

Intrinsic Value in Pragmatism: Trojan Horse or Savior?

Pragmatism has been sneaking around the science of international relations (IR) for some time now. Its (still modest, yet trendy) traces are to be found around the tables in the IR conference rooms, university curricula, and, with a growing tendency, also in the realm of the IR journals. A passing look at the pragmatic contribution reveals many of its potential faces: a vehicle to bridge or to bypass ‘stalled epistemological and/or ontological debates’; a carrier of more reflexive, tolerant and plural approach to the IR theories; a bearer of fallible, historically conditioned, yet relevant knowledge; a provider of practical science, all of this inevitably wrapped in the velvet ‘community of inquirers’ which serves as anchorage of the otherwise extreme and – from the pragmatist point of view – unnecessary relativist reading of science (and that of IR). In this text, I will try to argue that instead of trying to delimit pragmatism by referring to both naïvely realist (or ‘a priorist, or transcendentalist) accounts of science as well as to their radically relativist mirror image it is more important to face its most competent challenger, recent scientific or critical realism. By doing this I hope to clarify some questions that are inherent to pragmatism itself – namely to clarify his general position on epistemology and to explore his notion of the ‘social’ which in many respects plays a key role in the entire pragmatic outlook. In general, these questions touch very closely on the issue whether pragmatism is to be seen as instrumental philosophy or if it is necessary to contemplate some of the intrinsically scientific values that are on many occasions so thoroughly criticized as unnecessary inflationism.

Somewhat in line with the Bourdiean point of view that detects the most fruitful debates in dialectics between those most competent and relevant challengers, I would like to use a clash between scientific realism and pragmatism as a background for this text, serving to show what decisions the IR pragmatism has to make. Thus, I argue it is more valuable to sidetrack a debate of any ‘a priorist’ transcendental accounts of science (such as logic-based positivism) as well as I can save this text from discussion of radically relativist anti-scientific postures. To expose pragmatism against scientific realism is all the more important as certain features of both approaches could be easily used interchangeably. Take as an example the following quote, taken from the editorial note of the special issue of Millennium journal devoted to pragmatism (Editorial, 2002: iii): “Pragmatism invokes a methodological pluralism

and disciplinary tolerance grounded in the resolute acknowledgement of fallibilism and contingency, as well as the celebration of dialogism (...) Pragmatism encourages a multi-perspectival style of inquiry that (...) benefits from the complementarity, rather than opposition, of different understandings of world politics.” It is to be wondered, how many contemporary IR scientific realists would not put their signature under this statement (see, especially, Wight, 2006a). As I will try to point out, however, there is an extremely deep difference between these two. This difference is necessary to explore in order to carry on with the pragmatic IR voyage because it can show some of the dilemmas pragmatism itself faces.

Because, as already mentioned, to write on pragmatism within IR does not mean to hit an unbeaten path, it seems without merit to try to assert my own vision of it, taken from without. This exercise would only end up in adding one more picture into the yet unstructured pile of From the same reason it does not seem to make much sense to overview the scarce literature on this topic either, since only to highlight the points of various authors would overwhelm this presentation. Instead, I chose to engage in a critical debate with one of the most prominent IR writers who lately turned to pragmatism, with Fridrich Kratochwil¹.

The most explicit articulation of Kratochwil’s pragmatism is to be found in his inaugural lecture of the Tartu CEEISA conference which later appeared in the JIRD as *Of false promises and good bets: a plea for a pragmatic approach to theory building* (Kratochwil, 2007a). From the outset, the author proceeds with a double objective on his mind: to refuse any claim for a grand and secure epistemological project and to navigate between this chimera on the one hand and the nihilist trap on the other. Thus, first, he shows that there is no secure place that provides an unproblematic and trans-historically valid Archimedean point, and, therefore, the traditional epistemological project cannot “make good on its promise”. With this anti-foundationalist background, he refutes what he sees as ‘twin fallacies’: 1) that in the absence of universally valid criteria everything becomes ‘relative’ and that, therefore, the adherents of a more critical or pragmatic orientation towards knowledge has to be either nihilists or charlatans since they deny ‘truth’. And, second, since the foundationalist claims of traditional epistemology can be shown to be faulty, indeed ‘anything goes’ and we need not worry about criteria that warrant our knowledge claims. (Kratochwil, 2007: 2 – 3 position that P. Bourdieu (2004: 3) summed up this position as follows: “it is the dogmatic idea or ideal of absolute knowledge that leads to skepticism” and that “relativistic arguments have their full

¹ Regrettably, before the completion of this paper, I was not in a possession of the text Evidence, Inference, and Truth as Problems of Theory Building in the Social Sciences’ where he raises some of the issues involved here and due to this fact I might make some mistakes in understanding his approach.

force only against a dogmatic (...) epistemology". Thus, by lowering down the claim for absolute knowledge, Kratochwil hopes to steal the winds from the skeptics' sails.

