

**Social Science as a Vocation:
Weber, Pragmatism, and Experiential Inquiry**

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Pragmatism would appear to be enjoying something of a surge of popularity in IR theory of late. The recent special issue of *Millennium* (2002) serves as an important document of this renaissance; a number of panels at recent ISA conferences, along with a series of papers (both published and unpublished) mainly written by European scholars,² provide further evidence of pragmatism's entry into the contemporary IR theoretical arsenal. At least for some scholars in the field, a "pragmatic turn" seems to be well underway.

A quick glance at the *Millennium* special issue, however, also reveals important gaps in the development of a pragmatic approach to IR theory. Of the eight main articles in that issue, four of them deal primarily with the relationship between pragmatism and democratic theory, as does Hauke Brunkhorst's keynote address. Here we see pragmatism treated as an ethical or normative theory, and deployed as a tool for the (mostly critical) evaluation of international institutions and state actions. Given that pragmatism begins as an account of how people produce knowledge in practice, this emphasis on ethics is curious, and probably says more about the condition of IR as a

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² Given that the classic theorists of pragmatism were by and large Americans, the fact that European scholars mainly lead the resurgence of pragmatism in IR is striking.

field (which certainly stands in need of richer resources for ethical reflection on global problems) than it does about pragmatism per se.

Indeed, in a special issue on pragmatism I would have expected to see more articles devoted to the *practice* of IR research and the construction of IR knowledge. Three pieces do in fact focus on such methodological and philosophical questions as how to gather and work with data, the proper status of knowledge-claims, and related issues, although only one of them—Iver Neumann's—actually draws on any sustained empirical research on actual practices. The other two authors wrestling with methodological questions (Cochran and Isacoff) end up invoking moral values, such as a commitment to cultural pluralism or peaceful transformation, as their primary justification for pragmatism as an approach—a twist in the argument which reaffirms the notion that pragmatism primarily has ethical, rather than empirical, insights to offer IR scholars.

The two other articles in the special issue address the role that pragmatism might play in helping to adjudicate theoretical clashes in the field. For all of their differences—including their attitudes towards poststructural scholarship—the authors of those pieces end up resting on the allegedly progressive character of pragmatism. David Owen looks to pragmatism as a way to make IR theory more centrally involved in practical politics, thus dissolving the difference between social science and social activism in a manner reminiscent of Bent Flyvbjerg in *Making Social Science Matter* (Flyvbjerg 2001). Peter and Ernst Haas go further, suggesting that the paramount goal of a pragmatic approach to global governing institutions should be to promote

“progress” –even at the expense of a theoretical and conceptual chaos in which “anything goes” as long as it advances their reformist agenda.

Hence we have two and a half options when thinking about what a pragmatic turn might mean for IR: either pragmatism converts IR theory into ethical reflection, or it converts IR theory into a political instrumentality, or –and this is the “half” option, since is it the least well developed to date – pragmatism provides us with both a novel way to think about knowledge-creation in IR and a set of precepts and principles for actually doing so. It is this underdeveloped third option that I think best represents the potential contribution of a pragmatic turn in IR, not in the least because (unlike the other options) the third option permits scholars to retain the logical distinction between facts and values, between science and politics –in short, to preserve the practical autonomy of social science as a distinct worldly endeavor. In so doing, pragmatism understood as a contribution to the philosophy and methodology of social science urges us to undertake what Dewey would have called a *reconstruction* of social science (Dewey 1920), rather than the *replacement* of social science that many scholars seem to think is the primary implication of a pragmatic turn.

To be blunt: I am unconvinced that a pragmatic turn requires, or even urges, the abandonment of the familiar Weberian distinctions (science/politics, facts/values) that uphold social science as a distinct knowledge-making practice. In fact, the main reason that a pragmatic turn does not require this is that Weber’s methodological writings are, by and large, *pragmatist*, although they display a historical pessimism that certainly gives them a different tone than the relatively optimistic writings of Dewey or James.

And there is something of value, I believe, in Weber's skepticism about "progress" and his emphasis on the inadequacy of knowledge to ever resolve value questions, particularly as an antidote to the elements of early-twentieth-century "science triumphalism" that are especially apparent in John Dewey's work.

I make my case in three stages. In Part I, I sketch two of the core wagers in the philosophy of science—one that deals with the relationship between observers and observed, and the other that deals with the relationship between knowledge and experience. Combining these analytical distinctions yields a 2x2 matrix that effectively contrasts four approaches to the construction of knowledge: neopositivist, realist, reflexive, and pragmatist. In Part II I argue that Weber belongs in the pragmatist box. Then in Part III I offer a sketch of the Weberian methodology of ideal-typification, which I suggest is a pragmatic methodology that can help to ground a pragmatic social science. I conclude with some thoughts about the tasks of a social science that adopted this methodology.

I. Core Wagers in the Philosophy of Social Science

The philosophy of (social) science³ is a complex field, featuring a variety of subtle distinctions between positions that concern the character and mechanics of knowledge-production. The usual IR strategy for discussing these issues has been to choose an

³ In what follows I am not going to draw a sharp distinction between the philosophy of science and the philosophy of *social* science, both because philosophers of social science tend to use the philosophy of the natural sciences as at least a point of departure for their own reflections, and because naturalism—the idea that one can study social and natural phenomena in the same basic way—remains an open question in the social sciences, and especially in IR.

author or a group of authors—Roy Bhaskar has been a popular selection of late, and Imre Lakatos remains a perennial favorite—and then to tell the story of the philosophy of science and its implications for IR from their perspective.⁴ Hence we find critical realists dividing approaches to empirical social inquiry into surface-level descriptions and explanations based on the grasping of deeper generative factors (Patomäki and Wight 2000; Wight 1999), Lakatosians looking for theoretical hard cores and progressive problem-shifts (Vasquez 2003), and interparadigm-debate Kuhnians emphasizing the extent to which different theoretical perspectives are incommensurable with one another (Banks 1985). And when these scholars discuss alternative perspectives on empirical social inquiry, they have a tendency to do so in a way that privileges their preferred perspective.

This is a sadly familiar pattern in our field: scholars making forays into the conceptual materials of other disciplines and then returning to IR bearing a more or less decontextualized version of some particular perspective on a question.⁵ Irrespective of whether any of these foreign imports into IR are *correct*, the central problem with these partisan incursions is that they generally fail to provide an appropriate context for the kind of robust empirical debate about the practical implications of any particular perspective. With very few exceptions, discussions of the philosophy of social inquiry in IR present a position that has to be accepted or rejected *tout court*, rather than

⁴ Or, at least, what is taken to be their perspective. While IR critical realists are generally pretty good at reading their source material and placing it in the proper philosophical context, the same can not be said for IR Lakatosians (Jackson and Nexon 2004).

⁵ We even see this pattern in the analysis of particular substantive issues; one has only to think of the strange career of ‘Immanuel Kant’ or ‘Anthony Giddens’ as touchstones for the analysis of democracy or the agent-structure problem respectively.

contributing to a discussion by articulating a contestable position that can be subjected to a variety of evaluations. This happens regardless of the particular position being advocated: neither King et. al. (King, Keohane, and Verba 1994) nor Colin Wight (Wight 2006) provide their respective readers with *arguments for* their perspective, although they spend considerable time fleshing out the implications of their perspective for scholarship.

