffle, a third sociology. But where is it? Apparently not in actual sociology, which he describes as an intellectual dead zone, “simply not very strong on broader intellectual substance,” in which anything goes. He says that “One sociologist has named the current state of affairs our ‘Pax Wisconsin’, after the large sociology department at Madison, Wisconsin, where a host of scholars work away on their different interests without much contention” (142). But the “tacit peace treaty specifying that everyone should mostly think and do whatever he or she wishes in terms of methods, theory, and intent and not suggest that what anyone else is doing might be a problem (unless he or she violates the sacred project)” has left us with a sociology which “engages in . . . few meaningful, substantive intellectual debates” and “has become less theoretically interesting and philosophically reflective over time” (142).

Then there is a fourth: ideal sociology, which apparently occasionally happens, but doesn’t sound much like scientific knowledge, and indeed sounds more like Dorothy Smith’s sociology for people than anything one finds in sociology journals. As Christopher Smith puts it

Sociology can do a lot of great value to foster that. Human self-understanding is thus enhanced through

the conceptually mediated, reasoned, theoretical comprehension of ourselves and our world, viewed through sociological lenses, in response to our personal and collective experiences over time. When sociology accomplishes that, I believe its contribution is invaluable. (186–7)

But “explaining the what’s, why’s, and how’s of human experience, institutions, and practices and, as a result, making better sense of them for the variety of people and groups involved” (186–7), what Edward Shils called the self-understanding of society, is precisely what sociology has failed to do, and indeed is effectively prevented by the sacred project itself, which imposes its own politically correct philosophical anthropology on the activity of self-understanding.

Smith is far from clear how this model of sociology relates to his critique. He accepts a basic claim of feminism, without acknowledging that it his views are in common with sociology’s main advocacy movements: that sociology is necessarily infused with “particular presuppositions, categories, interests, priorities, ethical concerns, and frameworks of explanation” and that although these “provide certain insights into some of the ways that the social world works” they are “also blinding in other ways” (147). If this is so, all sociology is essentially ideological. Smith says as much, when he comments that

Many sociologists will want at this point to evoke the Weberian line, namely that what I am calling their spiritual commitments legitimately inform what they *want* to study but do not influence *how* they study it. But that distinction is an illusion in this case. The topical interest of study and the way it is studied (and understood and explained) are not so easily separated. Weber was naïve on this point, and so are too many American sociologists. (147; italics in the original)

So why not say this and then just adopt the ideology of the sacred project? He insists that “Sociology needs to do some serious, truly open minded soul searching about its proper purpose, identity, and practices before it loses what is genuinely good in what it has to offer” (197). But this seems somewhat pointless. If the only alternative on offer is to adopt some other problematic ideology, most sociologists, on reflection, would be happy with the one they have.

Smith’s own alternative (2010) invokes the philosophy of Aristotle and “Personalism,” a philosophical-therapeutic-theological doctrine associated with Pope John Paul II, among others. Politically “personalism” was presented as an alternative to Marxism and Liberalism. What is distinctive about it is its philosophical anthropology, and particularly the idea that humans are spiritually oriented by nature. The relevance of all this should be obvious: when Smith claims that the sacred project of American sociology is rooted in an alternative

philosophical anthropology, he implies that each is on the same footing as forms of faith.¹

Moreover, there is a problem in claiming that perspectives reveal truth or are blind and then insisting that everything is constituted by presuppositions and is therefore perspectival: if that is so, where do the standards of truth and blindness come from? Either they come from within the perspective, and are themselves essentially ideological, or there are standards of truth and intellectual adequacy that allow us to judge the ideology, or at least to proceed without ideology.

A Tale of Two Studies

The philosophical confusion matters, and one sees why when Smith goes on to discuss at some length two cases of the reception of research results. The first is a study by Lenore Weitzman (1985) on the differential economic effects of divorce on men and women, was widely quoted because it suited the policy preferences of the women's movement. The second is a study (2012) by Mark Regnerus (Smith's student and collaborator, though he doesn't explain this relationship) that offended the LGBT community and produced a vast backlash and a call for a condemnation of his work. Smith tries to explain the difference in response in terms of the sacred project.