Kratochwil does not want to lose the sight of at least some form of criteria, but these are certainly not to be regarded as universally valid, neither they are able to provide the scientist with a direct access to reality. In this, Kratochwil firmly sticks to the first, epistemological (that is, not transcendental) part of 'Kantian turn': Kant legitimized the study of knowledge as independent from the study of ultimate reality; it was no longer necessary to answer all the skeptic's questions before proceeding with the scientific journey. Following this, two of the unavoidable themes of pragmatism and instrumentalism (in the Deweyan sense) break out. First, the cognizing process is no longer to be understood in terms of 'pupil' who listens to the nature (in Kantian terms, to the 'being itself'); it is not a wax-tablet, passively reflecting anything that happens to pass by; instead, it is an active initiative instrument of knowing, an "antenna that feels the way for the organism" (Bush, 1923: 701). Second, for the 'antenna' to work, it is not necessary to wait for the impossible, that is, for a guarantee that the knowing mechanism's ('science') products are 'true' or 'correct reflections' of the ultimate reality. To sum up, while it is impossible to know the objects in themselves, (for later discussion it is important to note that *this* is an epistemological stance), we are bound to work with the cognitive instruments available and to try to make the best of it.

However, it is not here where Kratochwil stops. Anticipating charges of mindless relativism, he carries on with step three, which, despite its anti-representationalist nature, aims to anchor the scientific enterprise in some acceptable harbor. With the Kantian turn in mind, of course, this harbor cannot be 'reality' itself as the various breeds of philosophical and scientific realist would like us to believe. Therefore, the argument goes, we have to move inward, to the knowing subject, in order to introspectively reflect and "critically examine the criteria that lent some force to our assertions (Kratochwil, 2007a: 4). This is where the path of Kratochwil and that of Kant diverge. Where Kant relied the transcendental reason, where the neo-Kantians relied on an *a priori* logic, where the crude instrumentalists or cynical coherentists relied on predictive power and pure community consensus respectively, Kratochwil follows the somewhat Bourdiean lead, who suggested turning to "socio-transcendental conditions of knowledge". (Bourdieu, 2004: 79). However, as I hope to show somewhat later, luckily, Kratochwil does not fully subscribe to this socio-transcendental view and instead pursues a position that can be described as intersubjective.

As it is the fundamental pillar of Kratochwil's pragmatic account, the socio-transcendental turn and interusubjective turns, deserve a further and deep elaboration.

However, I will save this point for later, while now I want to turn to what I have described as a fundamental challenge for pragmatism, that is, a challenge from scientific realism. It is worth to remind that the socio-transcendental step serves two purposes. First, by pointing at the fact that scientific knowledge stems not from a relationship of an individual scientist to a reality, but rather from the scientific community practices, Kratochwil finds a comfortable platform for his anti-objectivist and anti-realist stance. Second, the social embeddedness and 'rule following' nature of science also help to establish the intersubjective, and hence not relativist essence of science. As I have suggested, it is the former assertion that will be taken up at this point. Insisting that human mind does not have a direct access to reality (and even if it had it would not know), Kratochwil turned to an epistemology-based account of science, precisely as Kant did. Even if he does not suggest any particular epistemological solution, avoiding of which, after all, is *the* mission of his plea, he begins with an epistemological statement that influences all the later decisions: *human mind does not have a direct access to reality*, and we have to cope with this situation at our best.

Saying this, he sets himself open to an obvious charge of substituting metaphysical claims of ontology for equally obscured and unhelpful claims of epistemology. In another words, if *what* we know about reality is but a human product, and, thus, inherently fallible, how comes that what we know (or, better to say, what we *think*) about what *can be known* is something we should trust more? In still another words, why the general epistemological skepticism should be treated as an objective condition, if it is but another human construction? Indeed, this charge quickly came, in the form of response from an eminent scientific realist, Colin Wight (2007). According to Wight, there is more to know about the world than the human-made epistemological strait-jacket allows. Now, this position might be seen as close to the one of Kratochwil, but it is not. It is because in this case *epistemology* is not some particular way of legitimizing knowledge, but *the* general condition of what can be known about the world (in Wight's worlds, a Platonian cave of our own making). According to Wight, this human-made condition seriously limits the scope of inquiry right from the outset. As such it is unable to accommodate and to cope with the immense reality of the world (for a more thoroughgoing argument, see Wight, 2006).

As such, this is really a charge that has to be faced, since it hits an extremely sensitive point of pragmatism, one of openness and pluralism. If pragmatism cannot withstand this challenge, I am afraid that its pluralist added value can readily be lost. Unfortunately, instead of sticking firmly to his socio-transcendentalist reading of science, in his response to Wight Kratochwil (2007b: 72 – 75) slipped a into largely scholastic mode, not dissimilar to the

question-begging framework that the decades long realism-instrumentalism debate gradually regressed to. Instead of discarding the whole Wight's premise as it stood, Kratochwil inflated his account of science with yet another epistemological stuffing.