The need for a framework

The core problem is the absence of something like a useful analytical framework for the discussion of how social inquiry in IR *ought* to unfold. As a field, IR generally lacks the conceptual equipment for such a discussion. Although this lack can be traced to the infrequency with which IR Ph.D. students, especially those trained in the United States, are required (or even permitted!) to take courses in the philosophy of social science during their graduate training (Schwartz-Shea 2003), the partisan manner in which philosophical positions are presented in the field certainly contributes to the problem. If IR scholars were more secure in the status of their research findings as *knowledge* (to say nothing of *science*), perhaps this habit of peering into other disciplines looking for some way to “fix” IR and place it on a more secure footing would vanish, but this is a counterfactual entirely too un-cotenable to evaluate.⁶

⁶ On cotenability and counterfactuals, see (Collins, Hall, and Paul 2004).

In the meantime, we are faced with the challenge of specifying a set of distinctions between approaches to the philosophy of social inquiry that might enable something like an informed debate between aficionados of various perspectives. Such a set of distinctions—such a classification scheme—should, as Dewey argues, should be evaluated “functionally, not structurally and statically”: the central issue should be whether the classification permits and promotes the particular end to which it is directed (Dewey 1920, 150). In the present case, the end to be promoted is a robust debate between perspectives, and this carries two consequences for the scheme: distinctions must be drawn sharply enough to clarify disagreements, but the resulting positions have to resemble (in the Wittgensteinian sense of a “family resemblance”) one another sufficiently that scholars can meaningfully evaluate the consequences of adopting one or more of those positions. This certainly does not mean that positions and perspectives on the philosophy of social inquiry have to be made *commensurable* in a way that would permit some kind of direct empirical test between them; indeed, inasmuch as such perspectives consist of ontological and epistemological commitments (which perspectives on the character of social inquiry necessarily do), no such empirical testing is even *conceivable* (Smith 1989, 21). But it does mean that, whether or not we believe in Kuhnian incommensurability,⁷ we have to construct positions that are

⁷ That it is possible to regard perspectives as incommensurable and still maintain that they are evaluable vis-à-vis one another is evidenced by Thomas Kuhn’s development, especially in his later work, of a notion of scientific inquiry in which key notions and terms are plainly *untranslatable* between perspectives (which is what “incommensurability” *literally* means), but the scientific community at any given point in time possess a set of “shared and justifiable, although not necessarily permanent” standards for choosing between perspectives on the grounds of their utility in solving particular puzzles (Kuhn 2000, 76).

susceptible of evaluation, because they are at the very least trying to occupy the same conceptual terrain.

Dewey also gives some helpful advice for the construction of such a classification scheme.

The teleological theory of classification does not therefore commit us to the notion that classes are purely verbal or purely mental. Organization is no more merely nominal or mental in any art, including the art of inquiry, than it is in a department store or railway system. The necessity of execution supplies objective criteria. Things have to be sorted out and arranged so that their grouping will promote successful action for ends (Dewey 1920, 154).

Two important procedural suggestions emanate from this observation. First, and in line with calls to bring *practice* back in to the analysis of social action (Neumann 2002), analysts neither should nor need to invent a classification scheme from scratch. Instead, analysts can and should take their bearings from extant classificatory practices, seeking only to bring some abstract order to the sorts of things that already and empirically going on in the social domain under investigation. Applied to the present task, this means that we should take our bearings for a classification of positions in the philosophy of social inquiry in IR from the existing contrasts and distinctions that active IR scholars in fact draw in their work. But second, analysts need not be bound to simply reproduce or redescribe extant social practices; instead, and much like the skilled craftsman in any other field of activity, scholarly analysts can and should abstract from particular practices in order to forge more useful tools for accomplishing specific purposes (Dewey 1920, 55). Hence the challenge is not to simply get various positions in

the philosophy of social inquiry “right” (whatever *that* might mean operationally); the challenge is to abstract from existing controversies so as to focus them and ultimately make them more productive.

Core Wagers

With that by way of prelude, let me now offer a methodological principle and a provisional set of distinctions that, when combined, form what I believe is a useful typology for the discussion of the philosophy of social inquiry in IR. The methodological principle is that we should regard positions on the character and conduct of social inquiry to rest on provisional commitments—*wagers*—about matters that can really never be settled definitively.⁸ “What is the nature of Being?” and “what is the purpose of human existence?”, to give two of the best-known examples, are the sorts of ontological/theological/ethical questions to which particular scholars give answers that depend, in the final analysis, on a measure of *faith*, precisely because they can’t be revolved empirically or rationally. But commitments of this sort undergird every instance of empirical research, implicitly shaping what its goals are thought to be and how it goes about trying to accomplish those goals. Even the most flat-footed

⁸ By referring to these commitments as “wagers”—a term that emerged in conversations between Dan Nexon and myself as we engage in our ongoing work on the character of theoretical debate in IR—I am deliberately remaining agnostic about the question of whether any particular scholar can or should subject their basic commitments to criticism and possible alteration in the course of debate and discussion. In principle, IR scholars ought to change their wagers when appropriate, but in practice, few do. Whether this constitutes evidence for the immature character of IR as a science, or simply reflects a sociological fact about the organization of the field into an academic discipline, is a matter that I do not want to get into here.

empiricist has implicitly decided that reality is made up of tangible, measurable stuff and that true knowledge consists in discovering how that stuff is related so that knowledgeable humans can better come to know their true limitations, and it is a measure of the conceptual and philosophical poverty of the field that we rarely if ever acknowledge, let alone *discuss*, these commitments. Instead, we focus on technical application, obscuring the world-constituting guesses that animate those technical procedures.

Wagers constitute worlds, in that they quite literally set the stage for the kinds of empirical and theoretical puzzles and challenges that a scholar takes to be meaningful and important. If one does not believe that the purpose of social science is to contribute to human emancipation, then the deplorable living conditions of much of the world's population at the present time, or the impacts on daily life wrought by the increasing interconnectedness of global financial markets, look very different than they do to a scholar who believes—as James Bohman does—that “the social sciences play a special role in not only reconstructing...communicative capabilities, but also in developing reflexivity sufficient to allow speakers to make manifest the limitations of existing discursive practices” (Bohman 2002, 507). At a minimum, a wager locates and specifies three things: the researcher, the objects of research, and the character of the relationship between them. Bohman's critical-theoretical stance, for example, separates researcher from social actors to the extent that the researcher is empowered to introduce or induce, through the practice of social science, changes in existing practices that are intended to disclose the deficiencies of those practices as ways of approximating a broad and subtle

notion of democracy. It also upholds the researcher's privileged—because social-scientific—grasp on the normative goal of democracy, even if the actual working-out of that ideal in practice depends on collaboration with social actors and even if that normative ideal is transcendently related to the actual practices of social actors rather than being handed down from some ideal realm *a la* Immanuel Kant.

