

14. Ernest Nagel & Felix Oppenheim Respond to Leo Strauss, and the Road not Taken.¹

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Abstract:

This chapter presents the reception of Leo Strauss by analytic philosophers after Strauss's emigration to the United States. It gives a brief survey of the polemics against Strauss and his school by analytic philosophers, which aided in the self-constitution of analytic philosophy as a rival school of thought in philosophy. But most of the chapter is devoted to recovering the significance and influence of a criticism of Strauss by Ernest Nagel. The chapter argues that this response is of intrinsic interest because it involves the relationship between science and values. Nagel's response was foundational to Felix Oppenheim's political philosophy or what Oppenheim called his project of 'conceptual reconstruction.' Lurking in Oppenheim's response to Strauss are questions about the role of the principle of sufficient reason in philosophy. The chapter argues that an opportunity was missed to clarify questions pertaining to the inductive risk of philosophy more than a half century before these issues became fashionable again.

1. Introduction

The *Proceedings of the American Philosophical Association of 1948-1949* reports that, on the recommendations of the Executive Committee, Leo Strauss (1899-1973) was elected to full membership in that year (APA 1949). There is no sign this was anything but routine. At that point Strauss had been stateside for close to a decade.

Educated by elite German philosophers, including Cassirer (who was his supervisor), Husserl, and Heidegger (whom Strauss admired), Strauss arrived Stateside alongside the great exile of the Vienna and Berlin schools, who with the scientific wing of pragmatism (under Ernest Nagel's leadership) helped form the backbone of American analytic philosophy which flourished simultaneously as the school named after him did. (For biographical information I have relied on Tanguay 2007 and Sheppard 2007) While the present chapter does not ignore the polemics between the schools, it primarily reconstructs how Strauss' *philosophy* was criticized by analytic philosophers, and this reconstruction sheds light on recent debates pertaining to the inductive risk of philosophy and the fact-value distinction as well as a forgotten episode in the reception of the Principles of Sufficient Reason (hereafter PSR).

¹ I thank the editor of this volume for his meticulous and helpful editing. I thank Robert Pippen, Quentin Skinner, and Anna Alexandrova for helpful comments. Some of this material was first tried out on my blog, *digressionsnimpresions*.

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Mijn belangrijkste opmerking is dat het goed zou zijn om iets meer duidelijk te maken op welke manier het stuk zich verhoudt tot de rest van de bundel (het migratiethema). Op de volgende pagina in de comments een aantal suggesties.

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Misschien zou je hier ook kunnen toevoegen dat dit hoofdstuk reconstrueert hoe Strauss's school bekritiseert werd door analytisch filosofen en dat deze interacties nieuw licht werpen op recente debatten mbt de inductive risk of philosophy en de fact-value distinction. Op die manier is het ook direct duidelijk wat het doel van het hoofdstuk is.

After a few years in England, where he published a major book on Hobbes, *The Political Philosophy of Hobbes: its Basis and its Genesis* (1936), Strauss had come to the United States in 1937, where he received an appointment at the New School. The book on Hobbes had been reviewed respectfully in *The Journal of Philosophy* and *The Philosophical Review* (Lamprecht 1937; Sabine 1938).¹ In these reviews, Strauss was understood as offering a developmental and contextual account of Hobbes. If Strauss had published nothing after the Hobbes book, he could have been understood as an early anticipation of the so-called Cambridge school's 'contextual' turn in the history of thought.²

In an otherwise critical (1951) review of Strauss' (1948) work, *On Tyranny*, Vlastos (then at Cornell), who in his prime was "one of the most influential" in ancient philosophy as analytically practiced (Annas 2004, 30, 42), writes, "Those who have read and admired Professor Strauss's earlier book on Hobbes will be disappointed in this monograph." In fact, Vlastos (1951, 593) goes on to *praise* Strauss' "learning" and "agility of mind" before complaining that "the weakness of this [new] work can be traced directly to his present addiction to the strange notion that a historical understanding of a historical thinker is somehow a philosophical liability".³

Once Stateside Strauss continued publishing numerous book reviews and articles often in *Social Research*, then recognized as a "philosophical periodical" (Strauss 1939; 1941; 1947).⁴ While American scholars of that age still actively read scholarly works in German I have found no trace of the impact of Strauss' earlier *Die Religionskritik Spinozas als Grundlage seiner Bibelwissenschaft* (1930) in the 1940s or 50s.

At the dawn of the cold war, in October 1949, Leo Strauss gave a number of Walgreen lectures at the University of Chicago that were published as *Natural Right and History* (1953; hereafter NRH).⁵ In 1949 he also became a professor of political science at the University of Chicago. The appointment and the book propelled him into prominence (Burnham 1954, 24).

¹ Sabine was an important historian of philosophy then, who helped build up the Cornell department. The *Journal of Philosophy* review is signed S. P. L. which probably stands for Sterling P. Lamprecht (a historian of philosophy). See also Oakeshott (1937), which is very positive and insightful, where critical (and very much worth re-reading) and Watkins (1955; 1957).

² While Quentin Skinner has been polemical toward Strauss in other work, he cites this book approvingly in *Reason and rhetoric in the philosophy of Hobbes* (1996). I do not mean to suggest that founding contextualism would have been Strauss' self-understanding; he clearly was also grappling with and responding to Carl Schmitt. On this see McCormick (1994).

³ In 1985, in his famous take-down of Strauss in the NYRB, Burnyeat also remarks "The range of his learning is indeed formidable; his command of ancient and medieval languages cannot fail to impress; his minute scrutiny of each text establishes an aura of reverence for its author."

