

# On Acting and Knowing

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## Abstract

This article moves from deconstruction to reconstruction in epistemology and research methodology. To begin with, we show why the hope of many political scientists that warranted knowledge in practical matters can be obtained through standard social scientific methodologies is mistaken. We do so by subjecting two versions of the belief in traditional epistemological projects to critical scrutiny. First, we discuss Kant's *Critique of Pure Reason*, which for centuries provided the most developed epistemological approach. Second, we critically reflect upon the "unity of science" position by showing how a series of more recent epistemological debates in mathematics and logic have ended in impasse. Having revisited Kant's critical epistemology and other attempts to set knowledge on secure foundations, in section two we argue that the time has come for a pragmatic turn. In sections three and four we suggest that a coherent pragmatic approach consists of two elements: the recognition of knowledge generation as a social and discursive praxis, and the recognition that research should be oriented towards the generation of useful knowledge. In the last section we provide some concrete suggestions for abduction as a pragmatic research strategy, and in the conclusion we deflect predictable anxieties that our ideas imply an end to the scientific search for sound and solid knowledge.

# On Acting and Knowing

Philosophers of science and informed readers of political science have understood for decades that the traditional epistemological quest for the incontrovertible foundations of scientific knowledge is doomed to failure.<sup>1</sup> What most of us have not yet understood, however, is what consequences follow from this insight. Shall we hold on and devote more time to the search for incontrovertible foundations, even though we are aware of the elusiveness of this quest? Shall we dig in and pretend that the foundations of our knowledge are incontrovertible, although we know perfectly well that the edifice is built on sand? Or shall we cut our losses and look for some pragmatic alternative? In this paper we argue that the latter approach is the way to go, and we come up with some concrete suggestions.

Political science, and especially the disciplines of comparative politics and international relations, is developing in two problematic directions simultaneously. In some quarters, the accumulation of law-like statements is still pursued as if positivism had never been questioned. Independent, intervening and dependent variables are tossed around as if the social world was similar to a bowling alley. In some other quarters, there is a hypertrophic concern with epistemology precisely at a point where discussions in the philosophy of science have largely abandoned the traditional epistemological project to guarantee field-independent and a-temporal criteria for secure knowledge. While philosophers of science have moved from traditional epistemology to the recognition of “science” as a communicative practice among scientists, the trend in comparative politics and international relations seems to be the reverse.

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<sup>1</sup> We would like to thank the following people for their valuable comments: Xymena Kurowska, Elisabetta Brighi.

In the present article we argue that, in order to acquire useful and reliable knowledge for science and practice, we need to look for practical alternatives to standard social scientific methodologies. Since practical matters require practical treatment, the epistemological criteria that are traditionally accepted in the social sciences are inadequate. On the one hand, philosophers of science have mostly abandoned the conception that “secure knowledge” can be based on field-independent procedures. On the other hand, the ongoing practices of scientific research have little to do with the “idealizations” familiar from textbooks reconstructing a science that never was. Hence, the time has come for methodologists and epistemologists in our discipline to realize that the traditional epistemological project has failed.

There is another important reason for a pragmatic turn in methodology. Standard social scientific methodologies maximize all sorts of important values, such as parsimony and logical rigor, but they do a tremendously poor job when it comes to the decisive purpose of human cognition: the efficient and efficacious production of useful knowledge. Just imagine that, in your own social practice, you had to find your way through the world by means of, say, deductive theory testing, or the inductive accumulation of theoretical facts. You would get lost, stumble around, and eventually share the fate of the astronomer-philosopher Thales of Miletus, who fell into a well while observing the stars, and was consequently scorned at by a servant who had both feet firmly on the ground. At the bottom of our hearts, we all know that the way we produce knowledge for our own social practice has clear advantages over following the orthodox canon of social scientific methodologies.

Let us take as an example the way one learns to drive a car.<sup>2</sup> Almost everybody will agree that the decisive stage is getting acquainted with the practice of navigating through traffic. What the novice learns in driving lessons is helpful to a certain extent, but she will quickly

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<sup>2</sup> We owe this example to Kerstin Friedrichs.

find out that what really matters is driving as a social practice. What she really needs is useful frames for driving in certain classes of situations. Driving in Naples during the rush hour poses a different challenge to driving on a small country road in Nebraska. Trucks and busses move differently to mopeds and bicycles. Fellow drivers using the horn, talking on the cell-phone, or wearing melon hats must be treated with special care. Traffic is clearly not random but, as with any other social practice, it is full of contingent behavioural regularities and reasonably clear rules of behaviour.

Usually, however, we don't discover these regularities and rules of behaviour by anything even remotely resembling experimentation, deductive theory testing, or other standard social scientific methodologies. Even the way we learn to speak a language, or to play chess, is radically different from standard social scientific methodologies. The bottom line is that, in our own practice, most of us manage to deal with a lot of difficult challenges, and the way we do this is completely different from, and far more efficient than, the way knowledge is generated according to standard scientific methodologies. Science is often a poor emulator of what we are able to achieve in practice. For example, nobody would suggest that the best way of learning to play tennis or to ride a bicycle consists of learning to solve complicated simultaneous equations, although there is no doubt that a lot of kinetics and ballistics is involved in these two activities. Human practice is the ultimate miracle, and science would do well to mimic it at least in some respects.<sup>3</sup>

This is in line with the classical pragmatist predicament that science is, or should be, above all a more conscious and systematic version of the way how humans have learned to practice problem solving and knowledge generation in their everyday lives.<sup>4</sup> Our goal in this

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<sup>3</sup> As academics we seem to share the belief that only through the development of explicit "theories" are we able to produce warranted knowledge that can provide guidance for resolving our practical problems.

<sup>4</sup> For a rich and dense introduction to classical pragmatist thought see Hammersley 1989: 44-65.

article is not to erect new epistemological idols but simply to propose a regulative ideal for scientific practice to compete with less practical alternatives such as naïve truth seeking attendant to Francis Bacon or deductive theory testing in the wake of Karl Popper.

## 1. From Kant to Can't

Immanuel Kant understood epistemology as a critical reflection of reason upon its own foundations, before a “court” constituted by Reason herself to establish what is and what is not independent from the contingencies of human experience.<sup>5</sup>

This was far from a trivial move. Up until then, epistemology had claimed to provide a “meta-theory” for more specific theories in various fields. The propositions of a theory would be warranted if, and only if, they satisfied the epistemological criteria specified. In particular, the Cartesian emphasis on “certainty” and “method” played an important role in refuting the challenges of scepticism and providing warrants for “secure” knowledge. But in spite of its critical intent and the argument for incontrovertible fundamentals (*fundamenta inconcussa*), Descartes’ “solution” to the problem of how to warrant secure knowledge remained strangely linked to theology since it was still God who, as a benign being, ultimately guaranteed that concepts “matched” with the external world. It was only Kant’s transcendental turn which finally removed the Cartesian prop of a *deus ex machina* who “cannot be a deceiver, since it is manifest by the natural light that all fraud and deception depend on some defect”.<sup>6</sup>

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<sup>5</sup> Immanuel Kant, *Critique of Pure Reason*. The work is quoted according to the two editions of 1781 and 1787, as contained in the authoritative edition of the *Preußische Akademie*; English quotes are either from Höffe 1994 or our own translation.

<sup>6</sup> See Descartes (1984 [1641]): 35.

Kant not only dispensed with Descartes' dogmatic "solution" to radical doubt; he also answered Humean sceptics by addressing the puzzle of induction they were preoccupied with;<sup>7</sup> furthermore, writing at a time when Newtonian physics was triumphant, Kant realized that the claim of epistemology to serve as a foundation for knowledge could be maintained only if metaphysics, through a critical examination of the nature of reason and of the preconditions for knowledge, became a "science" itself. Kant attempted to answer the challenges of empiricists as well as dogmatists and sceptics, establishing "metaphysics"<sup>8</sup> as a "propaedeutic"<sup>9</sup> science and not as ontological speculation above and beyond experience (the latter can only lead, according to Kant, to intrinsically endless and inconclusive controversies).