To put this a slightly different way, Bohman's position combines two analytically distinct wagers. The first involves the relationship between the researcher and the world, and speaks to the question of whether the objects of study have a more or less determinate essential character that is separate from the researcher's activity, or whether the process of empirical research in some sense constitutes the object of study *en passant*, in the course of gathering and assembling data. Critical evaluation of a set of social practices seems to call for the first answer rather than the second one, as it is difficult to conceptualize the standpoint from which a social-scientific researcher claiming social-scientific authority⁹ could possibly critique existing practices without some detached ground from which to launch such critiques. The second wager involves the kind of knowledge to which the social scientist is thought to have access, which in this case is super-empirical or transcendental (albeit in the complex, Habermasian sense

⁹ In other words, a social scientist *qua* social scientist, as opposed to as a participant, who would be perfectly able to utilize the existing practical/moral (Shotter 1993, 54-56) discursive resources characteristic of the community in question to affect changes, so to speak, *internally*. The issue in question is whether the advocate in question approaches the issue claiming some general or universal authority, or whether she or he utilizes local resources and focuses only on a local goal. The issue is made much more complex by the fact that 'universal validity' has become a publicly-available rhetorical commonplace in much of the world as a result of the twin processes of the Enlightenment and European colonialism, but for present purposes it is important to remember that we are concentrating on the justifications and arguments proffered by scholars in defense of their scholarship—arguments that are in the relevant sense local to the scholarly IR community.

of that term) rather than confined to the empirical or experiential sphere. Together, these two wagers produce an image of knowledge-production and an account of scholarly social-scientific practice that make possible the kind of critical emancipatory activity that Bohman argues should characterize more of IR scholarship.

Monism/Dualism and Experiential/Transfactual

Not by accident, these two wagers seem to me to constitute two of the most important philosophical commitments made by IR scholars, and suitably abstracted they provide a useful way of clarifying debates about the philosophy of social inquiry in the field. The first wager—concerning the relationship or connection between the researcher and the researched world—presents an ideal-typical choice between *dualism* and *monism*.¹⁰ The former option maintains a separation between researcher and world such that research has to be directed to properly crossing that gap, and valid knowledge must in the end be related to some sort of accurate correspondence between empirical and theoretical propositions on the one hand and the actual character of a mind-independent world¹¹ on the other. The latter, on the other hand, maintains that the research is a part of the world such that speaking of “the world” as divorced from the activities of making sense of it is literally nonsensical: “world” is endogenous to social

¹⁰ I elaborate this distinction in some detail in Jackson (forthcoming 2008).

¹¹ Note that “mind-independent” here means “independent from the mind of the researcher,” and should not be confused with the principle of scientific materialism that maintains that the only things that are real are those things that exist independent of *all* minds. There is no conceptual or philosophical problem with maintaining a dualist stance on a set of social relations, since this simply means that those social relations are thought to exist in a more or less determinate way separate of the investigator’s scholarly research activity.

practices of knowledge-production, whether these practices are scholarly or not, and scholarly knowledge-production is in no sense a simple description or recording of already-existing stable social forms.

The fact that the dualist position has often been characterized as “positivist” (Wendt 1999, 39) while the monist position is often characterized as “interpretivist” (Yanow and Schwartz-Shea 2006) is one of those examples of a less-than-useful classificatory scheme that does not really clarify the issues at stake in the philosophical distinction.¹² Despite the best intentions of many who use this distinction, the result of contrasting “positivist” and “interpretivist” scholarship seems to be the kind of faux “synthesis” advocated by David Laitin (Laitin 2003) in which participant-observation and other experience-near modes of data collection are assigned the role of gathering raw materials for the testing of nomological hypotheses (see also (King, Keohane, and Verba 1994). “Positivist” versus “interpretivist” is collapsed into a difference of *method*, rather than a difference of *methodology*, and the key wager of more anthropological modes of knowledge-production about our hook-up to the world is obscured. The only way to avoid this is to clarify the terms of the distinction more clearly, something that my terminological shift is designed to do, both by avoiding the “interpretivism-as-raw-materials-gathering” misunderstanding presently operative in large parts of the field and by refocusing attention on the ontological *and* epistemological issues at the heart of the distinction.

¹² The fact that this use of the term “positivism,” although all-too-common in IR, borders on philosophical absurdity is another thing that weighs against the utility of this distinction.

I emphasize “ontological *and* epistemological” because in large parts of the philosophy of (social) science narrowly understood these dimensions are separated out. The correspondence theory of truth (statements are true if they accurately mirror existing states of affairs), for example, is often discussed separately from the question of the existence of a mind-independent world, with the former issue classified as an “epistemological” question while the latter is an “ontological” question. Because so much of the recent flowering of philosophy of social science discussions has been furthered by critical realists drawing on Roy Bhaskar, this distinction has found its way into IR in the form of the commonplace realist claim that we should put “ontology before epistemology” and refrain from conflating our ability to know a thing with the question of whether or not that thing exists (Wight 2006, 2-3). But the question of the relationship between the mind and the world is *both* an ontological *and* an epistemological issue; an account of how we know that statements are true implicitly presupposes an account of how observers are related to things observed, *and vice versa*. Bhaskar’s claim is not particularly useful for bringing order to IR debates, inasmuch as the IR debates are about the general parameters of knowledge-production rather than about more or less technical issues within a different discipline. Putting ontology first, or focusing on procedures of verification and falsification as a way of demarcating scientific knowledge, are the kinds of intellectual operations that make sense when one is trying to account for the relative success or failure of particular efforts to construct knowledge. That is not generally the goal of philosophy of science discussions with IR, which are instead concerned with how we ought to do empirical research on global

politics; as a result, IR discussions of these issues inevitably cover both ontological and epistemological issues, and separating them accomplishes little.¹³

The dualism/monism wager, however, is not the only core wager that we need in hand in order to usefully order contemporary IR debates about social inquiry. After all, both falsificationists like King, Keohane, and Verba (in common with the vast majority of American IR scholars and political scientists, protestations about mechanisms and “qualitative” strategies of inference to the contrary) and critical realists like Wight and Bohman are dualists inasmuch as they posit an external world to which knowledge in some sense approximates, but there are clearly important differences between falsificationists and critical realists.

The second core wager, one that separates these two perspectives, involves the question of whether knowledge is purely related to things that can be experienced and empirically observed, or whether it is possible to generate knowledge of in-principle unobservable objects. Following language introduced by Bhaskar (Bhaskar 1998), I will refer to the position that maintains the possibility of knowing things about in-principle unobservables *transfactualism*, since it holds out the possibility of going beyond the facts to grasp the deeper processes and factors that generate those facts (Wight 2006, 18). The opposite position, *experientialism*, maintains to the contrary that it is neither necessary nor possible for researchers to “transcend experience by some organ of unique character that carries [them] into the super-empirical” (Dewey 1920, 77) – that knowledge, to the

¹³ In addition, the separation of ontology and epistemology has the potential to produce what Fred Chernoff (Chernoff 2005) refers to as the “ontological fallacy,” in which a set of entities and relations are posited at the outset of an empirical analysis but are then dogmatically maintained in the face of contrary evidence.

contrary, is a matter of organizing past experiences so as to forge useful tools for the investigation of future, as-yet-unknown situations (Dewey 1910, 126-127). Between them, transfactualism and experientialism define the parameters of this second wager, which both differentiates falsificationism from critical realism *and* illustrates a hitherto generally unremarked affinity¹⁴ between falsificationist and pragmatist approaches to knowledge-production in their common rejection of the idea that in-principle unobservable theoretical abstractions or notions have anything but an *instrumental* character when it comes to the production of knowledge.¹⁵

Putting these two wagers together generates the following 2x2 table:

¹⁴ Something like this was noted by Colin Wight and Heikki Patomäki (2000, 216-218) in a slightly different context, as they were drawing a parallel between “positivist” and “post-positivist” scholarship.