The word "inflated" was not chosen by an accident. It reflects another charge that is at hand when trying to undermine the "epistemological" turn. Here I rest on the notoriously known work by Arthur Fine (1984), who brought the idea of 'natural ontological attitude' (NOA) in to the philosophy of science. By showing what the NOA means and what kind of critical reflection it provoked, I hope to show how to cope with the above mentioned scientific realist charge.

For more than a century, the *realism instrumentalism debate* (RID), in all its incarnations, has been one of the most vivid, and also one of the most stalled debates in philosophy of science. No wonder that many philosophers have periodically tried either to escape, bridge, ridicule or transcend this dispute. These attempts have usually been associated with various breeds of anti-representationalists or anti-essentialists, who regard the entire division as verbal, meaningless or unnecessary. Apart from the notoriously known position of such tremendous figures as Richard Rorty, it was Arthur Fine who picked-up and carried the torch in seeking position that reaches beyond realism and instrumentalism² and thought to find it in his *Natural Ontological Attitude* (NOA). (See, Fine, 1984; 1986).

The underlying theme of Fine's anti-RID stance can be found in his conviction that the fundamental successes and applications of science are to be found all around us, in everyday life and culture. Since this "conspicuous success leaves little room for anything other than a common-sense acceptance of the world of science", he is quite curious why all the philosophical fuss over the issue of 'realism'. Indeed, this philosophical excitement fully inflates and attempts to transcend even that little room that is left besides the 'common-sense acceptance' (Fine, 1986: 149). What Fine tries to say is that philosophers (of science) seek to anchor and legitimize science in an external and transcendental authority. In Steve Fuller's words, Fine accuses philosophers and historians of science from extracting "surplus philosophical value from the historical labor of scientists" (Fuller, 1994: 200).

By doing this, philosophers of science keep pumping metaphysical (in the case of realism) or epistemological and semantic (in the case of instrumentalism) stuff into the science which can easily get along without these. Consequently, Fine accuses both, realism

² Interestingly, in his text, Kratochwil briefly mentioned Fine's contribution to the RID (2007b, 76e). However, he charged him wrongly with an attempt to 'mediate' the debate, which, (as I hope is clear) is not the case. This misunderstanding might also be responsible for not taking Fine's argument seriously.

and instrumentalism, from unnecessary and perhaps venomous inflationism (Fine, 1986: 170). For this, both positions are ‘unnatural’ and neither of them can hope in getting the laurel of the winner in the RID precisely because neither of these paths leads to an end. Is it not, then, more appropriate (and natural) to step back from the philosophical pedestal, to give up searching solid and transcendental anchorage outside the scientific field itself? This is exactly the step NOA proposes: to retreat from and to stay within the limits of the scientific practice. In another words, to *trust* science as it is. As Fine argues, “NOA urges us not to undertake the construction of teleological frameworks in which to set science. It suggests the subversive idea that perhaps there is no need for authority (inner or outer) nor for general authenticating. NOA whispers the thought that maybe we can actually get along without extra attachments to science at all, and NOA certainly proposes no additions that go beyond the history and practice of science itself. NOA is thoroughly deflationist.” (Fine, 1986: 172)

It was above all the charge of inflationism that provoked the fiercest response on the part of both – realists and instrumentalists alike. As Paul Abela nicely wrote, realists have attempted to defend metaphysical commitment by emphasizing the explanatory merit of this approach, while “[i]nstrumentalists have emphasized the importance of how belief is justified, reaffirming the priority of epistemological considerations over ontological concerns“. Despite their efforts, both do in fact “share a commitment to the essentialist view that science consists in a set of practices that are amenable to the discovery of intrinsic aims and purposes”. (Abela, 1996: 74).

At one point, Kratochwil (2007a: 2) actually gets close to the NOA reading of science, when he argues: “Should we not follow the path earlier scientists took when they rejected the metaphysical claims that in order to know anything about the world and its different forms of beings, one had to understand first ‘being’ itself?” Yet, instead of stopping here, he turns to ‘understanding’ of the epistemological criteria that inform our research and commits precisely the ‘inflationist sin’. If we accept Fine’s NOA, we can readily jump to a conclusion that this move was wrong. However, I do not think this is necessary. On the contrary – Fine’s contribution attracted rather extensive critical attention.

Of the various responses that Fine’s approach brought about (see, e. g. Fuller, 1994; Kukla, 1994; Abela, 1996; Brandon, 1997; Hendry, 2001), it is E. P. Brandon’s Steve Fuller’s contributions that is of a special importance for the further development of my argument.

Before focusing on these, it should be clearly stated that the following discussion is supposed to flow into lowering the ‘essentialist’ commitment that was highlighted by P. Abela (above), by, partially, slicing off the ‘intrinsic’ part of Kratochwil’s pragmatism.