The Regnerus result in question was that parental same sex relationships were associated with certain bad outcomes for children. There was a causal arrow question that plagued the study, the question of whether same sex relations were less stable and the instability led to the bad outcomes, or had some more direct effect. There were also many other issues, of the kind familiar from survey research generally. Weitzman's study was different: it pointed to inequitable outcomes, which

could, with suitable policy changes, be remedied, though of course, there were many steps from findings to policy, and other considerations of equity that Weitzman was not concerned with. The problem with Weitzman's study was that the findings were bogus. The story as Smith tells it focuses on Regnerus's vilification and Weitzman's misrepresentations. Weitzman was exposed by subsequent research, published in the *ASR* itself, but never censured. She provided, however, many excuses for the errors and the loss of the data, and stuck to her main conclusions (1996); Regnerus was vindicated but implicitly censured. But matters are more complicated.

A fair sample of the reaction to Regnerus is in a petition to the ASA to punish him for ethics violations.

Regnerus is part of the "expert witness project" of the National Organization for Marriage, an anti-gay hate group that hosts rallies where its chosen speakers yell through megaphones that homosexuals are "worthy to death." Regnerus participates in NOM conferences where he distorts what research on gay people says, in order to stimulate his fellow religious anti-gay bigots to further gay-bashing hatred. That obviously has a negative impact on innocent gay people and on children being raised by gay people. Additionally, Regnerus's behavior in doing that violates the ASA's Code of Ethics section on Public Communications. (<https://www.change.org/p/asa-president-cecilia-ridgeway-uphold-asa-code-of-ethics-against-mark-regnerus>)

Google this controversy and one will find a great well of vituperation against this study as junk science and hate-mongering, even as a threat to human rights.

The two cases have a kind of symmetry: both are cases of advocacy research, each has issues. Each was employed in court and in public opinion to confirm a long standing conviction, in one case by a powerful group with few powerful opponents that is allied with sociology's sacred cause, the other by a weak group with powerful opponents that is opposed to sociology's sacred cause. The bloggers go into these at length, and I will not repeat them here, but one issue does need mentioning. Smith seems to think, and he may be correct, that the Weitzman case is a simple case of data misrepresentation and concealment, while the Regnerus case is one where, as the Wikipedia article on the case quotes Smith, "his article is not perfect," nevertheless "it is no scientifically worse than what is routinely published in sociology journals," and that the attacks on it are a "witch hunt," and that "Regnerus published ideologically unpopular research results on the contentious matter of same-sex relationships. And now he is being made to pay" (Wikipedia: Witherspoon: Regnerus Study. http://en.wikipedia.org/wiki/Witherspoon_Institute).

What does all this have to do with Smith's four sociologies? One needs to distinguish the study itself from the

¹ It is somewhat ironic that Smith fails to see that the sacred project of sociology might itself be rooted in religion, a claim made at great length by Vidich and Lyman a generation ago:

From the beginning social thought in the United States had its roots in Christian religion, especially Protestantism. The emergence of secular social thought out of Protestant theology may thus be regarded as a transmission of religious values into civil society. Protestant thought fosters a secular attitude toward the world—in Weber's terms, it is inner-worldly rather than other-worldly. It is guided by a mission—that of establishing the Kingdom of God on earth. Nearly all of the major proponents of American sociological thought have hoped that sociology would contribute to forming a better world, even a utopia. Frequently they thought of their science as an instrument that would help carry forward the Christian religious promise. (1985: xi).

advocacy uses to which it was put. Smith is probably right that it was no worse than other conventional sociological studies. But this was a study, as was Weitzman's, with a specific political aim. Regnerus was testing a contentious political claim about the merits of same sex couples as parents. This would have something to do with "science" if there were some sort of science behind it—a theory or larger scientific framework, or attempt at understanding the what's, why's, and how's of human experience, as Smith puts it, that made this an interesting intellectual problem on its own. Needless to say, that was not the case. Neither was it the case for Weitzman, who had no interest in the causes of the discrepancies she reported or the experience of divorced men. If she had, she would have soon discovered her error, as her critics, who did have an interest, were able to (Braver 1998: 55–86). She was making an advocacy point. To make it stronger, she used the machinery of social science validation to present this point as legitimate research, and herself as an expert, and, helped along by the women's movement and the courts, succeeded so well that the claims were treated as true despite being false, and despite funny business with the data, including a long period of what appears to be concealment of discrepancies between the data and the reported results, and despite the inconsistency of the results with other research.