By this move, Kant seemed to finally provide the "fundament" which Descartes had sought in vain, and he mapped out a proper role for "metaphysics", which was deprived of its ontological moorings. For Kant, reason itself must provide justification as it reflects upon its own "transcendental" presuppositions. The latter are independent of all experience, since they make any experience possible, and are thus prior to any perception or assertion about the world and its objects. By this turn away from the *object* to investigating the *preconditions* of our knowledge, as provided by reason itself, Kant hopes not only to set metaphysics onto "the secure path of science",<sup>10</sup> but he likens this turn to the Copernican revolution.<sup>11</sup>

As he points out, the importance of the Copernican revolution consisted not so much in the refutation of traditional astronomy but rather in the counterintuitive way in which truth is established by overcoming commonsense and unmasking the apparently uncontroversial "empirical" evidence of the sun going around the world as mere appearance. This entails an en-

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<sup>7</sup> On the puzzle of induction see Hume [1748] 2000: §4.1.20-27, §4.2.28-33, or consult [www.georgiacenter.uga.edu/idl/webid/portfolio/flash/interactive/puzzle\\_of\\_induction.swf](http://www.georgiacenter.uga.edu/idl/webid/portfolio/flash/interactive/puzzle_of_induction.swf).

<sup>8</sup> Kant 1968 [1781]: viii.

<sup>9</sup> Kant 1968 [1787]: 25.

<sup>10</sup> Kant 1968 [1787]: vii.

<sup>11</sup> Kant 1968 [1787]: xvi.

tirely new conception of “objectivity” and “science” since, as Kant suggests, modern scientists such as Bacon, Galileo and Toricelli were no longer learning from nature in the role of a “*pupil* who listens to everything a teacher chooses to say”, but in that of “an appointed *judge* who compels the witnesses to answer questions which he himself has formulated”.<sup>12</sup>

Because the procedure in the “court” of reason is “critical”, Kant can reject both empiricism and dogmatism, and at the same time provide an answer to the sceptics who either denied the possibility of true knowledge or, like Hume, downgraded knowledge by reducing it to subjective psychological habit.<sup>13</sup> The Kantian move of turning metaphysics into a critical inquiry into the epistemological foundations of knowledge was widely accepted, and rightly so. But let us examine more closely what this “epistemological” position actually entails.<sup>14</sup>

While Kant’s “logic of truth” remains within classical bounds, e.g. by assuming the correspondence between concepts and objects, his transcendental turn implies that there can be no direct “test” of concepts and propositions against reality because the subject is deeply implicated in the constitution of the object.

This recognition has three implications. First, if concepts do not do their work through some “mirroring” of the objects, they are discursive rather than deictic or intuitive. Second, knowledge is not simply limited to connecting phenomena causally. To show how things fit and are part and parcel of a larger order is as much an “explanation” as is showing a causal connection between two phenomena, a point that has surfaced in the recent debates about constitutive explanations.<sup>15</sup> Third, since the objects of experience are not simply “there” in the

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<sup>12</sup> Kant 1968 [1787]: xiii.

<sup>13</sup> We are indebted to Otfried Höffe (1994: 31-38) for this interpretation.

<sup>14</sup> This is all the more important since rather implausible claims are sometimes connected with Kant, such as the “unity of science” position which rests on Neo-Kantian foundations but seems, as a whole, rather precariously perched on various Kantian arguments.

<sup>15</sup> See e.g. Wendt 1999: esp. Ch. 2.

outer world, but are the results of our constructions and interests, neither concepts nor theoretical assertions can be built on mere inference, as Hume had suggested.

This dispenses on the one hand with “essences”, but also with the notion that issues of method can be settled by direct access to things themselves, and that a rigid distinction between ontology and epistemology can deliver the necessary warrants for our knowledge claims. The formal object of an investigation, such as for example a table, is something entirely different for the physicist, the chemist, the cabinet maker, the user, the art historian etc., and it serves little purpose to point out that there must be a common substratum – the Kantian “thing in itself” – a point which is uncontested, but largely irrelevant. As for the issue of concept formation, the cognitive revolution has borne out Kant’s arguments, even if in important respects it has modified his notion that the concept of “reason” must be a-historically and transculturally valid, since reason is always characterized by universality and necessity.<sup>16</sup>

To that extent, the advice of an influential “primer” in political science that the logic of “inference” is the *via regia* to knowledge seems like a giant step in the wrong direction.<sup>17</sup> It is bound to return us to the conundrum of empiricism and scepticism à la Hume – if the problems are taken seriously and not simply ignored, as is common nowadays – a dilemma from which Kant has liberated us by circumventing it.

While Kant’s first *Critique* certainly attempts to provide a foundational account of the nature of knowledge and its production, great care should be taken not to over-burden his construction. True, Kant himself invited such errors by suggesting that little was left for his successors since his critique, by its “logic of truth”, had “put an end to all errors” and had solved most of the important problems.<sup>18</sup> However, Kant was also very modest in that he understood

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<sup>16</sup> On the “cognitive revolution” see e.g. Lakoff 1987.

<sup>17</sup> See e.g. King et al. 1994; for a critique see Brady and Collier 2004.

<sup>18</sup> Kant 1968 [1781]: xii, xiii, xx, 87.

epistemology only as a critical reflection of reason upon its own presuppositions.<sup>19</sup> There is systematic reason to doubt that Kant could have espoused the position that epistemology was also, via the specification of “the” scientific method, the key for progress in the various sciences, as later claimed by the adherents of the Vienna circle. The latter influenced, through Hempel and Popper among others, the epistemological debates in the social sciences well into the second half of the 20th century.<sup>20</sup> Kant himself, by contrast, had clearly emphasized that his “propaedeutic” science was meant to clarify the presuppositions of knowledge through the critique of pure reason, and not to increase our knowledge about the empirical world.<sup>21</sup>

Precisely because his transcendental critique is not concerned with objects themselves, however constituted, it does not compete with the different sciences nor does it specify a field-independent proto-science that provides the paradigmatic understandings to the particular sciences, nor is it a critical reflection on the conceptual and theoretical developments within the sciences, as the “unity of science position” has claimed.

Seen from this perspective, nothing seems to be more alien to the Kantian project than the specification of “the” scientific method that could be “applied” to different fields of inquiry, and that would provide us with universally valid propositions as well as with cumulative knowledge of the world and its objects. On the other hand, we can also see that despite the existing gap between the knowledge of the objects of the world and the transcendental conditions of knowledge in general – a gap that no “one size fits all” epistemology seems to be able to bridge – the epistemological inquiry into the nature of knowledge is no idle undertaking, even

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<sup>19</sup> Kant 1968 [1781]: xiv.

<sup>20</sup> Diesing 1991: Ch. 1 and 2

<sup>21</sup> Kant 1968 [1787]: 25.

though Kant himself admits that the main function of his *Critique* is “only negative” since it does not expand, but only clarifies our reason.<sup>22</sup>

With these philosophical arguments in mind, we can revisit the second modern site where epistemology was supposed to provide the key to the solution of puzzles arising in the narrower field of “science”, both in actual research and in meta-theoretical disputes.