⁴ See: "Notes," *The Philosophical Review*, 49(2) (Mar., 1940):281 & "Notes," *The Philosophical Review*, 51(2) (Mar., 1942): 242. During his time at The New School, Strauss published more work in *Social Research*, then edited and published by the Graduate Faculty of Political and Social Science of the New School for Social Research.

⁵ For a review that anticipates many of the subsequent, recurring criticisms lodged at Strauss, see Plamenatz (1955).

Before long Strauss was known in his own right and for the school he had founded. This 'school' received notoriety with the (1962) publication of Herbert J. Storing (ed.) *Essays on the Scientific Study of Politics*—all the authors, in addition to Strauss (who contributed an "epilogue") were students of Strauss or had spent considerable time with Strauss! *Essays on the Scientific Study of Politics* is a fierce (and often uncharitable) polemic against then mainstream empirical science.⁶

That Straussianism came to be seen as a school can be inferred, from for example, in a (1962) review by the prominent game and social choice theorist, William Riker, of Buchanan and Tullock's *Calculus of Consent*, Riker could mention, in passing, that "Some, like Leo Strauss, have urged a return to the great tradition; but unfortunately the process of return, as it appears in the work of both master and students, has turned out to be an even more sterile historicism than that against which they revolted" (Riker 1962). In the flagship journal of the American Political Science Association, Stanley Rothman starts his anti-Straussian polemic with the following two sentences: "Perhaps no single individual has had as much impact on the discipline of political science during the past several years as has Leo Strauss of the University of Chicago. Both he and his disciples (and they are disciples in the "classical" sense) have engaged in a full scale attack upon the premises underlying the contemporary study of politics" (Rothman 1962, 341).⁷

Compared to the fire-works that Straussianism generated within political science, the response by philosophers was relatively muted. The full-scale polemics against Strauss really start with John Yolton (1921-2005), who was a PhD student of Ryle's (1950-52) that resulted in a D.Phil. thesis, "John Locke and the Way of Ideas; a Study of the impact Locke's Epistemology and Metaphysics upon his Contemporaries" (Buickerood and Wright 2006). This dissertation and the book, *John Locke and the Way of Ideas*, are important mile-stones within the development of so-called 'contextual' historiography within analytic philosophy. After the book was published, in a subsequent article, Yolton contested the "violently distorted interpretation recently advanced by Leo Strauss" of Locke in NHR (Yolton 1958, 478).⁸ He used the occasion to argue that "Strauss's general esotericist thesis suffers a severe blow when we consult the techniques he employs" (ibid.).⁹ Strauss' reliance on diagnosing and practicing esotericism was a subsequent recurring source of polemics among analytic philosophers.¹⁰

In particular those working in ancient analytic philosophy objected to Straussian esotericism from the start (Vlaston 1951) and returned to it regularly, which is especially notable in a very famous (1985) NYRB essay, "Sphinx without a secret," by Myles Burnyeat then Laurence Professor of Ancient Philosophy. Later Malcom Schofield called this a

⁶ For then contemporary (and critical) reaction, see Schaar and Wolin (1963).

⁷ The accompanying footnote reads: "The most articulate of Strauss' students include Walter Berns, Joseph Cropsey, Harry V. Jaffa and Allan Bloom."

⁸ It is worth asking why the treatment of Plamenatz (1955) was not thought conclusive.

⁹ Yolton's case is not as tight as one might wish for, see <https://digressionsnimpresions.typepad.com/digressionsnimpresions/2022/06/on-esoteric-locke-and-cicero-yoltons-polemics-with-strauss-pt-2.html>.

¹⁰ Outside of analytic philosophy, the best and most sober criticism of Strauss' esotericism is Blau (2012). For a thoughtful defense see Melzer (2020).

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“devastating critique” of “Leo Strauss’s treatment of Plato and its development into a cult and a political ideology” (2019, 62).¹¹ I do not mean to suggest that Strauss’ reliance on esotericism was the only source of disagreement. Ancient philosophers, especially, also objected to Strauss’ reliance on and handling of the evidence derived from Xenophon’s testimony of Socrates.¹² Outside of analytic philosophy, political theorists and historians of political thought also objected to Strauss primarily on methodological grounds.¹³ These polemics, and the boundary poling involved, have much to teach us about the sociology and discipline/identity formation of post WWII analytic philosophy Stateside.

Even so, this paper focuses on less well remembered and more subdued interactions. For, I show that Strauss’ work also elicited a more substantive philosophical response, especially from Ernest Nagel. This response is of intrinsic interest because it involves the relationship between science and values. I show that Nagel’s response was foundational to Felix Oppenheim’s political philosophy or what Oppenheim called his project of ‘conceptual reconstruction.’ In addition, I show that lurking in Oppenheim’s response to Strauss are questions about the role of the principle of sufficient reason (hereafter PSR) in philosophy. My paper suggests that an opportunity was missed to clarify questions pertaining to the inductive risk of philosophy more than a half century before these issues became fashionable (as ‘public philosophy’ and ‘responsible speech’).

In the first two sections, I show how in his classic (1961) *The Structure of Science*, Ernest Nagel engaged quite seriously with Strauss on the fact-value distinction. I also show that Nagel’s argument is essential to Felix Oppenheim’s method of political philosophizing.

In the third section, I analyze Oppenheim’s criticism of Strauss in the 1950s.¹⁴ Oppenheim represents a different way of doing political philosophy from both Strauss and the subsequent dominant strain of Rawlsianism, and so this provides a useful glimpse at a largely forgotten episode in the history of analytic philosophy.