¹⁵ The phrase “in-principle unobservable” is critical, since it will hopefully prevent an important misunderstanding from continuing to gain currency in the field. Whether something is in-principle unobservable is different from a) the question of whether it has been actually observed by anyone yet; b) the question of whether it can be observed with unaided human-normal senses or whether it requires specialized equipment to be observed; and c) the question of whether a non-human could observe it. The first issue is an empirical question—whether something has in fact been observed or not—and the denial of in-principle unobservables should not be taken to imply a limitation of possible experience to its present boundaries. Likewise, the second issue—which is more properly a question about the meaning of “observation”—has no bearing on whether we could generate knowledge of things that we could not even observe *in principle* regardless of our specialized equipment. The third issue is similar—think here of the employment of dogs to smell things that ordinary human senses cannot detect—and likewise entirely beside the point. What defines something as in-principle unobservable is the claim that there is *no way* that it could be directly observed or experienced, that its reality is purely transfactual such that all we can see are its putative effects in the world—effects that we can use to abduce its existence, but nothing more. Critical realists, especially, claim that we can have valid knowledge of social structures (like capitalism) which cannot be observed even in principle, a position very much unlike that of a physical scientist who posits the existence of something that she or he cannot observe *yet* and then proceeds to construct an apparatus that will allow her or him to observe it. “I haven’t experienced this” is *not* the same as “I can’t, *even in principle*, experience this.” The transfactualist claim is that we can have knowledge of both of these kinds of entities or forces; the experientialist would permit the former, but scrupulously bar the latter.

		Relationship between the knower and the known	
		dualism	monism
Type of knowledge / objects of knowledge	experientialism	neopositivism: KKV [what we mistakenly call “positivism” in IR nowadays]	pragmatism
	transfactualism	scientific or critical realism: Bhaskar, most Marxists and post-Marxists, critical theorists	reflexivity: Bourdieu, some feminism(s), Edinburgh “strong program”

Space prevents me from elaborating on the “reflexivity” box at the moment; suffice to say that this is where any perspective on social knowledge that emphasizes the importance of reflexivity as a means to transcend the merely empirical and generate some kind of more firm or solid knowledge of unobservable generative structures like “patriarchy.” Space also prevents me from saying much about the contemporary terminological chaos surrounding the term “positivism” in IR; for the moment, suffice

to say that nobody present identified or self-identifying as a positivist in IR is particularly concerned with the elimination of metaphysical postulates so as to produce a more descriptively accurate scientific language from which true postulates can be deductively derived.¹⁶ Instead, I want to talk about the “pragmatism” box, and about Max Weber’s location within it.

II. Weber and the Pragmatists

My labeling of the “monist experientialist” box as “pragmatist” should not be particularly controversial. An anti-representationalist (Bellamy, 485-486) or holist (Festenstein, pp. 551-553) epistemology—a commitment that covers roughly the same conceptual ground as my term “monism”—is widely acknowledged to be central to pragmatist thought. The basic idea here is that a pragmatic approach to knowledge-construction dissolves the hoary old philosophical distinction between the observer and the observed in favor of a focus on what Dewey calls an “interaction situation”:

An experience is always what it is because of a transaction taking place between an individual and what, at the time, constitutes his environment, whether the latter consists of persons with whom he is talking about some topic of event, the subject talked about being part of the situation; or the toys with which he is playing; the book he is reading...or the materials of an experiment he is performing (Dewey 1938, 43-44).

This transactional character of experience, the way that experience happens somewhere *between* the individual having the experience and the environment in which

¹⁶ I am presently working on a book that will flesh out all four quadrants of the chart.

she or he is situated, effectively demolishes the idea that the construction of knowledge could in any meaningful sense involve the drawing of an accurate picture of a mind-independent world, replacing it with the far more fluid notion of knowledge as an assemblage of facts and theories that are efficacious in bringing about a result. Pragmatism thus dispenses with both “the mind” and “the world” as separate entities, replacing both with the notion of *experience* (concrete and situated) as the substrate out of which both things and thoughts are made. “Thoughts in the concrete are made out of the same stuff as things are,” William James declares, and no entity called “consciousness” exists (James 1978, 183). A more monistic point of view would be hard to imagine.

Similarly, the scorn heaped by pragmatist thinkers on any and all attempts to claim knowledge of a transfactual character should be obvious to basically any reader of the relevant philosophical texts. Richard Rorty’s observation that “the idea that there is One Way the World Is and One Truth that corresponds to that Way” (Rorty 2001, 45) doesn’t add anything meaningful to a set of empirical claims about a particular situation is a representative example of the way that pragmatists typically deal with claims by scholars that they have somehow transcended experience to grasp something True. Much like the later Wittgenstein (Wittgenstein 1953), pragmatists look at such claims in terms of their concrete effects, and generally conclude that the effect is either negligible or positively harmful: negligible if the claim is just a bit of verbal ornamentation, and harmful if it either constricts future experiences by telling an investigator not to go any further than what we already know to be True (Dewey 1938,

25-26), or if it becomes a justification for bringing to heel those who refuse to acknowledge the Truth—by force if necessary (Rorty 2001, 50). The solution to both of these problems, for pragmatists, is to stop looking for a Truth that transcends experience—to move from “contemplative” to “practical” knowing (Dewey 1920, 121)—and to focus instead on the rational ordering of experience so as to produce the possibility of better, richer experiences in the future. From here we get the common pragmatic habit of evaluating ideas based on their contribution to human happiness (Rorty 2001, 47-48), a stance that completely sets aside any kind of transfactual claims whatsoever.

In philosophical and methodological terms—in other words, as appropriate for a discussion of how to conduct empirical IR research—the twin commitments of monism and experientialism jointly constitute the pragmatist stance. But although I take this to be more or less uncontroversial, the fact that I want to locate Max Weber in this box will probably raise more than a few skeptical eyebrows. In part, this stems from the fact that Weber is regularly misunderstood in various English-language literatures as having promoted the notion that there could be such a thing as universal or classically objective value-free knowledge; that the purpose of social and historical inquiry was to construct great nomothetic schemes and stage-models of developmental processes; and that the point of empirical investigation was to shed light on the dynamics of deep structural phenomena like “capitalism” and “modernity.” Although these and related positions are attributed to Weber innumerable times in the scholarly literature, they are all wrong—or at the very least, they are all deeply problematic readings of Weber’s work.

Undoubtedly some of this stems from the translation ambiguities introduced by Talcott Parsons and others who produced the first English-language editions of Weber's work (Erdelyi 1992), but the misunderstandings have persisted even though better translations have long since become available. While not surprising, it is unfortunate, because Weber's work lends itself quite easily to a considerably more pragmatist reading; indeed, Weber's rather more pessimistic take on the powers of reason and science to reconstruct (Dewey's word) the ends of human social action provides a sobering contrast or corrective to some of the early American pragmatists' more grandiose claims. In the interests of both textual fidelity and the development of a pragmatist turn in social inquiry, then, Weber ought to be placed into the "monist experientialist" box.