Already in Kant himself, there was an apparent ambiguity in that the *Critique of Pure Reason* saw the construction of the world as the result of causal connections characterized by necessity and universality, while the *Critique of Practical Reason* saw it as a product of decisions of a self-determined free will, able to set into motion new causal chains.<sup>23</sup> The emphasis on free will as the basis for explaining human action reinforced the previous criticism of humanists like Vico, who had protested that the Cartesian quest for universal necessity led to a fundamental misunderstanding of the social world, which was inevitably contingent. Insofar as the criterion for knowing the social world was not truth but the *verisimile*, Vico had called for a “new science” based on the “likely” instead of the “true”.<sup>24</sup>

Another fundamental question concerns the controversy between “erklären” and “verstehen”.<sup>25</sup> To many people it makes intuitive sense that understanding social actors is an operation that requires some form of empathy rather than reliance on “external” causal accounts. Nevertheless, the idea that we as scientific researchers have privileged access to the motives of actors and that, to understand an action, we simply have to place ourselves “in the shoes” of an actor, is hopelessly naïve. Here Max Weber’s contributions have provided an important impulse. He showed that taking an interpretive point of view has little to do with empathy. We

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<sup>22</sup> Kant 1968 [1787]: 25.

<sup>23</sup> Kant 1956 [1788].

<sup>24</sup> Vico 1999 [1744].

<sup>25</sup> For an accessible account of the differences see Hollis and Smith 1990.

can never know what someone else actually thought and felt, but in trying to understand some action we always impute certain motives to actors. Like the later Wittgenstein, Weber suggests that understanding has little to do with delving into the psyche or examining the private ruminations of an actor. While we certainly cannot feel someone else's pain, we nevertheless know most of the time what it is like, and our communications about it are not simply meaningless because the personal or private realm of others remains inaccessible.<sup>26</sup> Our explanations entail attributions to be tested and revised, but rather than relying on subjectivity they are based on *inter-subjectively* shared understandings and culturally transmitted schemes that, as is also the case with language, cannot be reduced to idiosyncratic personal dispositions or utterances.<sup>27</sup>

Furthermore, since social scientific "facts" are not natural but constituted by values, our interest in them is not simply limited to those incidents or social arrangements that are directly connected via causal chains to the present.<sup>28</sup> Otherwise our interest in ancient Egypt or China would be inexplicable. Weber, despite his emphasis on "causal relations", clearly had a much broader understanding of the task of social science than some of his interpreters suggest, linking him via the Humean fork and "causality" to a positivist conception of science. For one, Weber's interest in ideal types and typologies (rather than generalizations) indicates that virtually all of his explanations are of the constitutive rather than the efficient causal type. In addition, his argument that "value relations" (*Wertbeziehungen*) are constitutive of our social world and its "facts" is more in tune with the tenets of constructivism than with the positivists' or even scientific realists' notion of the world "out there".

Only Weber's insistence on the fact/value distinction and the "value-freedom" of science are somewhat perplexing in this regard. His treatment of politics as consisting of essen-

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<sup>26</sup> See e.g. Wittgenstein 1964; see also Pitkin 1972: Ch. 6.

<sup>27</sup> The term "subjective" is misleading in this context, since its semantics also denotes "the opposite of objectivity and truth", a meaning that Weber definitely did not want to evoke.

<sup>28</sup> See the remarks in Weber 1985 [1903-1906] and 1985 [1906]: esp. 256-265.

tially existential decisions (largely limited to leaders however) that are not susceptible to debates or persuasion link him, in a way, to the scepticism of Hume, for whom value questions were, like matters of taste, beyond discussion (*de gustibus non est disputandum*).<sup>29</sup>

At the beginning of the last century, the epistemological debate was also fuelled by several other sources. The empiricists were still struggling with the problem of induction, while others who believed in the strict determinism of nature and the universal and time-reversible validity of natural laws were puzzled by the fact that the final mathematical solution to the three body problem contradicted the classical implications of the Newtonian world view, as expressed by Laplace. Poincare's solution, which was never fully embraced even by Poincare himself, as it laid the foundation for chaos theory,<sup>30</sup> implied that small changes in some parameters resulted in large changes later on, so that the simple *actio est reactio* assumption could not even be applied to the development of natural systems of a certain complexity. This not only opened a gap between determinism and predictability, it also corrected the old notion that the universe would run along the same course again if restarted – a notion that was familiar from the mechanical imagery of clocks, springs and levers. Concomitantly, as emergent properties, equifinality and multiple realizability made their appearance, the belief that nature could be entirely explained in terms of simple efficient causality had ultimately to be jettisoned. Finally, quantum physics showed the inappropriateness of the notion of a fixed and already existing nature, the “discovery” of which was akin to lifting a veil, as Einstein still believed.<sup>31</sup>

Even worse news for foundationalist accounts came from the areas of mathematics and logic whose contradiction-free system of assertions had always been taken for granted. After all, in the Cartesian and Kantian attempts logic had served as the main criterion in judging the

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<sup>29</sup> Weber 1977 [1919].

<sup>30</sup> See the discussion in Toulmin 2001: Ch. 4.

<sup>31</sup> For a staggering account of the implications of the quantum revolution for our understanding of “reality” see Zeilinger 2003.

truth or falsehood of assertions. The possibility of “science” presupposed not only that “nature” answered our questions in experiments – even if with Kant we can no longer reach it directly but only in terms of our categorical framework and the synthetic *a priori* – but that nature answers in terms of the clear “yes or no” scheme of logic that does not allow for a third category in between “is” and “is not”. Even “maybe” (probability) is part of the “is” category, insofar as the ascription of a probability is assumed to be either true or false.<sup>32</sup>

Classical logic, of course hinted at such a problem in the famous liar’s paradox, for example when “Epimedes the Cretan says: all Cretans are liars”. Paradoxes were mostly considered as “sophistry” and logic, like mathematics, was held to be a contradiction-free and stringent system of assertions. Nevertheless, it was eventually recognised that paradoxes raised the issue of logical “necessity” and required a specification of the very foundations of logic. But can logic provide a specification for its own foundations without this specification itself becoming self-referential and thus involving us in the very same problems which gave rise to the paradoxes in the first place? The attempts by Tarski and Carnap to solve the problem via the construction of a meta-language distinct from the first-order object language seemed to point to a way out. But the solution presupposed not only exact criteria for separating the languages on different levels, but it also required exact boundaries for each concept in the object language.<sup>33</sup>

Unfortunately, several difficulties militated against reaching this goal. The first was that, as Wittgenstein’s language theory was showing, our concepts do not function as simple matches of the objects “out there”, making their “essence” transparent to us. The nature of objects is not simply recorded by a neutral observational language, but is largely constituted by it. Most clearly we see this in cases of institutions which are not natural but depend on conven-

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<sup>32</sup> Nor do probability theory and fuzzy logic offer an easy way out, since quantifying the “degree of truth” of an assertion is only another way of defining the “is”. Unlike such strategies for avoiding the problem, “undecidable” questions challenge the bivalence principle more head on.

<sup>33</sup> This was one reason why “basic sentences” recording simple observations became a *conditio sine qua non*.

tions and rules governing human practice. Later in the 20th Century, this was born out by the cognitive revolution in psychology.<sup>34</sup> The implications of both Putnam's argument that even ideal languages can be false<sup>35</sup> and Goedel's theorem that mathematics cannot be represented as a contradiction-free system suggested that the efforts of traditional epistemology to create its own foundations in logic and a clear notion of truth as reference were doomed to failure.

## 2. From Deconstruction to Reconstruction

After this "critique of the critical critique", it is our intention to move from deconstruction to reconstruction. Before proposing a pragmatic turn in epistemology and research methodology, however, we need to finally dispatch a common fallacy that seems to fuel much of the discipline's hypertrophic concern with epistemology. Simply stated, the fallacy suggests that everything will become "relative" if there are no secure, universally valid and trans-historically established criteria. If the foundationalist claims of traditional epistemology are faulty, then indeed "anything goes" and we can stop worrying about benchmarks for our knowledge claims. From this perspective, even the adherents of a more critical or pragmatic orientation towards knowledge production must be either nihilists or charlatans, because they deny "truth".<sup>36</sup>

This *reductio ad absurdum* is fuelled by the mistaken belief that truth is a property of the "world out there", and that theorizing should consist of attempts to grasp "reality" by conceptual matching operations. In practice, however, our concepts are always deeply enmeshed in our constructions of the "world". Insofar as concepts do not simply represent a fixed world "out there", assertions about this world cannot be justified in terms of a truth-conditional theory of meaning. Nevertheless, recognizing that this is not feasible is not the same as showing that

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<sup>34</sup> Lakoff 1987.