2. Ernest Nagel vs Strauss

The discussion of Strauss in Nagel’s *Structure of Science* occurs in a chapter 13 on the “methodological problems of the social sciences” in section (V) on “The Value-Oriented Bias of Social Inquiry.” This is a rather long, ambitious chapter (over 50 pages). The chapter made Nagel a recognized authority in the philosophy of social science, and got him invited to debates internal to (say) economics (Schliesser 2022). The chapter also includes, for example, Nagel’s

¹¹ Schofield is a Fellow of St John’s College, Cambridge, and Emeritus Professor of Ancient Philosophy in the University of Cambridge.

¹² In addition to Vlastos (1951), see especially Irwin (1974) to get a sense of the polemics.

¹³ See Rothman (1962) and, especially, Skinner (1969).

¹⁴ I do not mean to suggest it is the first substantive criticism of Strauss among American political philosophers. In 1954, Arnold S. Kaufman published, “The nature and function of political theory” in *Journal of Philosophy* with very interesting criticisms of Strauss. But it is not obvious Kaufman ought to be understood as an analytic philosopher then, although he certainly was educated in a NYC pragmatist environment.

ferocious criticism of Hayek's *The Counter-Revolution of Science*, which Nagel partially recycles from a polemical 1952 review in *Journal of Philosophy*.

Nagel could be a very fierce polemicist, including a public, political polemicist (Schliesser 2022). But the response to Strauss in *Structure*, does not fit that mold. Rather the section—it's about five pages—attributes to Strauss a family of arguments, which Nagel calls “sophisticated” (Nagel, *Structure*: 490), that [A] ‘social sciences cannot be value free.’ Nagel treats this as entailing or nearly identical to the thesis [B] that there is no “compelling reasons” that “an ethically neutral social science is inherently impossible.” (Nagel 1961, 495). It may seem a bit surprising that [A] is thought to entail [B] because ethical neutrality may well be a significant ‘value’! To be sure, Nagel himself derives this formulation from Strauss, who attributes the identity of “value-free” or “ethically neutral social science” to Weber (1953, 40).

Before I get to the details of Nagel's arguments two observations: first, Nagel responds to a version of Strauss' argument published in 1951 in *Measure*. To the best of my knowledge this journal did not have a long life or wide circulation. But Strauss reprinted a version of the article as chapter II in NRH. The passage(s) from Strauss that Nagel quotes can be found there on pp. 50-53. The omissions in Nagel are basically all the places where Strauss is criticizing Weber directly. (I return to that below.) NRH was published in 1953, and famous, so it is a bit odd that Nagel does not cite it.

Second, a few years ago, Anna Alexandrova published a paper, “Can the Science of Well-Being be Objective?” in *The British Journal for the Philosophy of Science* (2018) that has quickly become a classic in its own right. This paper engages with the material I am about to discuss (and mentions that Strauss was Nagel's target), and expresses my own substantive disagreements with Nagel better than I could. So, it is by no means obvious that Nagel's position still represents the consensus view. Alexandrova has indirectly helped rehabilitate a position akin to Strauss (but decoupled from the bad baggage of Straussianism). So, I am primarily focused on describing the significance of the debate rather than evaluating it.

Section (V) of Nagel's chapter is devoted to the following issues:

Since social scientists generally differ in their value commitments, the ‘value neutrality’ that seems to be so pervasive in the natural sciences is therefore often held to be impossible in social inquiry. In the judgment of many thinkers, it is accordingly absurd to expect the social sciences to exhibit the unanimity so common among natural scientists concerning what are the established facts and satisfactory explanations for them. Let us examine some of the reasons that have been advanced for these contentions. It will be convenient to distinguish four groups of such reasons, so that our discussion will deal in turn with the alleged role of value judgments in (1) the selection of problems, (2) the determination of the contents of conclusions, (3) the identification of fact, and (4) the assessment of evidence. (Nagel 1961, 485)

I quote it for two reasons: first, because it shows that the way Nagel operationalizes ‘value neutrality’ is in terms of unanimity, or consensus. To readers of Kuhn's *Structure* this should be familiar! The broader context of Nagel's argument shows that the relevant class participating in or constituting the relevant consensus is “competent workers in the ... sciences” (Nagel 1961, 448). In addition, the question for social science is articulated in terms of the

purported “unanimity so common among natural scientists concerning what are the established facts and satisfactory explanations for them.” Nagel is aware, of course, that natural scientists do not always agree, but this occurs for him either at the research “frontier” (ibid., 448) or in contexts where the disagreement is an effect of “alternative formulations” that are “mathematically equivalent” (ibid., 158). So, value neutrality is constituted by expert agreement over facts and the theories or (equivalent) models that explain them.

Second, the response to Strauss falls under what Nagel calls (3) “the identification of fact.” Nagel identifies Max Weber's position regarding the role of value judgments with (1). He does not identify a single author with (2), but in it he treats S.F. Nadel (an anthropologist, who was “a pioneer of multi-sited ethnography,”), his own teacher Morris R. Cohen, and A. E. Burt (whose work on Newton is still important) as typical representatives of it (Nagel's own discussion draws on work by the economist and later Nobel laureate Gunnar Myrdal). Finally, Karl Mannheim is treated as the exemplar of someone who holds (4). What is neat about this list is that it is both politically diverse and reflects the practices of different kinds of social science(s).

Nagel attributes to Strauss two distinct criticisms: the first is that “the distinction between fact and value ... is untenable when purposive human behavior is being analyzed, since in this context value judgments enter inextricably into what appear to be ‘purely descriptive’ (or factual) statements” (ibid., 490). The second is that purported “factual claims about means-ends statements” are themselves infected with values (ibid., 491). It is not hard to see that they are similar in kind, because in both contexts we are dealing with the possible presence of a kind of teleology or opaque context.