As E. P. Brandon argues, “NOA may well come closest to the normal thinking of a normal scientist (...) [a]nd that normal thinking may not be too precise about issues that worry philosopher“. Put it differently, “if you can make it do something, it is real” (Brandon, 1997: 234). As such, within science itself, NOA may work quite well.

But, consider the rather lengthy following quotation“ from Brandon:

“The essentialism Fine claims they share surely reflects this fact about such practices as science, astrology or fundamentalist revealed religion, as against practices like baseball and cricket: that the former are seen as dependent to some extent upon, and thus answerable to, external and uncontrollable factors, and so have monopolistic tendencies within their domain of application built in, whereas with the latter there is no inherent tendency against proliferation, because everything that matters is internal to the practice. Realism and correspondence theories of truth no doubt push the explication of the former tendencies further than most participants would naturally go (...) But these philosophical constructions are attempts to address a genuine issue, for humans (...). To echo Gellner (...) the attitudes or Weltanschauung characteristic of natural science are far from natural for members of the human species, or typical of most of its history. Whether or not the predominant -isms in philosophy of science successfully characterize the striving towards objectivity that marks some (...) of our social practices (...) it is prima facie there to be found and ought not to be taken for granted by too unreflective an endorsement of our contingent form of life. Fine considers no alternative to trust in the 'overall good sense of science and our overall good sense'. When we broaden the options by allowing for seriously opposed attitudes to the achievements of science (...) may be that the -isms Fine rejects as powerless within the enterprise can be refurbished to answer [some] wider questions (...) Realism, and instrumentalism in its own style, tell us to prefer science to magic, or this science rather than that self-professed science, by supplying principles that are intended to rule out a lot of other belief systems. Such principles try to articulate the good reasons why 'conspicuous success leaves little room for anything other than a common-sense acceptance of the world of science' (...) In simply taking this for granted, NOA arrives after the serious selections have been made. (Brandon, 1997, 234-5)

Having said this, Brandon did an impressive job in undermining Fine’s argument that there is something ‘natural’ in the scientific development. Instead, it always takes conscious and serious efforts, even if stakes and outcomes are far from certain. More

importantly, these efforts are (co)informed by the larger social setting. Therefore, the philosophical ‘inflation’ is not intrinsic to philosophy and/or science; rather it is part of the reflection of the overall societal development. This assertion relieves a bit of the ‘essentialist’ charge, since it aligns the big ‘isms’ in philosophy of science with its societal context.

What can be also derived from Brandon’s exercise in ‘socializing’ and ‘historicizing’ philosophy of science is an assertion that neither philosophy of science neither epistemology can be said is ‘finished’ or ‘complete’ at any given time. And this is precisely the argument that can be used against the Wight’s accusation from locking all the possible human knowledge (past, recent and future) in to the predefined, slim, epistemological box. Why Wight’s argument seems so convincing is because he wants us to believe that epistemology (and, thus, the horizon of what can be known) is defined once for all from the times of ancient Greeks. Indeed, *the* cornerstone for Wight’s ‘ontological revolution’ is a notion of progressive science while epistemology remains unprogressive. While history of science shows that epistemological contemplations *change and develop* and even ‘wrong’ epistemological projects yielded valuable scientific innovations at its times, it is clear that Wight’s accusation must be off the point. Taking epistemology ‘socially’ means that it can be changed and developed over time even if it does not provide us with the transcendental Archimedean point, which, apparently, scientific realists need to have in possession in order to take any introspective epistemological reflections seriously.

However, for pragmatism, it is crucial to bear in mind that these reflections that are able to move or/and reshape our epistemological horizon (note that this in no way presupposes a progress toward greater objectivity or to truth!) are deeply socially conditioned, both within the scientific community itself and in the social context of science in general. Indeed, Kratochwil himself hits on this point (albeit in rather another context) when saying that “the impact of science on the industrial revolution through technology about a century and a half ago was based on then held ‘theories’ *which by now are utterly discredited*” (Kratochwil, 2007b: 71, emphasis original). To put it in another framework, if ‘wrong’ theories can be instrumentally fruitful then – how would we find out that they are ‘wrong’ if not by conscious and critical introspection (from this point of view, the ‘miracle’ argument that scientific realists love to argue with seems actually to turn against them)?

We can further this discussion and add another important dimension to it by focusing on Fuller's reaction to Fine's NOA³. In accordance with his 'social epistemology' grounding, Fuller aims to uncover the very root/rationale for RID which, as he argues, has been lost under the debris of the century-long dispute. And, Fuller's arguments goes, if we lay bare these fundamentals, as well as the original stakes in the battle and expose them to the beam of particular historical context, RID can be said to deserve "a new lease on life" (Fuller, 1994: 200). It is worth to recall that Fine's critical argument rested on the assumption that scientists themselves simply do not care about the philosophical debate and carry on with their daily business and that the stakes involved in the clash are without merit. While the first point might very well be accurate observation today⁴, Fuller's analysis aims to show that at times of the outbreak of the debate the exact opposite was the case, why and how much RID has mattered to scientists (Fuller, 1994: 201).