<sup>35</sup> See Putnam 1983: 1-25.

<sup>36</sup> Here rather mindless research activism or some crude form of pragmatism at bargain basement prices is typically supposed to take care of the problems.

there is no other way, as the charge of “nihilism” implies. Instead, we can trace our steps back and see at what points other paths offer themselves as an alternative. In this way we can avoid the old epistemological impasses and be free to explore new avenues that are suggested by a new perspective.

Another reason for the abovementioned fallacy results from an uncritically accepted assumption of traditional epistemology, namely that logically there are only two possibilities, as the traditional argument about the excluded middle (*tertium non datur*) implies. Either something is the case, or it is not. But alas, traditional binary logic is a poor philosopher’s stone. While experiments seldom result in clear “yes” or “no” answers, the class of “undecidable” questions – a category which supposedly cannot exist – is embarrassingly large. But if the bivalence principle of logic is neither able to adjudicate competing knowledge claims, nor to provide for a fruitful heuristic in guiding empirical research, then attempts to see in logic, or even in the categories of reason itself, the guarantor of true knowledge, must fail because the traditional epistemological project cannot deliver on its promise.

That such a bitter insight generates enormous anxieties and frequently results in charges of relativism or even nihilism against the bearers of this bad news is understandable. But such anxieties are misplaced. After all, we have to realize that the world did not come to an end when we became aware that it was not at the centre of the solar system or even the universe. Accepting the failure of the traditional epistemological project does not imply an end to all epistemology, nor does it amount to “nihilism” or the denial of truth. It simply suggests that the way we have pursued incontrovertible foundations for our knowledge for centuries is now at a dead end.

Even if traditional logic is not “rich” enough to provide incontrovertible foundations, we are not justified in believing that, if this claim has shown to be false, the contrary must be

true and that, therefore, “anything goes”. Precisely because of the failure of traditional logic, we are obliged to search for and critically examine alternative criteria that shall lend force to our assertions. Thus, quite contrary to the charges of some ayatollahs of mainstream political science that a critical stance towards the traditional conception of “science” ultimately leads to “nihilism”, the espoused position obliges us to search for viable criteria for the assessment of our theories, instead of relying on “imports” from other fields and disciplines in the vain hope that these issues have been resolved somewhere else, be it in physics, logic, mathematics, economics, or philosophy.

As a way out of the epistemological quagmire, let us now move on from deconstruction to reconstruction. We propose a pragmatic solution that takes its departure from acting (*practein*) and aims at the generation of useful knowledge. In the next section we show how an understanding of pragmatism as a consensus-oriented practice of discursive communities is at the antipodes of traditional epistemological projects which start from “pure reason”, “being”, or some other metaphysical assumptions. We will then proceed to suggest heuristic principles for a pragmatic research strategy, aiming at a kind of useful knowledge that should help us to find our way through the complexities we encounter in the social world.

### **3. Pragmatism as Practice**

Pragmatism derives from practice. In pragmatism, scientific research is savoured as the social practice of researchers. There are various reasons why this holds promise.

*First:* pragmatism liberates us from unnecessary headaches. The cure for the anxiety induced by radical doubt does not consist of the discovery of absolute certainty, which is a phantasmagorical undertaking.<sup>37</sup> Instead, the remedy lies in recognizing the unproductive nature of

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<sup>37</sup> See e.g. Peirce 1997.

universal doubt. Letting go of unrealisable plans and notions that lead down the road to delusional projects and acquiring instead the ability to “go on” in spite of uncertainties and the unknown, is probably the most valuable lesson to learn. Thus, even if it has turned out that mathematics as the most rigorous and secure system of thought is not free of contradictions, this realization does not prevent mathematicians from continuing to solve mathematical problems. *A fortiori*, most of us have to act most of the time without having the privilege of basing our decisions on secure and universally valid knowledge. In this spirit, pragmatism does not begin with “the thing in itself”, or with “reason” or “thought”, but with “acting”. As we all know from experience, praxis creates its own logic.

*Second:* pragmatism recovers the creative potential in science. It renounces the idea that social science is “just” the accumulation of knowledge or the approximation to truth. If the world “out there” were ready made only to be discovered, then scientific research would be a simple accumulation of true statements, leading us automatically closer and closer to “the truth” conceived as the totality of all true statements. What comes to mind here is Popper’s initial interpretation of scientific progress as a self-correcting process of conjectures and refutations.<sup>38</sup> But, as the history of science suggests, scientific progress is characterized by conceptual revolutions and not just by “normal” science, quite aside from the embarrassing problem of what to do with all those parts of Popper’s “world 3” or “third world” that have turned out to be pointless, such as the indivisibility of atoms, ether, phlogiston, or what have you.<sup>39</sup> Similarly misleading is the imagery of scientific progress as a closer and closer approximation to “the truth” without ever reaching it, thereby foxing the problem of revision. For example, the image of approximation in Popper’s verisimilitude argument draws its persuasiveness from the successive approximation of polygons in approaching the perimeter of a circle, when determining

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<sup>38</sup> Popper 1963, 1972 [1967].

<sup>39</sup> See Popper 1979.

the enclosed area.<sup>40</sup> But if we have learned anything from the studies of various disciplines then it is the fact that progress consists of being able to formulate new questions that *could not even be asked previously*. Thus, whatever we think of Kuhn's argument about "paradigms", we have to recognize that there are times of revolutionary change when the bounds of sense are being revolutionized, and when we do not simply learn more and more about an already encircled area.<sup>41</sup> By renouncing the objectivist idea that warranted knowledge is generated either through cumulative knowledge or the closest possible approximation to the world "out there", a pragmatic starting point takes the preliminary character of scientific knowledge seriously and frees our minds from the elusive quest for ultimate certainty.

*Third:* pragmatism appraises science as a process of knowledge production. Even without ultimate certainty, knowledge is construed within communities of practice.<sup>42</sup> The process is determined by rules in which scientists themselves constitute the definitions of problems, rather than lifting a veil from nature. Importantly, scientists often debate questions which are "undecidable" on the basis of existing data.<sup>43</sup> Rather than being able to rely on the bivalence principle of logic as an automatic truth finder, they therefore need to "weigh" the evidence.<sup>44</sup> Insofar as science is understood as a social practice, the critical element of the Kantian epistemological project is retained, only that the "court" which Kant believed to be reason, consists of the scientific practitioners themselves. Instead of applying invariable standards, each scientific discipline provides its own tribunal and judges the appropriateness of its methods and practices.<sup>45</sup> It

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<sup>40</sup> See for example Popper 1963: Ch. 10.

<sup>41</sup> For a more extensive discussion of this point see Kratochwil 2000.

<sup>42</sup> On communities of practice see Wenger 1998.

<sup>43</sup> Many scientific enquiries end in an aporetic situation; see the work of the physicist and philosopher of science Ziman (1991).

<sup>44</sup> See Kratochwil 2007 forthcoming.

<sup>45</sup> In staying within the Kantian metaphor of a "court", we also have to correct Kant who adhered to a rather implausible interpretation of law, i.e. that there are "unique" verdicts. As we know from jurisprudence and from case law, similar cases can be decided quite differently. Nevertheless, this does not justify the inference that law is

ought to be recognized that the tribunals constituted by scientific communities of practice are the best available surrogate for the Kantian court where Reason sits in judgment of itself.