In his response to the views he attributes to Strauss, Nagel emphasizes that Strauss is *right* about three features: “(a) that a large number of characterizations sometimes assumed to be purely factual descriptions of social phenomena do indeed formulate a type of value judgment; (b) that it is often difficult, and in any case usually inconvenient in practice, to distinguish between the purely factual and the ‘evaluative’ contents of many terms employed in the social sciences; and that (c) values are commonly attached to means and not only to ends.” (Nagel 1961, 491 [letters added to facilitate discussion]).

Nagel insists that Strauss equivocates on two notions of ‘value judgment:’ first, “the sense in which a value judgment expresses approval or disapproval either of some moral (or social) ideal, or of some action (or institution) because of a commitment to such an ideal; and the sense in which a value judgment expresses an estimate of the degree to which some commonly recognized (and more or less clearly defined) type of action, object, or institution is embodied in a given instance” (ibid., 492) Nagel adds that these notions are often conflated in the social sciences and that sometimes it is not so easy to distinguish them. But he concludes his argument that “there are no good reasons for thinking that it is inherently impossible to distinguish between the characterizing and the appraising judgments implicit in many statements, whether the statements are asserted by students of human affairs or by natural scientists” (ibid., 494).

Now, rhetorically, it is important that Nagel's argument—and this is characteristic of his argument in the whole chapter—generally has the following form: ‘an apparent problem X in social science occurs *also* in natural sciences and when X occurs in natural science, X does not prevent the development of consensus in the natural sciences (and, thus has been tamed in

the natural sciences) and so X does not pose an in principle obstacle to consensus in social science.’ So, for example, Nagel illustrates the two senses of “value judgment” with an example from biology.¹⁵ And the effect of this move is to turn the critic of value neutrality in social science into a critic of the purported value neutrality of natural science. Nagel assumes nobody (not even the sociologist of knowledge he discusses under (4)) will go that far. Of course, somebody may well concede Nagel’s position for general relativity, but worry about conceding it in the life sciences.

It is worth saying something about what Strauss is up to. In its original context, Strauss’ argument is directed against Weber. In particular because Weber is taken to be a spokesperson for the following position: “Natural right is then rejected today not only because (i) all human thought is held to be historical but likewise because it is thought that (ii) there is a variety of unchangeable principles of right or of goodness which conflict with one another, and none of which can be proved to be superior to the others” (1953, 36). Strauss associates Weber with features of what he calls ‘historicism’ (viz, i) and with value pluralism (viz, ii). To be sure, Strauss recognizes that Weber is not a pure historicist, because Weber recognizes the historical situatedness of the sciences alongside the “trans-historical” nature of its “findings regarding the facts and their causes. More precisely, what is trans-historical is the validity of these findings” (1953, 39). And because Strauss wants to argue for natural right, or at least, the inherent possibility of the recovery of natural right, he is critical of Weber.

So, Strauss is not especially interested in doing social science (although I qualify that below). But he is interested in a kind of self-limitation Weber puts on social science: “the absolute heterogeneity of facts and values necessitates [for Weber] the ethically neutral character of social science: social science can answer questions of facts and their causes; it is not competent to answer questions of value” (1953, 40). Unless values are self-contradictory or help generate incoherence, the social scientist must be silent about them. Now this position is coherent, and it is not obvious why Strauss cares about social science method at all.

The answer to that question is that Strauss (correctly) discerns that Weber is a kind of skeptic about value: “his belief that there cannot be any genuine knowledge of the Ought” (1953, 41) And so observed, value pluralism is an *effect* of this Weberian skepticism (which according to Strauss naturally leads to a nihilism that Weber obscures from himself). To be sure, Strauss attributes to Weber a kind of formal possession of the norm, “Thou shalt have ideals,” (1953, 44); so on this view Weber is not a radical (second order) skeptic about value. For Weber the content of this norm is, as Strauss notes, “Follow thy god or demon,” which Strauss reinterprets as “devotion to a cause” (1953, 46).

As an aside, the really significant observation on Weber’s philosophy for political theory by Strauss is that “Weber’s thesis that there is no solution to the conflict between values was then a part, or a consequence, of the comprehensive view according to which human life is essentially an inescapable conflict. For this reason, ‘peace and universal happiness’ appeared to him to be an illegitimate or fantastic goal. Even if that goal could be reached, he thought, it would not be desirable; it would be the condition of ‘the last men who have invented happiness,’ against whom Nietzsche had directed his ‘devastating criticism’” (1953, 65). That

¹⁵ To what degree Nagel can presuppose the value-neutrality of biology is actually an interesting topic connected to his life-long effort to provide an analysis of function statements.

is to say, Strauss diagnoses how he takes Weber's Nietzscheanism to anticipate Carl Schmitt's position (even if Schmitt is not mentioned by Strauss in *this* context.)

Now, the objection to Weber by Strauss that Nagel cites occurs in the discussion of Strauss' analysis of Weber's sociology of religion, which "presupposes a fundamental distinction between 'ethos' and 'techniques of living' (or 'prudential' rules)." And Strauss suggests that "the sociologist must then be able to recognize an 'ethos' in its distinctive character; he must have a feel for it, an appreciation of it, as Weber admitted" (1953, 50). Then occurs the passage Nagel (selectively) quotes on 490-491. In Nagel's hands it is completely unclear that Strauss is offering an immanent critique of Weber.