Is this stuff supposed to be, to use Smith's phrase, "scientific knowledge about society"? If so, it is a sign that the relevant distinctions have already broken down. Survey research only produces results that need a lot of interpretation, and translating these results into policy, or evaluations, requires taking a lot of inferential steps, many of which are value-laden, all of which are likely to be problematic and "political." In these cases, the interpretation came from advocacy groups, not "science." So has the criticism, which goes farther into these issues than the print material (<http://www.regnerusfallout.org/>). Like many studies with supposed policy implications, Regnerus's study was only tangentially related to the policy in question, namely allowing same sex marriages, a policy which would change the conditions for such partnerships in a dramatic way. Even if the findings were valid, as exhaustive inquiries seemed to confirm, any extrapolation from his very limited findings to this policy change was suspect, unscholarly, and unjustified. In the case of Weitzman, the same issues arose, together with the problem of misrepresented data (<http://www.acbr.com/biglie.htm>). So let us call this what it is: advocacy research.

The judge in the Regnerus case figured out that this was what Regnerus was doing:

The Court finds Regnerus's testimony entirely unbelievable and not worthy of serious consideration. The evidence adduced at trial demonstrated that his 2012 "study" was hastily concocted at the behest of a third-party funder, which found it "essential that the necessary data be gathered to settle the question in the forum of public debate about what kinds of family arrangement

are best for society" and which "was confident that the traditional understanding of marriage will be vindicated by this study." . . . While Regnerus maintained that the funding source did not affect his impartiality as a researcher, the Court finds this testimony unbelievable. The funder clearly wanted a certain result, and Regnerus obliged. (<http://www.hrc.org/blog/entry/michigan-judge-delivers-devastating-blow-to-junk-scientist-regnerus>)

This is of course the essence of advocacy scholarship, the very meaning of "Public Sociology," and the reason the ASA gave awards to Weitzman: the research served a cause, with the sociologist in an "organic" relation to the cause, to use Burawoy's Gramscian language. Weitzman was free with assertions about what was wrong with policies—no value-freedom here. Regnerus apparently didn't misrepresent the data itself, as Weitzman did, so the cases are not exactly parallel. And Weitzman was funded by "substantial grants from NSF and NIMH" (Weitzman 1996: 537), not an advocacy group, though this hardly makes the funding less political.

The Regnerus case produced outrage and a backlash. Weitzman influenced many laws. Regnerus was implicitly condemned by the ASA; Weitzman received awards. Much of what passes for sociology today also comes from advocacy communities or is motivated by normative concerns, such as inequality. There is a larger systematic reason for this: academic life and science in general have become, as a result of the funding system and the closely associated fact of the decline of the academic ethic, oriented to what users want in the short term. There are big rewards for siding with the right causes, and punishments for running afoul of them. In science studies, terms like post-normal and post-academic science are used to describe this new situation. In the philosophy of science, we find prominent defenders of this new situation, such as Philip Kitcher, who argues that scientific research should not be done if it undermines social justice goals (2000: 102–108, 2011: 182–186). The predominance of this kind of research in sociology makes much of the language we have traditionally used obsolete and irrelevant. Smith is right to point this out. But he misses the significance of the change.