*Fourth:* in pragmatism, truth claims do not become arbitrary. Within communities of practice, there are rules for the admissibility of behaviour. Despite the fact that truth is no longer a function of the bivalence principle, and no longer anchored either in things themselves or in reason, it is not abolished or supplanted by an “anything goes” attitude. Instead it becomes the product of a procedural notion of rule-following in accordance with the practices of a community, as nobody can simply make the rules as he goes along. These rules do not “determine” outcomes, as the classical logic of deductions or truth conditions suggested, but they do constrain and enable us in our activities and allow us to go on.

*Fifth:* pragmatism reinstates the historical contingency of scientific truth. Precisely because rule following does not simply result in producing multiple copies of a fixed template, assumed to be unchanging or universally valid, rules provide us with orientation in new situations. In this way we are able to accommodate both continuity and change when we engage in making validity claims. Validity now no longer has to assume a-historical universality, as the world is no longer understood as temporally irreversible, as in certain differential equations where time can be re-run but then runs again exactly the same way. “History” is now able to enter the picture and matters, because change can now be understood as “path dependent” development, even as evolution, or in the form of radical historicity, and not only as a nuisance impairing true knowledge.

*Sixth:* pragmatism is up to the state of the art in the sociology of knowledge. Although a pragmatic approach is sensitive to the social conditions of cognition, it is neither another version of the old sociology of knowledge, nor is it the utilitarian celebration of whatever “works”.

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inherently arbitrary. Determinacy need not coincide with uniqueness neither in logic (multiple equilibrium), nor in science (chaos theory), nor in law (Ronald Dworkin [1977] notwithstanding).

On the one hand, pragmatism differs from the old sociology of knowledge. It does not hinge on the *cui bono* question of knowledge, not to mention the farther reaching Marxist claim of false consciousness, since no argument about a link between social stratification and problem definition is implied here.<sup>46</sup> On the other hand, it is preposterous to understand pragmatism as some version of instrumentalism à la Friedman, accepting anything that provides “useful” predictions.<sup>47</sup> Although usefulness is indeed a practical idea, not every employment of it satisfies the more exacting criteria of a coherent pragmatic approach, which recognises the critical and inter-subjective nature of knowledge generation. Instead, pragmatism is compatible with approaches such as Bourdieu’s, or with more constructivist accounts of knowledge production such as Fuller’s social epistemology, because it highlights the interdependence of semantics and social structures.<sup>48</sup>

*Finally:* pragmatism is attuned to the “practice turn” in social scientific ontology.<sup>49</sup> This should not preclude more conventional research into power, interest, preferences, etc. After “culture” and “discourse”, we should beware of “practice” as another totalizing ontology that aspires to encompass everything social. If we take a more broad-church attitude, however, it turns out that from a pragmatic standpoint social practices are a particularly rewarding object of enquiry. Because pragmatism is sensitive for knowledge generation as a social practice, it is suitable to lead us from “simple” via “double” to “triple hermeneutics”: not only will a pragmatic researcher try to understand the texture of human practice via observation; he or she will also be reflexive about the intersubjective rationalizations of the practitioners themselves; furthermore, the pragmatic researcher will duly ruminate his own conceptual instruments.<sup>50</sup> He

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<sup>46</sup> For the non-Marxist variety of the old sociology of knowledge see Mannheim 1936.

<sup>47</sup> Friedman 1968.

<sup>48</sup> Bourdieu 1977; Fuller 1991.

<sup>49</sup> Schatzki et al. 2001.

<sup>50</sup> On double hermeneutics see Giddens 1984.

will not become enmeshed into interpretive or ethnographic research to such an extent as to surrender his own critical judgements to the practitioners in the “field”.<sup>51</sup>

For all of these reasons, taking a pragmatic attitude towards our research allows us to get along with our projects without the misplaced hope that a foundationalist notion of epistemology implies, and on which it cannot deliver, while remaining sensitive to the theoretical problems that arise in the course of our inquiries. Instead of relying on false promises, we will learn to follow a course of action that represents a good bet.

#### **4. Pragmatic Research Strategy**

If we accept that knowledge generation in social practice has important advantages over orthodox social scientific methodologies (as suggested in the introduction to this article); if we admit that the attempts of traditional epistemology to provide incontrovertible foundations for the generation of field-independent knowledge are doomed to failure; and if we further accept that there are no alternative epistemological projects that shall miraculously do the trick; then it seems reasonable to develop practical suggestions for a research methodology that would mimic the way we generate knowledge in social practice.

This is not to idealise the “quick and dirty” mode of knowledge production in social practice, which is even more plagued by emotional biases and conceptual blinkers than social scientific methodologies. There are good reasons why social scientists are not usually satisfied with intuitive rules of thumb for grasping certain classes of phenomena, and prefer explicit and intersubjectively valid propositions instead. What is needed, therefore, is a reasonable compromise between standards of scientific methodology and the way we produce knowledge in social practice. To provide such a compromise, let us suggest a methodologically informed

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<sup>51</sup> For an advanced introduction to interpretive research see Yanow and Schwartz-Shea 2006.

version of what the American pragmatist Charles Sanders Peirce used to call “abduction” or “retroduction”.<sup>52</sup>

The typical situation for abduction is when you become aware of a certain class of phenomena that intrigues you for some reason, but for which you lack applicable theories. You simply trust, although you obviously do not know for certain, that the observed class of phenomena is not random. Therefore you start collecting pertinent observations and, at the same time, applying concepts from existing fields of your knowledge. Instead of trying to impose an abstract theoretical template (deduction), or simply gathering and processing all relevant facts (induction), you start reasoning at an intermediate level (abduction).

Your choice of observations to be made and concepts to be applied will be strongly determined by the reason why you were intrigued by the class of phenomena in the first place. Do you want to control complexity? Do you want to solve problems? Or do you simply seek understanding? These are all legitimate objectives, but you should be explicit and conscious about which ones you are actually pursuing. If the concepts selected do not help you to see the kind of orderly patterns you are looking for, you may either reject or refine them. Alternatively, you may redefine the boundaries of the class of phenomena under examination. Eventually, a procedure of mutual adjustment and “educated guesswork” will lead to a framework of analysis (or set of propositions, or even theory) to grasp the class of phenomena as it evolves in the process of your research.

It is left to the interpreters of Charles Sanders Peirce to decide whether this is what the dean of American pragmatism really had in mind.<sup>53</sup> If we agree, however, that this is what we do in our own social practice when confronted with complex challenges; and if we agree, further, that it works better than what we usually do in social science; then it will be worthwhile

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<sup>52</sup> Peirce 1998: vol. 5, § 590-604; vol. 7, § 218-22.

<sup>53</sup> See also Josephson 2000; Magnani 2001; Reichertz 2003.

exploring whether and to what extent abduction can improve the way we generate social scientific knowledge.<sup>54</sup> One would expect the result to be quite different, on the one hand, from purely idiographic research and, on the other hand, from the search for scientific laws through deductive theory testing or inferential statistics.

Fortunately, there is no need to start from scratch. Abduction can build upon existing methods of comparative case study research.<sup>55</sup> Unfortunately, though, comparative case study research is not always practised in a very practical way. Its typical objective is causal inference rather than the efficient generation of useful knowledge. Even *Qualitative Comparative Analysis* (QCA) and *Fuzzy-Set Social Science*, which are bold enough to abandon the quantitative template and drop the ideal of correlation analysis, nevertheless depend upon heavy epistemological assumptions about necessary and sufficient causation.<sup>56</sup>

As will become clear from the following discussion of seven pragmatic principles, abduction provides an alternative to conventional methods of comparative case study research.