If one only reads Nagel (without knowledge of Strauss' original) then it is natural to think that Strauss's rejection of ethical neutrality of social science is itself an obstacle to consensus or unanimity in social science; so that lurking in Strauss' argument is a kind of denial of social scientific objectivity. **But this is a misrepresentation by Nagel; for Strauss the whole point of recognizing values in social science is to make social scientific and historical objectivity possible.** (Because Strauss is often so critical of then existing social scientific practice this is easy to miss.) This is completely explicit in Strauss. For, Strauss concludes his own argument against Weber (at least the present one that Nagel is focused on) as follows:

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The rejection of value judgments endangers historical objectivity. In the first place, it prevents one from calling a spade a spade. In the second place, it endangers that kind of objectivity which legitimately requires the forgoing of evaluations, namely, the objectivity of interpretation. The historian who takes it for granted that objective value judgments are impossible cannot take very seriously that thought of the past which was based on the assumption that objective value judgments are possible, i.e., practically all thought of earlier generations. Knowing beforehand that that thought was based on a fundamental delusion, he lacks the necessary incentive for trying to understand the past as it understood itself. (Strauss 1953, 60-61)

That is to say, for Strauss establishing the possibility of establishing natural right just is the possibility of establishing a trans-historical consensus/agreement or unanimity in social science. (To avoid confusion: all parties to the debate then agree that history, as a scholarly discipline, is at least partially part of social science—in German, history is a *Wissenschaft*.)

So, somewhat ironically Nagel and Strauss *agree* about what we might call the formal aims of social science, that it involves a species of objectivity that makes a scholarly, fundamental consensus possible. This fundamental *agreement* is obscured by Nagel's presentation. So a reader of Nagel who is unfamiliar with Strauss may well come away thinking that Strauss is a critic of the sciences or anti-scientific. It cannot be ruled out that this rhetorical effect is an intentional pay-off of Nagel's treatment of Strauss.

As an aside, recently a transcription was published of Strauss' (1965) lecture course, "Introduction to Political Philosophy," at The University of Chicago. He had assigned Nagel's *Structure* in the course, and he prepared a response to some of Nagel's criticism that is very

much worth exploring.¹⁶ The existence and nature of this material suggests that Strauss also made an explicit decision not to engage directly publicly with some of his analytic critics.

As presented here, Nagel's criticism of Strauss may be thought to belong to the philosophy of social science. However, as I show in the next section, it also had an influential afterlife within somewhat forgotten pre-Rawlsian era of analytic political philosophy.

3. Felix Oppenheim's Program of Conceptual Reconstruction

As Daniel Dennett informed me, Felix Oppenheim was the son of Paul Oppenheim of Hempel & Oppenheim (1948) fame.¹⁷ While there was not much distinctly analytic political philosophy before Rawls, Oppenheim (1913-2011) would have to figure in any such a story.¹⁸

In the "post-script" to a 2001 festschrift, *Freedom, Power and Political Morality*, devoted to him, Felix Oppenheim (1913-2011) rhetorically asks if he is the "lone survivor of a movement by now of merely historical interest?" and distances himself from the suggestion that he was a logical positivist because he rejects "operationalism and radical empiricism" (Carter and Ricciardi 2001, 218). Rather, he prefers to understand himself as committed to *conceptual reconstruction*, which provides concepts "with descriptive definitions in order to make them available for fruitful communication even among persons or groups with different normative views" (ibid.).

Conceptual reconstruction is, thus, a species of conceptual engineering or conceptual articulation and has two goals: first, it is in the service of fruitful communication in the context of substantive normative and political pluralism. (As such, the project is not far removed from the spirit of the Rawlsian enterprise of conceptually constituting an overlapping consensus.) Of course, one may do conceptual reconstruction in a homogeneous society, but there it would be less needed.

What is distinctive about Oppenheim's program is that he thinks that a 'descriptive' definition does "without the use of valuational notions" (Carter and Ricciardi 2001, 219). This also makes clear that facilitating fruitful communication is not the only possible use of conceptual reconstruction: second, Oppenheim also intends, second, to provide social scientists — he explicitly mentions political scientists — with concepts that can be used in their descriptive or empirical social science (ibid., 221). The absence of valuational notions facilitates in the aim of avoiding the imposition of normative views on the social phenomena studied.¹⁹

Assuming that such conceptual reconstruction without valuational notions is possible, Oppenheim offers explicative definition "which might diverge from ordinary language" (Carter and Ricciardi 2001, 219). To what degree such divergence makes the output of concept

¹⁶ Strauss (2010). The discussion is wide-ranging: Strauss treats Nagel as a kind of 'positivist' who he compares to Reichenbach (p. 110).

¹⁷ One might add "Oppenheim & Putnam (1958)" fame and (inter alia) "Bedau & Oppenheim" (1961) fame, too.

¹⁸ So what follows is meant, in part, as a supplement to Wolff (2013).

¹⁹ See also Deutsch and Rieselbach (1965, 153-4), where Oppenheim is mentioned.

reconstruction less amenable to political life is ignored, but it seems safe to predict that conceptual reconstruction is more useful to those committed to value-free social science than to political agents in the middle of public contestation. Descriptive definitions so conceived “are not true or false, since they are linguistic stipulations” (ibid.). They are, rather, more or less useful. And this consequentialist understanding of success has a Carnapian flavor to it.

It is to be allowed, however, that in the context of mutual trust or elite bargaining, “political actors sometimes make some effort at ‘resolving those differences through argument and persuasion’” and then the presence of descriptively defined concepts may be very useful (ibid., 221).²⁰ Oppenheim’s program can work in a political culture in which technocratic tools (say a lexicon) are welcomed to reduce social friction or are used to reduce conflict over the terms of a bargain. What this requires is that “a lexicon be uncontestable, in the sense that its vocabulary be made up of definitions that are not valuationally tinted” (ibid.). Such a lexicon can be contested and contestable in the sense that an improved vocabulary might facilitate more efficient or consensual (etc.) bargaining. No lexicon is final.