Weber as Monist

Weber's monism is perhaps most clearly expressed in his famous comment on the "objectivity" of social-scientific analysis:

There is simply no "objective" scientific analysis of cultural life—or, put perhaps somewhat more narrowly but certainly not essentially differently for our purposes—of a "social phenomenon" *independent* of special and "one-sided" points of view, according to which—explicitly or tacitly, consciously or unconsciously—they are selected, analyzed, and representationally organized as an object of research (Weber 1999, 170, emphasis in original).

The goal of the social sciences *cannot* be to neutrally reflect an externally existing world. Instead, Weber places the human "capacity and the will to deliberately take up a

stance towards the world and to lend it a *meaning*” at the center of his reflections (*ibid.*, 180). “The quality of a process as a ‘socio-economic’ event is not something that inheres ‘objectively’ in the process as such,” Weber argues; this is a rather surprising statement to make in the editorial introduction of a journal devoted to what we would now probably call the analysis of political economy. “It is far more conditioned by the direction of our knowledge *interest* as it arises from the specific cultural significance that we attribute pertaining to the process in an individual case” (*ibid.*, 161). In this way, the social sciences are *productive* of the world, beholden not to some externally existing set of objects or their essential dispositional properties but rather to the cultural values that define and orient the investigation from the beginning.

As is proper to a monistic ontology, Weber’s position – that cultural values shape the ways that the world appears to the researcher – extends even to the level of the most basic *description* of a phenomenon; there simply is no apprehendable “world” (or series of externally existing objects) that could be used to limit the application of any particular theoretical concept, let alone to falsify a theory by contrasting it to “reality” (which is the typical dualistic research strategy of hypothesis-testing). But it does not follow that theoretical concepts are somehow true by definition and never subject to refinement, as principles of an ideology would be. Concepts and theories are indeed hypothetical in the sense that Dewey uses the term (Dewey 1920, 145), in that they are guesses about phenomena and relationships rather than representational copies of them – and as such are always provisional rather than final.

Weber refers to concepts and theories as *ideal-types*. Rather than “a ‘presuppositionless’ copy of ‘objective’ facts,” ideal-types are

formed through a one-sided *accentuation* of *one* or *more* points of view and through bringing together a great many diffuse and discrete, more or less present and occasionally absent *concrete individual* events, which are arranged according to these emphatically one-sided points of view in order to construct a unified *analytical construct* [*Gedanken*]. In its conceptual purity, this analytical construct [*Gedankenbild*] is found nowhere in empirical reality; it is a utopia (Weber 1999, 191).

As I discuss in more detail below, an ideal-type is a deliberately partial way of configuring the world, arising from a subtle combination of empirical observation and the value-commitments made by the researcher. What one *does* with an ideal-typical concept or notion is to apply it to a concrete situation in a more or less instrumental manner, in order to illuminate particular aspects of a given empirical situation. Weber is quite clear that the construction of ideal-types is a means rather than an end in itself, further distancing his position from the dualist knowledge-production strategy of steadily, whether through induction or deduction or some combination of the two, building up a more and more accurate picture of the world. Indeed, Weber even points out that “whoever accepts the standpoint that knowledge of historical reality should or could be a ‘presuppositionless’ copy of ‘objective’ facts will deny any value to ideal-types” (*ibid.*, 192-193). Or, to rewrite Weber’s claim in my terms: a dualist would reject ideal-typification, since it is a monistic strategy of inquiry.

Weber as Experientialist

When discussing ideal-types, Weber continually asks about the value of such things for an “*Erfahrungswissenschaft*.” Literally, the German word means “experience-science,” which thus places the orientation towards experience squarely at the heart of Weber’s methodology. Unfortunately, the most popular English translation of Weber’s famous “objectivity” essay (Weber 1949) renders the term as “empirical science,” a slippage in meaning that quite torpedoes Weber’s manifest intent both in using the word and in emphasizing the first part of the word. While experience is in a certain sense empirical, an orientation of knowledge towards the empirical tends to sound like an orientation of knowledge towards objects and entities that exist somewhere beyond the knowing subject: “empirical” opposed, in a way, to “fanciful” or “hypothetical.” Unlike a focus on experience, which keeps the analytical center of attention squarely fixed on the transactional relations between knower and known, a focus on the empirical character of a science would tend to pose questions about the certainty of empirical claims—and this in turn opens the door for all kinds of transcendental and transfactual criteria.

In my reading, Weber’s work, and especially Weber’s methodological writings, fit more easily into the former conception than into the latter. Indeed, the central thrust of ideal-typification is towards precisely the kind of logical ordering of experience that the pragmatists sought, and the methodological attitude towards conceptual instruments is virtually the same. To begin with, Weber’s standard for the correctness of

an empirical account has almost entirely to do with its technical correctness, not with the validity of the cultural value-orientations on which the account rests.

A systematically correct scientific demonstration in the social sciences, if it wants to achieve its goal, must be recognized as correct even by a Chinese (or, more accurately, it must constantly *strive* to attain this goal, although it may not be completely reachable due to a dearth of documentation). Further, if the *logical* analysis of the content of an ideal and of its ultimate axioms, and the demonstration of the consequences that arise from pursuing it logically and practically, wants to be valid and successful, it must be valid for someone who lacks the “sense” of our ethical imperative and who would (and often will) refuse our ideal and the concrete *valuations* that flow from it. None of these refusals come anywhere near the scientific value of the *analysis* (Weber 1999, 155-156).

Weber’s “Chinese” represents any observer with value-orientations radically different than our own, rather than an observer with specific set of cultural commitments. The general point—that even someone who rejects our values should be able to appreciate the results that we produce by systematically applying those values to the study of empirical reality—remains valid regardless of the specific differences of value-orientations involved. Application becomes a more or less technical question in Weber’s conception, and it is on *this* basis—and *not* on the specific content or character of the value-orientations thus applied—that the “scientific” character of an investigation can and should be evaluated.

But this also means a rejection of any super-experiential standard for either the conduct or the evaluation of social-scientific scholarship. Precisely because there can’t be definitively correct values or value-commitments, the only remaining arena in which to judge whether a piece of scholarship is a good one is in the realm of concrete social

experience. A notion like “class struggle” or “economic rationality” or “state sovereignty” can only be meaningfully evaluated in terms of what kinds of insights into concrete cases it generates, and any purely conceptual or theoretical elaborations of these or any other ideal-typical notions only have scientific merit inasmuch as they help us make sense of actually-experienced situations: either situations we experience ourselves, or situations for which we have the records of the experiences of others. According to Weber, the value of an ideal-type lies precisely in its being “entirely used as a means for the *comparison* and *measurement* of actuality,” and the conceptual error to be avoided involves the slippage from ideal-type to putatively super-experiential standard:

In this sense the ideas are naturally no longer purely *logical* devices, and no longer concepts, with which reality is comparatively *measured*. They are instead ideals by which it is evaluatively *judged*. Here it is *no longer* a matter of a purely theoretical process of *referring* to values empirically, but instead of value-*judgments* which have been taken over into the “concept” of Christianity. *Because* the ideal-type claims empirical *validity*, it towers into the region of the evaluative *interpretation* of Christianity. The ground of experiential science is forsaken; before us stands a profession of faith, and *not* an ideal-typical *conceptual* construct (*ibid.*, 199).