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| <ol style="list-style-type: none"><li>1. The purpose of research, including personal motivation, must be stated in public.</li><li>2. Orientation in a relevant field is more important than causal theorising.</li><li>3. Pragmatic research is constituted more by concepts than by theory.</li><li>4. Conceptual distinctions should elicit patterns of similarity and difference.</li><li>5. Case sampling may follow a “most important” or a “most typical” case scenario.</li><li>6. Complexity can be reduced by appropriate formal tools.</li><li>7. Abduction is eventually compatible with causal theorising.</li></ol> |
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**Table: Seven pragmatic principles**

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<sup>54</sup> See also Kolb 1984 on experiential learning.

<sup>55</sup> For the vast literature on case study research see only George and Bennett 2005; Gerring 2007.

<sup>56</sup> Ragin 1987, 2000; see also Mahoney and Goertz 2006.

Let us start with the purpose of research. A pragmatic researcher should be affirmative about the fact that the main purpose of research is the generation of useful knowledge with a particular research interest in mind. Whatever that interest is, it should be stated in public. It is simply not true that personal motivation “should not appear in our scholarly writings”.<sup>57</sup> On the contrary, the interest of the researcher should always be stated as clearly as possible. It will then be up to the relevant evaluators and the peer community at large to establish whether and to what extent a specific research project serves a legitimate, useful, and relevant purpose. Truth in social science is not simply a property of the world. Truth claims are meaningful only in the context of our motivations and the questions we ask.

Causal inference is neither the only legitimate nor the most important purpose of pragmatic research. Usually, the goal of abduction is to enable orientation in a complex field of research. This consists of mapping a class of phenomena in order to increase cognitive understanding and/or practical manipulability. To reach this objective, it is mostly sufficient to detect patterns of similarity and difference that allow for the identification of a certain degree of order within an otherwise confusing field. To the extent that abduction helps make intelligible or malleable a field that previously escaped our cognitive or operational parameters, it has served its most important purpose. In some cases, it is possible to formulate a sort of “grounded theory”.<sup>58</sup> However, this is not always necessary. Given the contingent nature of the social world, contingent generalisations, rather than the quest for causal laws, are appropriate for the social sciences.<sup>59</sup> While existing theories can help by informing the process of abduction, a pragmatic researcher will not agree that causal inference is a necessary condition for success.

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<sup>57</sup> King et al. 1994: 15.

<sup>58</sup> Glaser and Strauss 1967; Strauss and Corbin 1998.

<sup>59</sup> Schedler 2007, forthcoming.

Abduction is concept-driven rather than theory-driven. Concepts, rather than full-blown theories, allow the pragmatic researcher to constitute a meaningful field of research. The pragmatic researcher will reject a “causal, ontological, and realist view of concepts”,<sup>60</sup> and prefer a view that recognises their constitutive, inter-subjective, and semantic nature.<sup>61</sup> Not only do concepts constitute our field of observation, but what we see in that field will in turn elucidate or modify our initial understanding of the concepts. Especially during the initial stages of the research process, it would be counterproductive to ban the adjustment of concepts. Instead, the pragmatic researcher will start by engaging in a careful reworking of concepts. The very process of research should then lead to increasing operational and denotative clarity. Rather than accepting the positivist view that the definition of concepts should be stipulated at the beginning of the research process and then be held constant, it is better to allow for the mutual adaptation of conceptual framework and empirical findings. Self-imposed conceptual blinkers are not useful, nor is it helpful to cast concepts into the procrustean bed of a lexical definition. Human cognition happens in a hermeneutic circle, and we should welcome the kind of circularity in which our understanding of the whole is modified by our progressive understanding of its parts.

A field of research is constituted by a limited number of core concepts, maybe two or three. It is then divided, by further conceptual distinctions, into a variety of subfields or “domains”. Whereas positivist research designs examine the causal impact of variables, abduction is concerned with the heuristic value of core concepts and conceptual distinctions. Core concepts and the field, as well as conceptual distinctions and domains, are two sides of the same coin. Usually, conceptual distinctions take the shape of overlapping categorisations. When useful, they elicit patterns of similarity and difference that increase our knowledge. If not, it will

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<sup>60</sup> Goertz 2005: 5; cf. Sartori 1970, 1984.

<sup>61</sup> Davis 2005.

be better to try other distinctions. Since the objective of abduction is detecting patterns of similarity and difference, it should remain possible to readjust conceptual distinctions in the course of research, especially in the early stages of the process. Instead of causal inference, it will then be possible to examine whether and how different distinctions are important in structuring the field under examination. Since the objective is to map a class of phenomena, finding the most useful distinctions is an important achievement in itself.

The next issue is sampling strategies. Usually, pragmatic case sampling will follow a “most important” or a “most typical” case design. As we have seen, a field of research and its domains are constituted by a small number of core concepts and a larger number of conceptual distinctions. A pragmatic researcher will tend to select either the most important or the most typical cases in each domain. Either of these sampling strategies is reasonable, but to avoid unnecessary asymmetries it is convenient to choose one of them.<sup>62</sup> An important reason for choosing a “most important” or a “most typical” case design is that, in practice, the conceptual boundaries of a field or domain are always contested. Social-scientific concepts are hardly ever mirrored by a homogenous population of real-world manifestations with clearly defined boundaries. There are always borderline cases that are hard to subsume under the concept at hand. At best, a reasonable degree of consensus can be expected for the empirical prototypes or theoretical ideal types at the core of the case population.<sup>63</sup> Especially at the beginning of a research programme, it is therefore practical to study those cases that are close to the core of a field or domain, regardless of its boundaries.<sup>64</sup>

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<sup>62</sup> In cases of doubt, it will be useful to turn to practitioners or scan the relevant literature in order to find the most important or the most typical cases.

<sup>63</sup> Davis 2005: 61-91.

<sup>64</sup> When the research programme is more advanced, one can move towards the frontiers to sound out how far the concepts applied can be stretched without losing their analytical value.

Then there is the problem of controlling complexity and, closely related, cognitive and emotional biases. Social science can be understood, at least in part, as being geared towards the containment of complexity and biases. On the one hand, abduction offers a promising research strategy precisely because it helps to detect patterns of similarity and difference in a complex field of research. On the other hand, due to the practice of drawing distinctions there is also an inherent drift in abduction towards complexity. While some distinctions divide the field into domains, thereby determining case selection and preparing the ground for cross-case analysis, there will be other distinctions to structure the examination of cases and thereby to specify the parameters for within-case analysis. Abduction typically involves a large number of cross-cutting distinctions that produce a large number of case studies for intra-case and inter-case comparison. This may easily lead to a degree of complexity beyond our cognitive capacities. There are limits beyond which it becomes difficult to keep track of the ramifications of our own research design, and it is precisely when we reach these limits that we are tempted to indulge in cognitive or emotional biases in order to maintain the illusion that we are still the master of the Golem that has been created by virtue of our own conceptual distinctions.

When a purely hermeneutic approach to data analysis is beyond our cognitive capacities, formal tools can help to make sure that patterns of similarity and difference remain detectable despite the complexity induced by cross-cutting conceptual distinctions. For example, complexity can be controlled by virtue of the following four instruments: structured-focused comparison, formal coding, synthetic indices, and descriptive statistics. While abduction is fundamentally based on a qualitative understanding of the cases, it is possible to set up a unified set of aspects that shall be covered in every narrative. This is typically done by the method of structured focused comparison.<sup>65</sup> Formal coding will then involve the creation of a matrix

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<sup>65</sup> See George and Bennett 2005: 67-72 for further references.

containing the most pertinent information from each case study. Synthetic indices can be used to aggregate this information, while descriptive statistics can help to detect patterns of similarity and difference in the dataset. Once detected, it is fundamentally important always to (re)interpret the patterns in the light of qualitative evidence.