Oppenheim allows that his conception of descriptive definition is dependent on an “idea” he “did take over from the logical positivists: the separability of ‘facts’ and ‘values’ on the *conceptual* level” (ibid., 219, original emphasis). In context, he does not argue for the claim. However, Oppenheim had defended the claim in a 1973 paper. There he acknowledges that in the speech acts of *ordinary or everyday life* there is no such separability (1973, 58). So, there is another sense in which Oppenheim echoes Carnap’s philosophy: it relies on a relatively sharp distinction between a natural language of ordinary folk and a specialist language (which is often formal). In the 1973 paper, it is explicit that Oppenheim is more focused on designing a “the language of social and political inquiry” (ibid., 59).

Unfortunately, the 1973 paper argues by way of criticizing those who had either de facto or in practice denied such separability. So it is not really helpful in reconstructing Oppenheim’s positive argument *in favor* of the conceptual separability of facts and values thesis. But for Oppenheim the positive argument is embedded in his earlier account of descriptive definitions. For example, near the start of his (1970) “Egalitarianism as a Descriptive Concept,” Oppenheim writes

Equality and inequality of characteristics are no doubt descriptive concepts. Indeed, whether A and B have the same age or nationality or income can be empirically ascertained. So can assertions that A has greater ability or aptitude than B. These are characterizing value judgments: such statements are descriptive, not normative. (1970, 143)

This terminology is clearly indebted to the material of Ernest Nagel’s (1961) *The Structure of Science* discussed in section 2 above. Unsurprisingly Oppenheim cites the relevant page-numbers of Nagel (in which he is critical of Strauss) in the accompanying footnote. So, the first main pay-off of this investigation is that Oppenheim’s program of conceptual reconstruction *presupposes* that Nagel’s criticism of Strauss is substantially correct, and that, thus,

²⁰ Oppenheim is partially quoting Ball’s contribution to the festschrift.

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characterizing value judgments can be kept sufficiently distinct from the normativity we find in appraising judging.

As I show in the next section, there is very good evidence that Oppenheim was familiar with Strauss' *Natural Right and History*. So, he clearly must have made up his own mind about the adequacy of Nagel's response. However, If the reader agrees with me that Alexandrova's (2018) criticism of Nagel on this score is apt, then Strauss' unwillingness to pursue the exchange with Nagel in public also gave Oppenheim false confidence in the robustness of his program.

4. Felix Oppenheim's Criticisms of Leo Strauss

In a 1955 essay, Oppenheim defended a form of epistemological relativism. In particular, Oppenheim is committed to the claim that "words such as 'good,' 'desirable,' and 'valuable' do not designate properties of things or events or actions, but express the speaker's subjective preferences" (1955, 411). Oppenheim is explicit that he is echoing Charles L. Stevenson's *Ethics and Language*. To be sure Oppenheim's "relativism is opposed to value-objectivism, not to objectivism in science. If 'objectivity' means possibility of objective, i.e., intersubjective, verification, relativism denies the objectivity of intrinsic value-judgments, but not the objectivity of empirical statements" (ibid.). Oppenheim goes on to claim: "Relativism does not question the possibility of explaining or giving reasons for people's valuations; relativism denies the possibility of validating or giving grounds for them" (ibid., 412).

An auxiliary claim that matters a lot to Oppenheim's argument is that "logically, there is no necessary connection between any particular value-judgment and either absolutism or relativism" (ibid.). This will play an important role in Oppenheim's argument against Strauss. Oppenheim identifies Strauss twice as one of his targets. First, in a footnote attached to the following passage:

[A]bsolutists tend to consider relativism incompatible with whatever goals they adjudge demonstrably valuable. Thus value-absolutists who are also political absolutists (i.e., who claim that the values underlying political absolutism are the 'true' values) maintain that relativism is dangerous because it promotes democratic values. Absolutists who believe that the democratic way of life can be proved to be the best often hold that a subjectivist be a 'true' democrat. The latter thesis is based on arguments such as the following: if good and evil is not a matter of objective knowledge, then everything is a matter of indifference. The relativist must therefore be tolerant of every ethical viewpoint, even of doctrines which command the overthrow of democracy. Or still worse: if there is no justice, it does not matter how one acts; so one may just as well climb on the bandwagon. Thus relativism becomes akin to nihilism, cynicism, and opportunism.

In the accompanying footnote, Oppenheim quotes the following passage from Strauss NRH: [I] "If our principles have no other support than our blind preferences, everything a man is

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willing to dare will be permissible. The contemporary rejection of natural right leads to nihilism - nay, it is identical with nihilism" (1953, 5, Roman numeral added to facilitate discussion).²¹

Second, Oppenheim goes on to claim, shortly thereafter, "absolutists are likely to shift the argument from the logical to the psychological level and maintain that it is psychologically impossible to commit oneself whole-heartedly to any set of values unless one is convinced that it corresponds to the objectively valuable." And in the accompanying footnote, Oppenheim quotes Strauss from NRH as follows: [II] "Once we realize that the principles of our actions have no other support than our blind choice, we really do not believe in them any more. We cannot wholeheartedly act upon them any more. We cannot live any more as responsible beings" (1955, 415).²²

Both of Strauss' claims can be found in the "Introduction" of NRH. In immediate context Strauss does not offer detailed arguments for these claims so they will seem rather ungrounded. It is a bit peculiar that Oppenheim ignores here the rest of Strauss' arguments developed later in the book. Even so, there is something interesting to be said about the state of the debate between Strauss and Oppenheim.