“Professions of faith” – faith in a transfactual grounding for knowledge-claims – are precisely what Weber wants to remove from social science. This is less about removing *values* from social science than it is about removing claims to transcendental (or, to use Kant’s word, “apodictic”) certainty, which is precisely what pragmatists like Dewey and James also sought to do by returning philosophy to the sphere of the

experiential. We can only know what we can experience, and abstract theorization should be regarded instrumentally, as a means to that end (Dewey 1920, 150).

There is, however, one important difference between Weber and these early pragmatist thinkers. John Dewey, especially, was a great believer in the progressive power of reason to increase human freedom, largely by subduing both nature and society and giving human beings “power to frame *new* ends and aims and to proceed in regular system to their actualization” (Dewey 1920, 72). Social progress, in Dewey’s conception, would come from the application of scientific principles to ethical and political affairs; this would allow the informed engineering of conditions so as to promote human happiness. It is this faith in progress that comes closest, in the writings of the early pragmatists, to an abandonment of the sphere of the experiential: suitably impressed by the achievements of the natural sciences in their sphere, early pragmatist writers reasoned analogically (or optimistically extrapolated a trend-line) that scientific reason would be equally efficacious at solving ethical and moral problems. In fact, Dewey looked forward to a scientific “moral progress” which would remake politics, education, and morality along more rational lines, and often referred to his philosophy as a contribution to this end (*ibid.*, 125-126).

Here is a place where Weber, ironically, is a better experientialist than the pragmatists, since he is meticulous about wanting to confine reason and analysis strictly to the experiential realm. Because of this experiential character of knowledge, Weber is doubtful that scientific analysis can ever settle questions of values, comparing the contrast of different value-orientations to a conflict among gods (Weber 2004, 23);

science cannot resolve such conflicts, but can only clarify the choices that must be made by anyone professing adherence to any particular set of values:

If you take up this or that attitude, the lessons of science are that you must apply such and such means in order to convert your beliefs into a reality. These means may well turn out to be of a kind that you feel compelled to reject. You will then be forced to choose between the end and the inevitable means. Does the end “justify” these means or not? The teacher can demonstrate to you the necessity of this choice. As long as he wishes to remain a teacher, and not turn into a demagogue, he can do no more (ibid., 26).

Science, accordingly, is confined to the clarification of values, and strictly prohibited from claiming or attempting to alter or improve them. Weber’s cautionary limitation of science is based both on a logical claim that has to do with the character of ideal-typical knowledge and its relation to value-commitments, and on an empirical claim about one celebrated case of the rationalization of values—the consolidation of the “spirit of capitalism” (Weber 2002, 123-124)—and the kinds of disastrous effects that process had on the world (“until the last ton of fossil fuel has burnt to ashes”) and the individuals inhabiting it (“narrow specialists without mind, pleasure-seekers without heart; in its conceit, this nothingness imagines it has climbed to a level of humanity never before attained”). Weber regards even these judgments to be outside of the realm of “purely historical analysis,” however, since they rely on the acceptance of a common set of value-standards within which such judgments would be comprehensible; all that the responsible scientist can do is to present the empirical case, and let people judge for themselves what to make of it.

But this does *not* mean that Weber believes in the possibility of knowledge divorced from value-commitments. Indeed, the whole procedure of ideal-typical analysis is all about the transmutation of cultural values into useful analytical tools, and the precise character of this transmutation underpins Weber's logical claim that science cannot possibly resolve value-questions. Ideal-typical analysis, Weber's main methodological innovation, has the potential to really cash in on the value of a pragmatic, monist-experientialist approach to knowledge-construction, and to ensure that pragmatism in IR is not simply a warrant for a fuzzy "anything goes" eclecticism or pure normative critique.

III. Ideal-Typical Analysis as a Pragmatic Methodology

Weber's methodology of ideal-type analysis is considerably more complicated than most commentators, especially those in the American academy, acknowledge. Indeed, such is the dominance of neopositivism in the present life of the social sciences that critical distinctions, like the difference between an ideal-type and a variable attribute, or between constructing a causal configuration and testing a hypothesis, are frequently elided—with the result that Weber's methodological innovations are generally overlooked. This is unfortunate, and can really only be remedied by a re-articulation of Weber's position from the ground up. Critical to such a re-articulation is the recognition that Weber's methodology has two separate but related components: the formation of ideal-types in the first place, and the configurational application of those

ideal-types to concrete cases in what Weber called “singular causal analysis.” Ideal-typical analysis properly understood incorporates both of these components.

The whole procedure can be summarized in the following table, in which three distinct intellectual moves link four elements:

A		I		B		II		C		III		D
Sphere Values	of	<i>Stand-taking</i>		Value- commitment(s)		<i>Formalization</i>		Analytic		<i>Application</i>		Facts
The Weberian Procedure of Ideal-Typification												

Reading from left to right, the procedure begins in (A) the concrete sphere of values and norms in which the researcher is located. Ideal-typification begins with that researcher (I) taking of a value-laden ethical stand, by which she or he locates him- or herself with respect to the values and norms in circulation in her or his social context; the (B) value-commitment(s) contained in this stance are then (II) formalized and abstracted, in part by blending them with empirical observations in order to create limiting-case representations, in order to produce an analytic consisting of multiple ideal-types (C). Then that analytic is (III) consistently applied to specific empirical cases in order to produce (D) facts: “particular ordering[s] of reality in terms of a theoretical interest” (Easton 1953, 53). Hence the facts produced are both dependent on and distinct from the value-commitments that ground the ideal-typical analytic in the first place: dependent on because particular factual knowledge-claims are generated by a conceptual apparatus with its roots in specific value-commitments, but distinct from because it is possible to evaluate a given piece of research and the facts it produces by

focusing our attention on intellectual moves II and III and essentially ignoring intellectual move I and the specific contents of element B.

The Formation of Ideal-Types

Weber compares the process of forming an ideal-type to the construction of a limit in calculus, or to the depiction of a utopia by abstracting from and then deliberately exaggerating features of the object of class of objects that one wants to analyze.

An ideal-type is formed through the one-sided *accentuation* of *one or more* points of view and by the synthesis of a great many diffuse and discrete *individual* phenomena, present sometimes more, sometimes less, and occasionally not at all, which are arranged according to those one-sided emphatic points of view into a unified construction *in thought*. In its conceptual purity, this thought construction can be empirically found nowhere in reality, it is a utopia, and *historical* research has the task of determining in each individual case how near or how far reality stands from this ideal picture (Weber 1999, 191).

Several things are noteworthy in this description of the process. First, ideal-types are nothing like pictorial representations of objects or entities; they are more like deliberate caricatures or partial sketches, or perhaps specialized conceptual filters that focus our scholarly attention on particular aspects of actually existing things to the detriment of other aspects of those same things. Second, this deliberate slant is less due to any putatively dispositional characteristics of the object under study, and more to the “emphatic points of view” – what I have labeled “value-commitment(s)” (B) in the diagram above – which, in a sense, *direct* us to focus on particular aspects and not

others. From this it follows, third, that a different researcher, formalizing different value-commitments into a different analytic, might well focus on different aspects of the same entity or object, and they would not in any simple sense be “wrong” for doing so (*ibid.*, 192).