As he put it, [w]hen, say, Boltzmann and Ostwald, or Planck and Mach, argued about the existence of atoms, they were not merely trying to second-guess what empirical research would eventually show; rather, they were trying to influence the direction that such research should take and the way it should be evaluated. Building upon works of J. Blackmore and J. Heilbron, Fuller uncovers the essential strands of instrumentalism and realism in the dispute between E. Mach and M. Planck, respectively. For Mach, instrumentalist, science was "an abstract labor-saving device that facilitates the satisfaction of human needs, thereby freeing up time for people to pursue other things. Mach did not hold the practice of academic science to be itself an especially interesting or ennobling pursuit". Saying this, Mach actually denied the prevalent view of his day that it was only the state-of-the-art physicist who had a privileged epistemic access to the "deeper" realm of reality. Or, to put it better, that claiming this privileged epistemic-access pretension does no good to science at all. The value of science was one of an ability to "economize on thought", not one of seeking an exhaustive explanation in terms of ever deeper, and ontologically ever more prior type (Fuller, 1994: 202; 205)⁵. Behind this perspective laid an anti-Comte caution about stretching the authority of science beyond its actual realm.

³ It should be noted, that Steve Fuller wrote his text as a contribution to a debate over the fate and nature of history of science, not one of philosophy of science.

⁴ Here we can of course speak solely about natural sciences. Social sciences, including IR, present a completely different picture, with this paper being an eloquent proof.

⁵ Indeed, recent IR realists rest precisely on the account opposite to that of Mach, that is, on the idea that the deeper a phenomenon or cause lies the more ontologically prior it is. Various forms of ontological emergentism (see Wight, 2006/7) are an eloquent proof. As another realists, George and Bennett, show, this ontological considerations have an immediate impact on the practical methodology (George – Bennett, 2004)

This was precisely the attitude that Planck was opposed to: in his view science imparts an ever more coherent world-picture “whose strictures could deepen the understanding and formalize the practice of virtually any field“. For Planck, the epistemic distinctiveness of science and physics in particular “the epistemic distinctiveness of physics lay in its ability to reach closure on an ever wider body of observations by a larger number of observers, all encompassed under a single unifying theory“ (Fuller, 1994: 203; 205). Furthermore, to gain such a coherent knowledge, one needs to invest an increased effort without the warranty of a direct practical benefit, which was *the* cornerstone of Mach’s instrumentalism. To use an uncharitable simplification: Mach’s instrumentalism intended to *subject* science to society or to any individual need, regardless the value orientation of science. Against this, Planck’s realism sought to place science with its rational world-picture and with its intrinsic value orientation *within* society. The fundamental difference is here revealed - to see theories as instrumental “equations” designed to smooth the process of transmission, storage and retrieval of information cannot be more distant from attempting to *see* the world *through* these theories, through an overarching model of reality and to *live* in accordance with it. In a way, the issue in stake was also one of reflectivity – Mach’s scientist does not need to think reflectively about the underlying principles of a theory in order to make it to do its job exactly as an ordinary computer user does not need to know what is going on behind the metal case in order to carry out his task. Instead of an ongoing ever deeper reflection, scientists might as well do something else with their time. As Fuller puts it, the same applies to, for example, the core of Newtonian mechanics, “which can be effectively black-boxed as computational devices”. (Fuller, 2001: 114) Planck’s scientist, instead, ... doplnit (

Potential pitfalls of both lines of thought are now becoming apparent. In order to avoid an overextension of scientific authority and in order to avoid a harmful indoctrination of science candidates, Mach urged for a liberal-like autonomy of science without any signs of quest for a value that transcends its instrumental use. However, denial of value orientation intrinsic to science implicitly gives a way to recognition that science can be put to disposal of any purpose whatsoever. As Fuller puts it, the World War I. was soon to show that “for every Mach who resolutely refused to involve his scientific expertise in the war effort, there were plenty of Machians who ‘freely’ enrolled in the Kaiser's cause”. (Fuller, 1994: 206)

To overcome this danger, Planck argued against a division between science and values. Before science can be instrumental to society and serve it, its own intrinsic ends must be served (Fuller, 2001: 103; 116). Because of its unique ability to add a rational and disciplined thought to any line of work, science was to be socially recognized alongside with other