Let us be explicit. The academic freedom formerly enjoyed by the social sciences was governed by a harsh Hobbesian deal: if a scholar stayed within the limits of scholarship in teaching and research, the profession would protect him or her; stray, and there would be no protection. One could get into politics and become an advocate if one wished, but the professional protection would vanish, and misrepresenting advocacy claims as disciplinary facts was a violation of disciplinary standards, and not protected. Whether one could keep one's job and make public pronouncements was a matter between you and your university employer. You kept your rights as a citizen, but, as a professor representing the university, you could not be an advocate. In the Vietnam era, professors were fired for appending their status as professors to

letters to the editor—leading to the convention of marking listings on petitions as “for identification only.”

This deal is effectively dead. Professors now have opted for a better deal: say what one likes as an advocate or expert, as long as there is a public constituency that will protect you for saying it, and preferably pay to support your research. The philosophical basis of this new deal is nicely articulated by Smith himself, at least part of the time—namely when he insists that Weber was naive and implies that all social research and thinking is based on ideological presuppositions. When the University of Colorado decided to create a chair of Conservative Thought and Policy, to provide some sort of intellectual balance, it was an implicit acceptance of this picture. And of course they fund-raised for it, from private sources.

One way of interpreting this new order is to accept it as the way things are, and try to make it fit with some sense of democratic legitimacy and neutrality—which is what the older deal achieved by segregating scholarship and advocacy for causes. One gets a simulacrum of this when there is contestation between causes about expert or factual claims: we can argue that this kind of “scholarship,” despite its character as partisan, does somehow improve democratic discussion. One could argue that this is in fact what happens: Weitzman’s research was discredited, as was Regnerus’s attempt to influence the courts. And one could argue that the fact that sociology is ideologically monochromatic—more so than any other field but anthropology (Klein and Stern 2009a, b)—doesn’t matter, because on any important policy issue there are going to be psychologists, statisticians, and economists who have different agendas, and different funding sources, and that in the wash of democratic discussion, these scholars will correct research that is produced in accordance with sociologists’ obvious political biases.

“Improving discussion,” however, is ambiguous: the more common kind of improvement that is advocated, and practiced, is Kitcher’s, namely to forbid or decline to fund research that comes to conclusions one does not like. And this fits with the now common academic practice of asserting limits to discussion based on considerations of civility, where speakers whose message a group one does not like are labeled as offensive. The Regnerus case fits this model: his work was characterized as a form of hate-speech. Moreover, the political discussion itself is not balanced. Weitzman’s false claims are repeated as true despite being discredited, *because* she has a powerful movement on her side. Regnerus had a weak one which was on the losing side of a major cultural and legal struggle. Politics ain’t bean bag.

Where does Smith stand on all this? Here the muddles become important. Smith complains about the blog attacks on Regnerus, which he considers to be ungoverned by professional standards. This is naive, and reflects the deep confusion that runs through the book. Smith wants the protection

part of the old deal, but not the obedience part, the basis of which—a clear distinction between scholarship and advocacy—he ridicules as naive. Smith, however, is naive. If one enters the political and especially the legal arena, as Regnerus and Weitzman did, by claiming expert status, one should expect to be challenged, especially if one’s claims go far beyond the available evidence, as they do in these cases, or are simply false, as they were in Weitzman’s case. And one should expect that the challenges would be political, and that they would focus on interests, and attack the funding sources and connections of the author, the character of the author, and anything else that could be attacked that had to do with the credibility of the claims.

The issues here go very deep. We don’t have a replacement for the language of objectivity, disinterestedness, and professionalism that sufficed in the past and governed what I have called the old deal, or for the institutional protections the old deal provided. The new deal is ambiguous between the goal of contributing to democratic discussion and debate and improving it by suppressing the wrong kind of research and discussion in order to influence public policy. The two often seem inseparable: the point of the research is often to discredit and exclude some viewpoint that is disliked. For much of science, especially those parts which are not particularly relevant to values or “social justice,” this ambiguity is largely without consequences. For sociology, the consequences are central: everything becomes subject to ideological vetting, with the most powerful ideological alliances determining what gets excluded. Christian Smith deserves a great deal of credit for describing the issues that follow from this situation. But oddly, for someone who is committed to the reality of social structures, he ignores the structures that have produced the situation.

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