The latter point in particular warrants a few remarks. When using statistics, a pragmatic researcher will preferably use intuitive tools such as frequency counts or cross-tabulation, which make it easy to check statistical findings against the qualitative record. While there is no need for a taboo against using inferential statistics as long as it is done for heuristic purposes, one has to be extremely careful with the alchemy of statistical methods that smuggle unwarranted assumptions such as the homogeneity or independence of cases into the dataset, and thereby “miraculously” lead to sweeping generalisations across and beyond the sample. Formal research tools can be helpful, but statistical sophistication is not a goal of pragmatic research. As we have seen on several accounts, the goal of abduction is far more straightforward: the detection of patterns of similarity and difference within a given field. The pragmatic researcher will therefore keep analytical procedures as simple and intuitive as possible, and prefer descriptive to inferential statistics.

While causal theory is not the main purpose of abduction, an intelligent pragmatic research design can allow for the formulation of a causal theoretical model. Abduction is not geared towards the detection of covering laws.<sup>66</sup> Nevertheless, pragmatic research is amenable to the search for causal theory in a broader sense. This can be accomplished by means of the same tools that are used for abduction as a descriptive instrument. Imagine a dataset containing

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<sup>66</sup> A pragmatic approach is more radical than simply moving from “variable-oriented research” to “case-oriented research”, where the final objective is still to detect necessary or sufficient causation (Ragin 2004). Instead, it moves further towards “problem-oriented” and “concept-oriented” research. Recent attempts at typological theory, as valuable as they are in expanding the boundaries of what is accepted as legitimate by the mainstream, fall short of this requirement since they still reduce causality to the search for law-like regularities in terms of dependent and independent variables (George and Bennett 2005: 233-262; Elman 2005).

observed causal pathways. If the number of pathways in the dataset is sufficiently large, nothing prohibits observing, coding, and counting their frequency. Abduction can be used not only for mapping descriptive patterns of similarity and difference, but also patterns of similarity and difference in the explanation of the observations made. In short, abduction is as suitable for mapping patterns of causality as for descriptive purposes.

In a nutshell, abduction can be seen as a comparative case study method. It starts with a research interest that relates to some relevant purpose. The specific field of research is constituted by a limited number of core concepts. A variety of distinctions are applied to divide the field into a number of domains. The most important or most typical cases in each domain are examined to establish whether and how each underlying distinction is important in structuring the field under examination. To that end, cross-case analysis is combined with within-case analysis. Despite a healthy dose of scepticism, formal methods can be helpful to control complexity, avoid biases, and analyse the data. The ultimate goal, however, is not methodological sophistication but orientation in a complex field. In addition to mapping a field descriptively, the development of a causal theory is also an option.

## **5. Conclusion**

As we have seen, accepting the consequences of the failure of false epistemological ambitions does not mean an end to all epistemology, nor is it the same as nihilism, relativism, or “anything goes”. If there are no incontrovertible foundations of scientific knowledge, a pragmatic strategy of knowledge generation is the obvious alternative. In this spirit, we have fleshed out problems of research design as they arise when one opts for the pragmatic approach of abduction. This is not to claim that abduction is the only possible pragmatic approach, nor is it to deny that other forms of scientific inquiry have their legitimate place. We do argue, how-

ever, that abduction represents a good bet in pursuing our research after the failure of traditional epistemology to provide the incontrovertible foundations of scientific knowledge.

In fairness, it should be recognised that abduction is a risky endeavour since there is no strict algorithm to guarantee successful research. Of course the same also applies to more positivist methodologies such as theory-testing, although novices are often lured into the belief that following a unified logic of inquiry is a guarantee of scientific success. But be that as it may, abduction is risky. Since it is an open-ended process there is no guarantee that abduction will always lead to relevant insights, and if one does not take the necessary precautions there is a risk of endlessly proliferating complexity. As we have seen in the course of this article, however, there are practical strategies that can dramatically increase the likelihood that a research effort will lead to useful results that will be accepted by the relevant evaluators of the research project and by a wider academic audience.<sup>67</sup>

Pragmatism as the practice of a discursive community and pragmatism as a device for the generation of useful knowledge are two sides of the same coin.<sup>68</sup> On the one hand, discursive communities can go completely astray when they are so alienated from their social environment that they disregard plain commonsense. Take as an example the medieval debate on whether the duck is a bird or a fish, or recent discussions among international relations scholars on what matters more: structure or agency. On the other hand, useful knowledge is pointless if it cannot be communicated in a meaningful way.

Pragmatism should therefore neither be reduced to the existing (or fabricated) consensus of a concrete group of scientists, nor to the utility of results regardless of their presuppositions and meaning. An academic research project should carry the day if, and only if, it fulfils

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<sup>67</sup> For an application of abduction in empirical research see Friedrichs 2007.

<sup>68</sup> Jürgen Habermas (2003: 36-42) has reached similar conclusions, although he emphasises the empirical fallibility rather than the practical usefulness of knowledge.

the following two criteria: first, it should “work” as a reasonable response to the problem at hand; and second, it should be convincing to the relevant evaluators. Academic communities should not become so self-referential to behave as the sole judge of their own discourse, without considering what other courts have ruled or what the jury says.

It is therefore important to maintain the communicability of our research questions and findings not only to fellow specialists but also to a wider academic audience and, ultimately, society at large. For example, it should matter to scholars of comparative politics and international relations if philosophers of science have reached the conclusion that there is no epistemological basis for the accumulation of law-like statements. It should similarly matter to them if political decision makers and people interested in politics are not interested in their theoretical ruminations because they either do not understand them or do not see their utility. Ideally, both the findings of empirical research and the gist of theoretical debates should be reported in a vocabulary close enough to everyday language to allow for cross-community dialogue.

The appropriate response to the epistemological impasse is neither indulgence in endless meta-theoretical debating nor a candid denial of the problem, but the pursuit of a pragmatic approach to empirical research. Or, more bluntly: let us recognise that neither lofty theory bashing nor blind research activism can provide secure foundations for our knowledge, and let us instead try to gain knowledge that will help us to deal with relevant questions and, ultimately, to find our way through the complexities of the social world.

## References

- Bourdieu, Pierre (1977) *Outline of a Theory of Practice*, Cambridge: Cambridge University Press.
- Brady, Henry E. and David Collier (2004) *Rethinking Social Inquiry: Diverse Tools, Shared Standards*, Lanham: Rowman and Littlefield.
- Davis, James W. (2005) *Terms of Inquiry: On the Theory and Practice of Political Science*, Baltimore, Md.: Johns Hopkins University Press.
- Descartes, René (1984 [1641]) 'Meditations on First Philosophy', in *The Philosophical Writings of Descartes*, translated by John Cottingham, Robert Stoothoff, and Dugald Murdoch (3 Voll.), Cambridge: Cambridge University Press, Vol. 2, pp. 1-62.
- Diesing, Paul (1991) *How does Social Science Work? Reflections on Practice*, Pittsburgh: University of Pittsburgh Press.
- Dworkin, Ronald (1977) *Taking Rights Seriously*, Cambridge, Mass.: Harvard University Press.
- Elman, Colin (2005) 'Explanatory Typologies in Qualitative Studies of International Politics', *International Organization* 59 (2): 293-326.
- Friedman, Milton (1968) 'The Methodology of Positive Economics', in May Brodbeck (ed.) *Readings in the Philosophy of Social Sciences*, New York: Macmillan, pp. 508-529.
- Friedrichs, Jörg (2007) *Fighting Terrorism and Drugs: Europe and International Police Cooperation*, London and New York: Routledge, forthcoming.
- Fuller, Steven (1991) *Social Epistemology*, Bloomington: Indiana University Press.
- George, Alexander L. and Andrew Bennett (2005) *Case Studies and Theory Development in the Social Sciences*, Harvard, Mass.: MIT Press.