branches of society (state, religion, industry). Science has to be recognized for its own intrinsic value as a pure goal of itself. But, if we are to believe that the value of science lies in its ability to gain knowledge that is deeper, more rational, more coherent and truer than that of the rest of society, we have to acknowledge that we are facing an ever growing qualitative divergence between science and the rest. However, this seems to grant science a privilege that extends its authority to an extra-scientific realm. According to Fuller, Planck's view implied "that ordinary people would either have to learn how to rearticulate their experiences in terms of the new physics or simply to let the physicists speak on their behalf" (Fuller, 2001: 118) That Fuller's rendition of the realist stakes does not miss the point is well illustrated by even a passing look at the leading philosophical journals of the first decades of the 20th century. The notion that "a real objective world, with its own physical nature, its own natural laws, its own principles of cause and effect" must be learned before it can be used was thought to be normal, simple and natural (Bush, 1923: 701). However, the two conceptions – realism and instrumentalism were not thought as only opponents, but as natural mergers too. The problem was not about the nature of reality, but of the nature of knowing mind. Lingering in the centuries old debate, the fundamental quarrel could be paraphrased as follows: is mind a wax-tablet, passively reflecting anything that comes across or is it instead an active initiative instrument of knowing, an antenna that "feels the way for the organism" (Bush, 1923: 701) This conception of instrumentalism was not thought to be in any clash with realism, instead, realism was thought to be an underlying principle (see, e. g., Montague, 1909). For us, this boils down to a question, is instrumentalism usable in Mach

Interestingly, John Dewey, thinker who is often portrayed as a father of instrumentalism⁶ (but who is also widely read as pragmatist) tried to take another path. Namely, in that technology and science, which appear to be both the same in Mach's account are held separate by Dewey. According to Dewey, science is "self-conscious and specialized inquiry", while technology is embodied "in operations conducted for a practical end instrumental apparatus that was invented to attain practical utilities and enjoyments" (Webb, 2002: 982). However, perfectly in line with Dewey's reasoning, it is *technology* that precedes *science*. The following Dewey's quotation illustrates this point more than sufficiently:

"All of the procedures and techniques of inquiry that yield stabilized beliefs, upon both the common sense and scientific level, are operations existentially executed (...).

⁶ Presented conflation of J. Dewey and E. Mach is not to say that both thinkers can be placed on the same board in every respect of their philosophy of science. In their instrumental treating of science they closely resemble each other, Dewey was short of the straight instrumentalism that seems to have kept Mach from considering the *ends* of science themselves.

The cumulative effect of these operations conducted for a practical end is to give authority to a set of conceptions made familiar in a given culture. Competent science begins when the instrumentalities employed in operations of inquiry are adapted and invented to serve the purpose of inquiry as such, involving development of a special language or set of symbols". (Quotation borrowed from Webb, 2002: 982)

This approach seems to bridge the division between the intrinsic and instrumental value of science, yet, it firmly sticks to the instrumentalist reading of philosophy of science. While this point of view bestow to science its own intrinsic aims – that is inquiry as such – this kind of science can only emerge from the ‘cumulative effect’ of practically motivated operations, and from ‘a set of conceptions made familiar in a given culture’. Both points fundamentally drift technology (as precondition for science⁷) away from any realist platform. The former provides this drift by putting instrumental desire and practice prior to knowledge, the latter by historicizing knowledge and, even more importantly, science as such. Historicity of knowledge is given by grasping it as believes that are stabilized within a particular culture; historicity of science is given by the fact that it arises only after this culturally framed knowledge acquires enough of authority within the particular milieu that it can transform itself into an intrinsic enterprise. As Web puts it, “Dewey saw scientific symbols, concepts, and theories as “social constructs” in the special sense that they are produced through a public and collaborative effort and, rather than being ‘discovered’ in some pure and ultimate state, they are constructed. But scientific symbols, concepts, and theories, though ‘socially constructed’ are not constructed from some infinitely malleable ethereal material according to the whim of an isolated intellectual. The ‘tools’ of science must confront and conform to a recalcitrant reality and be open to the critical scrutiny of the community of inquiry” And, what is essential, the ‘data’ involved in scientific inquiry are not given, but taken. They are selected, refined, interpreted through specially designed instrumentation and so on. (Webb, 1999: 993; 994)

Thus, we are facing two worlds, two competing vision of science, deciding between which is largely normative and individual choice. From this point of view, RID is deadlocked in a deep mutual suspicion which flows in a following dilemma: What is to be feared more: a closed science that has been reduced to an off-the-shelf technology (Planck's fear) or the mass indoctrination of a scientific theory that affords it an extra-scientific significance that it does not deserve (Mach's fear)?

⁷ According to Dewey, however, the process of technology-becoming-science is not natural and requires effort, training and reflection.