Finally, since ideal-types cannot be falsified as one would falsify a hypothesis—since comparing an ideal-type to the actual existence of the object the ideal-type was derived from would *invariably* “prove” that the ideal-type was descriptively deficient in some respect¹⁷—the only meaningful way to evaluate whether an ideal-type is a good one or not is *pragmatically*: i.e., to examine whether, once applied, the ideal-type is efficacious in revealing intriguing and useful things about the objects to which it is applied. This last observation might be thought of as the Weberian equivalent of what Dewey calls “the pragmatic rule” —“in order to discover the meaning of the idea ask for its consequences” (Dewey 1920, 163)—translated into the sphere of inquiry. A good ideal-type is a useful ideal-type, an appropriate means to the analytical end that animates the scholar’s scientific activity. Hence it is quite literally nonsensical to speak of an ideal-type as being “valid” or “invalid,” because in the sphere of scientific analysis these terms cannot be applied to analytical constructs—only to applications, and then only in a technical sense.

¹⁷ Along these lines, note that every theorist of bureaucracy who begins their analysis with a ritualistic pronouncement that actual bureaucracies don’t function like Weberian ideal-typical bureaucracies is, quite bluntly, *missing the point*. Of course real bureaucracies don’t look and function precisely like their ideal-typical conceptual limits. It would be quite surprising if they did, and it would most likely also mean that the supposed “ideal-type” was in actuality no such thing.

It should go without saying that ideal-types are in no way *ideals*, and cannot serve as the basis for moral judgments on or indictments of concrete social phenomena. An ideal-type doesn't even function quite like Dewey's practical utopias, which envision presently unavailable possibilities and serve as "platform[s] from which to scrutinize natural events" so that "things disclose properties hitherto undetected" (*ibid.*, 120), even though there is a similarity in that both Deweyian practical utopias and Weberian analytical utopias provide points of view from which the world and the entities and relationships within it appear in a certain way. The difference here lies mainly in the end to which the different utopias are put: Dewey is concerned with affecting concrete change in the world, while Weber is more concerned with developing knowledge of the world that can be used to check and discipline runaway political idealism. But because both of these techniques are instances of enacted monistic experientialism, their similarity is not surprising.

Singular Causal Analysis

Once one has a set of ideal-types in hand, the analytical work is only half-finished. Simply constructing a set of ideal-types does little to increase or improve knowledge, because ideal-types are necessarily *general* (abstract) while actual experienced reality is composed of *particular* (concrete) situations. If we are to have a "science of actuality" (*Wirklichkeitswissenschaft*), then we have to use these abstract notions to illuminate the concrete situations in which we are ultimately interested

(Weber 1999, 170-171)—because, as I have argued, Weber’s experientialist position would look with disfavor, if not scorn, on any discussion of ideal-types as though they were anything other than instrumental devices through which the world may be investigated.

Unsurprisingly, this is almost exactly what Dewey argued we ought to do with general principles:

...abstraction means that something has been released from one experience for transfer to another....Abstraction sets free some factor so that it may be used. Generalization is the use. It carries over and extends. There can be no assurance in advance that what is extracted from one concrete can be fruitfully extended to another individual case (Dewey 1920, 150-151).

Or, as he puts it elsewhere, abstract theories are “tools of insight; their value is in promoting an individualized response to the individual situation” (*ibid.*, 169). Conceived of as tools of inquiry rather than as tools of concrete political action (a distinction to which I will return in my Conclusion below), this means that abstract theories are nothing more and nothing less than analytical templates for making social-scientific sense out of a given phenomenon or entity or sequence of actions. The challenge for the social scientist is to apply these templates to concrete instances in a way that preserves their necessarily oversimplified, ideal-typical character but still allows us to substantiate causal claims about the significance of events.¹⁸

¹⁸ That a pragmatic social science should be causal is not a matter I have time to take up at the moment. Suffice to say that Dewey, James, Pierce, and Whitehead all agree that causation—albeit a kind of causation that emphasizes processes and mechanisms rather than lawlike generalizations—is central to a pragmatic account of the world, and that Weber agrees with them.

Weber's technique for doing this is not well understood, in part because the best-known English translation of the essay¹⁹ where he (tentatively) develops the procedure²⁰ is marred by the same terminological ambiguities as the most widely-used translation of the "objectivity" essay (Weber's best-known methodological piece). In addition, the argumentation is unusually dense and convoluted even for Weber, and makes use of some relatively obscure turn-of-the-last-century criminological writings in developing the argument. But certain key points can be abstracted, which when taken jointly point in a more or less definitive direction.

The first key point is that a concrete situation is never to be understood as resulting from or consisting of any one factor. Actually-existing entities, objects, situations, or sequences

can *not* be viewed as the result of a struggle between causes some of which strive towards the concrete result and some of which strive against it. Instead, the totality of *all* conditions back to which the causal chain leads from the "result" had to "act together" just so and in no other way in order to let the concrete result be realized. (Weber 1999b, 289).

The reference to causal factors struggling against one another is a reference to a notion of John Stuart Mill's that the probability of an event's occurring could be expressed or interpreted as a ratio of those factors working in favor of that occurrence

¹⁹ Perhaps it's the only one. I've never seen another one, actually.

²⁰ One of the maddening things about Weber's methodological writings is that he never really quite finishes fleshing out what he has in mind, although he lays out a general direction and spends some time criticizing alternatives. Part of this, I think, stems from the fact that Weber didn't think of himself as either a philosopher or a methodologist, but as a teacher and researcher: he would rather *engage* in knowledge-production than *talk about* it. If we take a pragmatic orientation seriously, then this kind of subtle dismissal of philosophy and methodology might well follow, making it all the more amazing that we have detailed writings from people like Dewey and James about the *philosophical* character of pragmatism (and helping to explain why Rorty is, last time I checked, teaching literature rather than philosophy).

to those factors working against it; if the ratio tilted in one or another direction, the event either would or would not occur (*ibid.*, 288). This idea might sound a bit obscure were it not for the fact that this is *precisely* the causal logic at the core of regression analysis: take an event, decompose it into causal factors by expressing it as a function of sum set of independent variables, and then—by examining multiple cases—estimate the coefficients for each independent variable, with each coefficient capturing the independent impact that the particular causal factor has on the outcome. The “struggle,” then, is between those variables with positive coefficients and those with negative coefficients, and the probability of the function taking on a particular value in some case is plainly contingent on the summed effect of those coefficients.²¹

Fritz Ringer to the contrary, Weber’s approach to causation is *not* the same as that modeled by a regression equation (Ringer 1997). Indeed, Weber’s configurational approach is in many ways the precise *opposite* of this kind of causal analysis so favored by neopositivists. Weber is quite dismissive of the idea that the operation of causality in the actual world (as opposed to, say, in the deliberately contrived situations of rolling dice and drawing colored balls out of urns of which people interested in the mathematics of probability are so fond) can be meaningfully captured in quantitative form:

One isolates those causal components of which the judgment of a “possible” result is composed, as distinct from the totality of all of the other conditions that are *possibly conceivable* as acting together

²¹ Whether we work with a discrete measurement of the dependent variable in question (e.g. “occurrence”/“nonoccurrence”) or a continuous measurement (which would permit us to talk about *degrees* of occurrence) is quite immaterial. The basic conceptual point remains unaffected.

with them. Then one asks how the collection²² of all of those conditions with the addition of which those conceptually isolated components are “suitable” to precipitate the “possible” result relates to the collection of all of those conditions with the addition of which they are *not* “expected” to precipitate it. Naturally, a “numerically” (in any sense) estimable relationship between these two “possibilities” absolutely in no way results from this operation (Weber 1999b, 284).²³

The “isolation” in question does not result from a mathematical operation at all. Rather, Weber advises that researchers begin with “the creation of—let us say it calmly:—*imaginative pictures* through the disregarding of one or more components of “actuality” that have been factually present in reality, and through the mental construction of a course of events altered in reference to one or several ‘conditions’” (*ibid.*, 275). In other words, researchers seeking to isolate causal factors should proceed by ideal-typifying causal mechanisms and processes, and then trying to imagine whether the observed result in question would actually occur if those mechanisms and processes concatenated in a different way, or if factors had been present which were not in fact present.