- Gerring, John (2007) *Case Study Research: Principles and Practices*, Cambridge: Cambridge University Press.
- Giddens, Anthony (1984) *The Constitution of Society: Outline of the Theory of Structuration*, Cambridge: Polity Press.
- Glaser, Barney G., and Anselm L. Strauss (1967) *The Discovery of Grounded Theory: Strategies for Grounded Research*, New York: Aldine.
- Goertz, Gary (2005) *Social Science Concepts: A User's Guide*, Princeton: Princeton University Press.
- Habermas, Jürgen (2003) *Truth and Justification*, Cambridge: Polity.
- Hammersley, Martyn (1989) *The Dilemma of Qualitative Method: Herbert Blumer and the Chicago Tradition*, London and New York: Routledge.
- Höffe, Otfried (1994) *Immanuel Kant*, Albany, N.Y.: State University of New York.
- Hollis, Martin and Steve Smith (1990) *Explaining and Understanding in International Relations*, Oxford: Oxford University Press.
- Hume, David ([1748] 2000) *An Enquiry Concerning Human Understanding*, ed. Tom L. Beauchamp, Oxford: Clarendon Press.
- Josephson, John R. (2000) 'Smart Inductive Generalizations are Abductions', in Peter A Flach and Antonis Kakas (eds) *Abduction and Induction: Essays on their Relation and Integration*, Dordrecht: Kluwer, pp. 31-44.
- Kant, Immanuel (1968 [1781]) *Kritik der Reinen Vernunft*, Berlin: Walter de Gruyter (1st edn) [Vol. 4 of *Kants Werke: Akademie-Textausgabe* (9 Voll.), pp. 1-252].
- Kant, Immanuel (1968 [1787]) *Kritik der Reinen Vernunft*, Berlin: Walter de Gruyter (2nd edn) [Vol. 3 of *Kants Werke: Akademie-Textausgabe* (9 Voll.)].

- Kant, Immanuel (1956 [1788]) *Kritik der Praktischen Vernunft*, Wiesbaden: Insel Verlag [Vol 7 of *Immanuel Kant: Werkausgabe* (12 Voll.), pp. 103-302].
- King, Gary, Robert O. Keohane, and Sidney Verba (1994) *Designing Social Inquiry: Scientific Inference in Qualitative Research*, Princeton: Princeton University Press.
- Kolb, David A. (1984) *Experiential Learning: Experience as the Source of Learning and Development*, Englewood-Cliffs, N.J.: Prentice Hall.
- Kratochwil, Friedrich (2000) 'Constructing a New Orthodoxy? Wendt's "Social Theory of International Politics" and the Constructivist Challenge', *Millennium* 29 (1): 73-101.
- Kratochwil, Friedrich (2007) 'Evidence, Inference, and Truth as Problems of Theory Building in the Social Sciences', in Mark I. Lichbach and Richard N. Lebow (eds) *Theory and Evidence in Comparative Politics and International Relations*, New York: Palgrave-Macmillan, forthcoming.
- Lakoff, George (1987) *Women, Fire and Dangerous Things: What Categories Reveal about the Mind*, Chicago: University of Chicago Press.
- Magnani, Lorenzo (2001) *Abduction, Reason, and Science: Processes of Discovery and Explanation*, New York: Kluwer Academic.
- Mahoney, James and Gary Goertz (2006) 'A Tale of Two Cultures: Contrasting Quantitative and Qualitative Research', *Political Analysis* 14 (3): 227-249.
- Mannheim, Karl (1936 [1929]) *Ideology and Utopia*, London: Routledge and Kegan Paul.
- Peirce, Charles Sanders (1998) *Collected Papers of Charles Sanders Peirce*, Bristol: Thoemmes (=reprint of the 1931-58 edition).
- Peirce, Charles Sanders (1997 [1868]) 'Some Consequences of Four Incapacities', reprinted in Louis Menand (ed.) *Pragmatism: A Reader*, New York: Vintage, pp. 4-7.

- Pitkin, Hanna F. (1972) *Wittgenstein on Justice: On the Significance of Ludwig Wittgenstein for Social and Political Thought*, Berkeley: University of California Press.
- Popper, Karl R. (1963) *Conjectures and Refutations: The Growth of Scientific Knowledge*, New York: Harper.
- Popper, Karl R. (1972 [1967]) 'Epistemology without a Knowing Subject', in Id. *Objective Knowledge: An Evolutionary Approach*, Oxford: Clarendon Press, pp. 106-152.
- Popper, Karl R. (1979) 'Three Worlds', *Michigan Quarterly Review* 28 (1): 1-23.
- Putnam, Hilary (1983) 'Models and Reality', in Id., *Philosophical Papers*, vol. 3, Cambridge: Cambridge University Press, pp. 1-25.
- Ragin, Charles C. (1987) *The Comparative Method: Moving beyond Qualitative and Quantitative Strategies*, Berkeley: University of California Press.
- Ragin, Charles C. (2000) *Fuzzy-Set Social Science*, Chicago: University of Chicago Press.
- Ragin, Charles C. (2004) 'Turning the Tables: How Case-oriented Research Challenges Variable-oriented Research', in Henry E. Brady and David Collier (eds) *Rethinking Social Inquiry: Diverse Tools, Shared Standards*, Lanham: Rowman and Littlefield, pp. 123-138.
- Reichertz, Jo (2003) *Die Abduktion in der Qualitativen Sozialforschung*, Opladen: Leske und Budrich.
- Sartori, Giovanni (1970) 'Concept Misformation in Comparative Politics', *American Political Science Review* 64 (4): 1033-1053.
- Sartori, Giovanni (ed.) (1984) *Social Science Concepts: A Systematic Analysis*, Beverly Hills: SAGE.
- Schatzki, Theodore R., Karin Knorr Cetina, and Eike von Savigny (eds) (2001) *The Practice Turn in Contemporary Theory*, London and New York: Routledge.

- Schedler, Andreas (2007) 'Mapping Contingency', in Ian Shapiro and Sonu Bedi (eds) *Political Contingency: Studying the Unexpected, the Accidental, and the Unforeseen*, New York: New York University Press, forthcoming.
- Strauss, Anselm, and Juliet Corbin (1998) *Basics of Qualitative Research: Techniques and Procedures for Developing Grounded Theory*, Thousand Oaks: SAGE, 2nd edn.
- Toulmin, Stephen E. (2001) *Return to Reason*, Cambridge, Mass.: Harvard University Press.
- Vico, Giambattista (1999 [1744]) *The New Science*, translated by David Marsh, with an introduction by Anthony Grafton, London and New York: Penguin.
- Weber, Max (1977 [1919]) *Politik als Beruf*, Berlin: Duncker und Humblot.
- Weber, Max (1985 [1903-1906]) 'Roscher und Knies und die logischen Probleme der historischen Nationalökonomie', in Id., *Gesammelte Aufsätze zur Wissenschaftslehre*, Tübingen: J.C.B. Mohr, pp. 1-145.
- Weber, Max (1985 [1906]) 'Kritische Studien auf dem Gebiet der kulturwissenschaftlichen Logik', in Id., *Gesammelte Aufsätze zur Wissenschaftslehre*, Tübingen: J.C.B. Mohr, pp. 215-290.
- Wendt, Alexander (1999) *Social Theory of International Politics*, Cambridge: Cambridge University Press.
- Wenger, Etienne (1998) *Communities of Practice: Learning, Meaning, and Identity*, Cambridge: Cambridge University Press.
- Wittgenstein, Ludwig (1969) *Preliminary Studies for the 'Philosophical Investigations'*, generally known as 'The Blue and Brown Books', Oxford: Basil Blackwell, 2nd edn.
- Yanow, Dvora and Peregrine Schwartz-Shea (eds) (2006) *Interpretation and Method: Empirical Research Methods and the Interpretive Turn*, Armonk, N.Y.: M.E. Sharpe.
- Zeilinger, Anton (2003) *Einsteins Schleier: Die neue Welt der Quantenphysik*, München: Beck.

Ziman, John (1991) *Reliable Knowledge: An Exploration of the Grounds for Belief in Science*,  
Cambridge: Cambridge University Press.