Looking at the most recent development at critical or scientific realism, one has to wonder how much, in its most fundamental presuppositions, it resembles the more than a century old stance as described above. As a result of the ancient pressure of hermeneutics on recognizing the distinct character of social world, namely in conceding that its existence is not separate from actions, ideas and understanding of the agents who populate it, recent critical realism indeed goes beyond any simple naturalism (see, Dean et. al., 2006: 10) On the other hand, it still expects that reality has its hidden secret and above all intransitive dimension which lingers beyond the actual and experiential dimension of life. The “real cannot be reduced to the actual or empirical” (Dean et. al., 2006: 12) As a result, there is “a gap between the real and our experience of the real and (...) this gap tends to promote systemic misunderstandings regarding the nature and significance of everyday experiences” (Dean et. al., 2006: 3). It is by closing this gap that realists hope to emancipate society from an oppression that goes unnoticed to the laic eye. But realists aim to go even further. They believe that “[t]he Enlightenment removed purpose and meaning from human existence, yet failed in its attempt to provide a robust epistemological framework that could take its place” (Wight, 2006: 34). The emancipative nature of knowledge that realists hope to generate is thus one of the vehicles that could bring the “purpose and meaning” back to human existence. As the old hard-core realist had it, what we are looking at here is an ontology with some intransitive hidden structure that can tell us what to do and what to escape from and science is here to communicate with that structure (even though, as realists concede, science is not sufficient in that endeavor). Despite all the efforts to save the uniqueness and responsibility of an individual (Wight, 2006), this account of science cannot but leave a deep suspicion that science can stand somewhere above society and change it not as a response to the societal impulses, but as a result of ‘true scientific’, ahistoric knowledge. If this is to be true than to be scientist really means something, for he looks at the world through totally different lenses, and this look also lends to a different way of ‘handling’ life.

There is no need neither place to engage with this realist fallacy. What should be added, though, that for to launch this noble undertaking, realists *have* to believe in their particular ontology. For if the hidden intransitive structure which informs the emancipative knowledge proves to be chimera, the entire scientifically driven emancipation explodes.

If we keep *this* picture of realism in mind, we find out that the normative stakes involved in the otherwise abstract realism-pragmatism debate are strikingly high. Nothing can be more distant from the pragmatic notion of science than the search for emancipative

true. To think of new encounters through a 'deep' and to the untrained eye 'hidden structure' of unite science is for pragmatists above all a naïvely and potentially harmful retrospective mode of thinking. If theories are to be treated as accurate reflections of deep reality and not as mere creative reflections of precedent observations, one runs the risk of treating novel practical situations solely by the application of precedents and, thus, limits the scope of new inquiries (which is also partially point of Kratochwil, 2006). This position is best characterized by Dewey himself: "anticipation [should be] therefore more primary than recollection; projecting than summoning of the past; the prospective than retrospective" (cited by Bush, 1923: 707). For pragmatists, knowledge is not emancipatory, it must be useful; it does not help to lift an oppressing conditions, it tries to help to navigate within complex conditions. It follows that pragmatism sees science above all as responding to the socially informed impulses, both in the way research is organized and the knowledge that is searched.

On the other hand, as Steve Fuller rightly warns, "a science that does not postulate its own intrinsic, or 'internalized', ends runs the risk of becoming captive to its context, (...) overadapted to its environment" (Fuller, 2001: 103). It seems to me that in this regard, Friedrich Kratochwil manages to withstand this challenge, albeit with some reservations. At this point we can turn our attention, to the socio-transcendental, or inter-subjective twist as mentioned above.

F. Kratochwil concedes that his way of approaching the issue of scientific knowledge is close to thinking of Pierre Bourdieu and Steve Fuller (Kratochwil, 2007a: 13). Here, however, one should pause for a moment, because both authors just mentioned radically differ in certain features. Let's first review Kratochwil's own position. As he argues, he discards the 'old sociology of knowledge', by which he means the cynical account of science, radically relativizing scientific knowledge to the point that whatever works for most people or whatever they decide to believe is true. This form of coherentism beyond the realm of acceptable if we want to navigate between both extremes – transcendental foundationalism and radical relativism. The deflection of radical coherentism is in Kratochwil's account grounded not intrinsically in the science; instead, it is put into a wider social context. As he argues, "the scope of the agreement among a group of people 'agreeing with each other' is not the same as that of generalized medium of exchange established throughout the entire society". The objects that a group of people agreed upon simply cannot be "willed into existence even if there are several people engaged in such wishful thinking" (Kratochwil, 2007b: 59). Thus, it becomes clear that intersubjectivity of

‘the entire society’ in which science is placed is *the* precondition of scientific knowledge and via this step he departs the radical constructivist account of scientific knowledge. Thus, Kratochwil explains epistemic authority by pointing at what society lets science to assert rather than *vice versa*. This, however, fundamentally widens the notion of the ‘social’, which, subsequently, needs much more explicit attention that has so far attracted, not only in pragmatism, but in IR in general. The widening of the notion of ‘social’ within Kratochwil’s pragmatism is all the more interesting, since otherwise he seemed to follow a more Bourdiean line that puts emphasis strictly on the scientific practice within scientific community itself.