In the first Strauss passage [I], Strauss does not claim here that relativism leads to "cynicism or opportunism" as Oppenheim suggests. Even so, it is not obvious why according to Strauss nihilism follows from the rejection of natural right or from thinking that our principles are derived from our preferences.

The second [II] passage helps start to explain why Strauss thinks nihilism follows from the kind of relativism somebody like Oppenheim defends. Obviously, it is not obvious why Strauss seems to be committed to such a dramatic slippery slope argument. But it becomes intelligible if we recognize that, fundamentally, there is a suppressed premise in Strauss' argument: that something needs to ground the principles of our actions or these grounds (and so on) to halt the slide down the slope (toward nihilism). So, lurking in Strauss' position is a kind of appeal to some version of the PSR to halt a regress argument.

That Strauss is committed to something like the PSR is not wholly obvious in context of the introduction to NRH. But later, in the long chapter 2 on his critique of Weber's account of the distinction between fact and values (which is the chapter that Ernest Nagel criticizes), in the context of ascribing to Weber the idea that "science or philosophy rests, in the last analysis, not on evident premises that are at the disposal of man as man but on faith," Strauss goes on to claim: "By regarding the quest for truth as valuable in itself, one admits that one is making a preference which no longer has a good or sufficient reason. One recognizes therewith the principle that preferences do not need good or sufficient reasons" (Strauss 1965, 72).

There is much to be said about Strauss' account as a reconstruction of Weber, and as a diagnosis of the effects of Weber's position. But what does seem clear is that Strauss associated the rejection of what Oppenheim calls 'absolutism' with the claim that preferences are ungrounded or at least not grounded in sufficient reason.

In fact, being alert to this feature of Strauss' position also helps explain the recurring use of "blind" in modifying "preferences" and "choice" in the passages [I]&[II] Oppenheim quotes in the notes. (This use of 'blind' echoes the kind of language early modern critics of

²¹ In the 1965 edition it is on p. 4.

²² Oppenheim is quoting Strauss (1953, 6). It is also p. 6 in 1965 edition of NRH.

epicureanism would use when drawing on the PSR.) Such preferences and choices are treated as unguided, and so de facto random. So, lurking in Strauss is a defense of, or at least an acceptance of (the utility of), the PSR,²³ and metaphysics more generally.

Interestingly enough Oppenheim *agrees* about the stakes (but not on the PSR). Oppenheim claims that his flavor of “Relativism does not question the possibility of *explaining* or giving reasons for people's valuations; relativism denies the possibility of *validating* or giving *grounds* for them” (1955, 412, original emphasis). So, not unlike Russell and much of the mainstream of analytic philosophy since, Oppenheim rejects the PSR (at least in the context of values).²⁴

I do not mean to suggest that the disagreement between Oppenheim and Strauss is only, ultimately, about the status of the PSR. Oppenheim's explicit criticism of Strauss are also framed by an *implicit* disagreement over the status of inductive risk of relativism. Oppenheim does not name the critic who holds that “relativism is subversive:” “they brand relativism as socially harmful if not outright subversive” (1955, 414). But that philosophy should be mindful of harming society is a commitment that runs through Strauss' philosophy.²⁵ Concern over the inductive risk of philosophical activity has not been fashionable within analytic philosophy. For, as Oppenheim retorts, such concern (and here he anticipates a move familiar from Nagel) does not undermine the “validity of the thesis: To argue this way would be like questioning the validity of nuclear physics by pointing to the H-bomb” (1955, 414).

In 1955, Oppenheim had understood Strauss's criticism as a kind of psychological thesis. He is unconcerned by Strauss's argument because he understands his own moral relativism as an epistemological thesis. Even if one is utterly unmoved by Strauss's position (which is expressed rather concisely and ultimately rooted in the PSR), it is pretty clear that Oppenheim has misjudged it. Strauss's claim is not a contribution to psychology, but existential. The fact that Strauss thinks our preferences lack ground means that for him they are fundamentally unexplained from the perspective of rationality. So, Strauss's position is much more related to epistemology than Oppenheim allows.

Oppenheim must have felt something akin to the problem I have just diagnosed because Oppenheim returns to his disagreement with Strauss in a 1957 APSR article. He quotes (in redacted form) the same passages from Strauss [I]&[II] as he did in 1955. But he offers a new response.

Before I get to that, one aside. In his article Oppenheim first mentions Strauss in the following way, “Non-cognitivism does not maintain that value-words, even in the intrinsic sense, are meaningless, but only that they have normative, evaluative, directive, rather than cognitive meaning. Natural law theorists are therefore mistaken when they claim, as e.g., Leo Strauss does, that [III] the denial of natural law implies ‘the prohibition against value judgments in social science’” (1957, 50).²⁶ Oppenheim quotes Strauss out of context. In context

²³ On this point, see Bruell (2011) which cites more passages in NRH in support of this claim. I thank Thomas Cleveland for calling my attention to this.

²⁴ See Della Rocca (2021), who cites Russell (2010).

²⁵ For evidence of this claim see, especially, the (very critical) chapter on Strauss in Holmes (1993).

²⁶ In an accompanying footnote he cites Strauss (1953, 52).

Strauss is criticizing Weber's account of the separation of facts and values! But this does not affect the rest of Oppenheim's or my own argument.