This imaginative isolation of factors allows an analyst to ascertain which factors are critical to the causal complex responsible for the outcome in question according to a simple rule: if one can imagine the outcome taking place despite the modification of some factor, then that factor is not part of the causal complex. It is the analyst’s

²² *Umkreis*, literally, “circumference” or “circle.” Weber distinguishes, albeit subtly, between an “Umkreis” of factors making up a particular historical situation and a “Komplex” of ideal-typically isolated factors thought to be causally related to an outcome. The point here is that one takes the ideal-typical Komplex and uses it as a way of making sense out of the case-specific contents of the Umkreis associated with an actually existing case..

²³ Weber was many things, but unfortunately, he was absolutely in no way a gifted prose stylist.

experiential feel for the material and the situation that allows her or him to render such judgments; Weber repeatedly makes reference to *Erfahrungsregeln*, “rules of experience,”²⁴ as the standard to which the analyst should refer when ascertaining whether some factor is likely to lead to an outcome or not. Weber terms those factors the combined presence of which we think will likely produce an effect regardless of what else transpires “adequate” causes of the effect. He also recognizes that some situations will be decisively influenced by unique, idiosyncratic factors, and terms those factors that were important to the production of an effect in a particular situation but not part of an adequate causal complex “coincidental” causes of the effect (*ibid.*, 286).

In both cases, it is the analyst’s experience that allows her or him to determine importance and significance; there is nothing of the mechanical procedure that is associated with the estimation of regression coefficients, but plenty that relates to the systematic organization of experience so as to shed light on novel situations. Singular causal analysis, therefore, is a *pragmatic* analytical technique: rooted in experience, opposed to transfactual claims, and thoroughly monistic inasmuch as what the analyst “sees” is a function of how she or he approaches or apprehends the situation. This application of ideal-types to concrete cases provides a grounded way for pragmatists to practice experiential social science.

²⁴ Frequently mistranslated “empirical rules” by translators desperately trying to make Weber into a neopositivist concerned with nomothetic generalization.

Conclusion: The Cash Value of Pragmatic Social Science

But should they? Is there a *purpose* to retaining the category of “social science,” and separating social-scientific knowledge-production practices from the activities of, say, participants in the political process, or activists striving to bring about a result? I think that there is a purpose to this distinction, and sufficient worth to the category of “social scientist” that we have to be very careful not to simply read the pragmatic turn as a call for all of us to simply go out and start advancing our preferred causes through our scholarship. Besides the fact that being perceived as so transparently partisan may erode the credibility on which what little authority that scholars possess in the public sphere appears to rest, there are a number of tasks that a pragmatic social science can perform that “knowledge activism,” or “political scholarship,” can not. To my mind, these tasks provide ample justification for the preservation of the distinction between “(social) science” and “politics,” as long as that distinction is understood in its proper Weberian form—which involves value-commitments as central components of the formation of ideal-types—and not in the neopositivist form that maintains some kind of absolute separation between the realms of fact and value.²⁵

First of all, a pragmatic social science can be *conservative* in the positive sense of that term: it can “conserve and not waste the values wrought by humanity” (Dewey 1920, 18) by holding those values at the very core of its research activities. In this way, those values can be more easily evaluated based on their productivity and efficacy in

²⁵ The unsustainability of the neopositivist position is one of the issues I explore in (Jackson forthcoming). I am hardly the first to do so, even in IR.

helping to shed light on various aspects of social reality, instead of remaining in some lofty realm of Pure Ethical Reflection. But at the same time, by placing value-commitments at the *beginning* of the research process, those values are in a sense insulated from particular empirical findings. Critiquing a piece of research on how its author has made intellectual moves II and III,²⁶ how she or he has formalized her or his value commitments and how she or he has applied them to empirical cases, are legitimate social-scientific operations; critiquing a piece of research for having the “wrong” values, which is an attack on intellectual move I, steps outside of the sphere of social science. Thus any number of value-commitments may remain intact, as long as they keep proving their worth as the cores of points of view that generate empirical insights.

Second, a pragmatic social science can be *critical* à la Kant: it can respect its own limitations and actively work to keep knowledge-claims restricted to their own proper domain. That domain is the world of *experience*, and keeping our sights focused there means that we can simply dispense with any and all claims to have somehow lifted the veil of the world as we experience it and penetrated through to Reality – or, at least, we can appreciate those claims for what they are and not for what their advocates sometimes claim them to be. A pragmatic social science that keeps to its own boundaries is in a way *justified* in aggressively policing the borders of knowledge, deflating metaphysical pretensions and bringing transfactual and transcendental claims

²⁶ Refer to the diagrammatic representation of the process of ideal-typification, above.

back down to the experienced world (Weber's "actuality") where they can be evaluated in terms of their practical effects. A pragmatic social science can analyze—or disclose—such metaphysical pretension without being open to the charge of hypocrisy, since if its practitioners are honest they *also* do not have any privileged access to the Real and should not be making any claims that presume such access. But if pragmatists abandoned the notion of social science as a distinct sphere of activity, then their claim would become just another political platform among others, instead of a practice of disciplining all knowledge-producing practices—including its own.

Finally, a pragmatic social science has the capacity to be *visionary*. Even though ideal-types are not the same as the kinds of practical utopias that Dewey identifies as spurs to concrete social and political action, there is no reason whatsoever that those ideal-types and the empirical research that they produce cannot serve as utopias or dystopias in the Lasswellian²⁷ sense: a vision of a possible future that can work to bring about effort either to bring that future about, or to prevent it from occurring. As Dewey points out, tool-users and tool-makers need not be the same people, and the value of tool-makers working separately from tool-users is that they can design instruments that are subtle and refined enough to be used in a variety of situations (Dewey 1920, 149). Social scientists, since they are working with cultural values and forging them into ideal-types, are precisely this sort of "tool-maker." If they stopped tinkering with tools and started just using them, where would novel analytical instruments come from?

²⁷ Reference is to Lasswell's work on garrison states. For next draft, insert actual reference!

What would happen to the stock of ideas and concepts that tool-users have at their disposal *because* of this social-scientific effort? Where would we turn to see what the world might look like if we were to adopt X set of values—what the consequences of such an adoption might look like in practice? *Those* are the kinds of things that social science at its best ought to be capable of. And that, in the end, is what a pragmatic social science can contribute.

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