The new 1957 response is basically Humean in character, although Oppenheim's terminology is ultimately derived from Carnap (1947).²⁷ In responding to [I]&[II] Oppenheim draws on his own account of rational choice and Carnap's idea of total evidence: "To make a rational choice, the decision-maker must predict the consequences of each of the alternative courses of action open to him in a given situation. His decision will be rational provided these anticipations are based on the total evidence available to him. Such predictions are, of course, true only with a certain degree of probability, and a decision may be rational, yet unsuccessful, and vice versa" (1957, 52).²⁸

Let us leave aside how compelling Oppenheim's view of rational choice is. Although it's notable how thin the view is; there is no requirement here of internal consistency at a time or over time. It is not even obvious we are dealing with a species of instrumental rationality (because the predictions are in no way connected to the arbitrary preferences or principles on which choices are founded). Having said that, and in light of his 1953 article ("rational choice"), I am assuming that Oppenheim is assuming a kind of instrumental rationality here, and that he intends to be describing how preferences (he uses "goals") can be satisfied in light of evidence and knowledge.

What is important for present purposes about Oppenheim's new response to Strauss is that he blocks the claim that choices founded on ungrounded preferences are arbitrary altogether. They are now constrained by evidence and how it relates to genuine possibilities and one's knowledge of these (since they are based on predictions). In addition, these actions can be intelligible in an important way because a spectator (who becomes privy to the evidence and knowledge used) may well predict and perhaps even understand the choices made. In fact, this is, on my view, really Oppenheim's underlying point. He wants (recall the discussion above of his program of conceptual reconstruction and looking ahead to his book *Dimensions of Freedom*) to provide conceptual tools for empirical science. He understands his position as "an extrinsic value-judgment, namely the empirical hypothesis: whenever you want to bring about the state of affairs which, under the circumstances, will be most valuable (or least disvaluable) to you, apply the rules of rational decision-making" (1957, 53).

Even if the Straussian would accept Oppenheim's account of rational choice, the Straussian rejoinder is not hard to guess: such predictable choices or instrumental rationality in the serves of ungrounded or arbitrary ends may well exhibit a species of madness/sociopathy or evil. (That is to say the nihilism charge has not been blocked.) So, I doubt that Strauss would be very impressed by Oppenheim's follow up even if he were to grant that given Oppenheim's aims Oppenheim has succeeded on his own terms. If we step back from their debate, the philosophically important point lurking here is that prediction by social science and the

²⁷ In his 1957 APSR article, Oppenheim cites his own (1953) and in it Carnap (1950). This incorporates material from Carnap (1947) article. (See p. 343, note 3 in Oppenheim 1953.) For discussion of underlying philosophical issues, see Good (1967).

²⁸ Cf. Oppenheim (1953, 343): "To arrive at a rational decision, it is sufficient to make warranted predictions about the significant effects of one's significant alternative potential actions."

rationality of what is predicted may come apart. That is of course a truism, even if it is often forgotten. The problem (of the inductive risk of philosophy itself) with the truism arises when predictions by social science (ground in certain philosophical conceptions of rational choice) become action-guiding and so, say, contribute to the irrationality of the outcomes (think of game theory and mutually assured destruction).

That is, lurking in the Strauss-Oppenheim debate is a debate over the relationship between philosophical theorizing and its instrumental role in social science, or other forms of social activism. With the benefit of hindsight, it is a shame that Kaufman's earlier (1954) criticism of Strauss did not play a role in this debate because Kaufman (who cautiously defends an instrumental, social role for political philosophy) notes that Strauss's epistemology of philosophical or eternal truths is itself underspecified and that it is by no means obvious there are truths separate or separable from sciences, including formal ones.²⁹ Oppenheim echoes the point (without citation of Kaufman), when he notes in a footnote, while reviewing Popper's *Conjectures and Refutations* that "it is, however, not always easy to determine whether the anti-empiricists here referred to would claim that their method for apprehending 'truth' is scientific, but non-empirical, or non-scientific—i.e., suprarational. This ambiguity may be found, e.g., in Herbert J. Storing, ed., *Essays on the Scientific Study of Politics* (New York 1962)" (Oppenheim 1964, 351fn14).

During the revival of recent interest in questions over inductive risk of philosophy, the fact-value distinction, and science & values more generally, only Alexandrova has noticed that some of the received, standard positions in analytic philosophy on these matters were sometimes sharpened in response to some elements of Leo Strauss's philosophy. And while it would be wholly misleading to suggest that the increasing rejection of these received view vindicates Strauss's arguments or his mode of philosophy, the dispassionate historian will note that the pull of some of the challenges Strauss articulated to the philosophical defense of mid-century social science was re-discovered independently from Strauss's writings. While this should not make us overlook the ways Straussianism can still provoke and annoy, it would be a mistake to reduce its significance wholly to a fork in the road worth forgetting altogether. For, as I have noted, lurking in Strauss' writing is (recall his apparent embrace of the PSR) a defense, or at least a conditional defense, of metaphysics—a seed of which that blossomed, most unexpectedly, in Syracuse.³⁰

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²⁹ Kaufman comes very close to grasping Strauss' skepticism. On the complex relationship between the PSR and skepticism, see Della Rocca (2017). Only Quentin Skinner seems to have realized Kaufman's significance to the debate between Straussians and their critics. See Skinner (1969, 12-13fn49)! When Yolton confronted Strauss' purported position that "either we are able to ground moral criticism in natural rights, or we are faced with moral nihilism," Yolton does not criticize Strauss' epistemology, but argues (in a proto-Gadamarian vein) that forms of circularity cannot be avoided in dealing with "accepted" social values but that such circularity need not be vicious. Yolton misses Strauss' skepticism and fideism altogether.

³⁰ See Bernadete (1964), especially p. viii.

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