While it is true, that Bourdieu denies the micro-social (‘laboratory’) studies of small scientific communities and focuses on “an overall theory of the scientific space” (Bourdieu, 2004: 32 – 33) he never assess the influence of the society external to scientific field, on the contrary, treats it as sufficiently autonomous realm (as against the Edingurgh school of sociology of science). He clearly explains this by asserting “that to say that the scientific field is relatively autonomous with respect to the encompassing social universe is to say that the system of forces that are constitutive of the structure of the field ([internal] tension) is relatively independent of the forces exerted on the field ([external] pressure). It has, as it were, the ‘freedom’ it needs to develop its own necessity, its own logic, its own *nomos*” (Bourdieu, 2005: 47). The logic of generating scientifically valid products is in Bourdieu’s account fully internal to science and the validity is not discredited by the very fact the internal logic is partially explicable by social variables. Bourdieu also explicitly denies the view that science is analogical to utilitarian and cynical battle for (political) power, a war of all against all, as well as science cannot be conceived as an idealistic homogenous society of people united by common purely epistemic concern (Bourdieu, 2004: 45 – 46).

Because of the intrinsically scientific, specific logic, Bourdieu argues it is possible to understand without appealing to any form of transcendence, “how it can be the historical site where trans-historical truths are produced”, The most fundamental of the distinctive properties of the scientific field is “its (more or less total) closure, which means that each researcher tends to have no other audience than the researchers most capable of listening to him but also of criticizing and even refuting and disproving him”. Because of this, scientists are “forced to apply at every moment all the available cognitive instruments and all the means of verification that have been accumulated in the course of the whole history of science, and so to give its full efficacy to the arbitrating power of ‘reality’ (as constructed and structured in accordance with socially defined principles) ”Thus he

concludes “that science is a thoroughly historical social fact without concluding that its productions are relative to the historical and social conditions of their emergence”. (Bourdieu, 2004: 69 – 70; 76) This is the “socio-transcendental” condition of knowledge that Bourdieu asserts: “objectivity is an intersubjective product of the scientific field: grounded in the presupposition shared within this field, it is the result of the intersubjective agreement within the field (Bourdieu, 2004: 83).

Now, I believe it is clear why Bourdieu’s account cannot be compatible with pragmatist reading of science. We do not even have to put much emphasis on the rather obscure conclusion of Bourdieu’s book *Science of Science and Reflexivity*, where the scientific field, “with all the antagonistic points of view clash in accordance with regulated procedures and are gradually integrated, through rational confrontation”, producing truth as “generalized relativity” flows into God-like “geometral of all perspectives”, to “the absolute point of view from which the world presents itself as a spectacle (...) the view without a point of view, the ‘view from nowhere and from everywhere’” (Bourdieu, 2004: 116). This alone, I believe, is a view that neither Friedrich Kratochwil neither much of pragmatists would subscribe. The major distinction, however, I see in the fact that pragmatism is inherently social and historical not only intrinsically, but also contextually. It produces historically and socially conditioned knowledge by using historically and socially conditioned cognizing processes.

But if it is so, then the notion of the ‘social’ must be explored with much greater care than it has been done so far. For example, in his Tartu lecture and subsequent reactions Kratochwil denied the *qui bono* question, he suggests to be agnostic about the question who profits from knowledge (Kratochwil, 2007b: 59) In the context of his contribution, this position is fairly understandable as it tries to avoid the rather simplistic instrumental version of battle for knowledge. However, is not precisely *this* also the reason to at least place the question of ‘who profits from pragmatic knowledge’? Is it society at large? In their presentation for the SGIR conference in Turin 2007, Jörg Friedrichs and Friedrich Kratochwil argue that knowledge has to be ‘useful’, for example for orientating in complex situations. This seems to open the question of *qui bono*. But, there are other, equally or even more important questions – where does the research agenda come from? How does the wider research context influence strategies of reasoning and ‘proving’ within science? How does the wider context influence the very ‘politics of science’ which, as we all know, is fundamental for our research? While these questions that have for long time resided in the history and sociology of science, they are only rarely asked within the IR or political

science (for an exception, see e. g. Steinmetz, 2005). And if we agree that the notion of 'social' is fundamental to the pragmatist reading of IR, we must take a special care in approaching them. We saw how F. Kratochwil thoughtfully touched upon this issue by pointing at the wider intersubjectivity. We could also see in other contributions to pragmatism that the question of larger context is raised, as for example by James Bohman (2002: 499) who asserts that "social inquiry is practical not simply by being instrumentally useful but by being oriented toward the realisation of normative ideals, most especially those of democracy". However, I believe that to reflect upon these questions takes much more than this and I believe that this could be one of the task of 'introspective epistemological reflection' for the future. And it seems to me, that, at least in my reading, Steve Fuller's particular account of 'social epistemology' heads more toward this direction, as he saw science embedded in and dependent on its supporting social contest, but also insulated in its practices from the influence of that context. The insulation then provides space for the philosophical and epistemological introspection, which is, in turn, again socially motivated and aims, among others, to normatively reflect precisely on this social motivation (see, Fuller, 1988; 1993; 2000)

Closing Remarks

Instead of conclusion, I cannot resist to try to answer the question of any practitioner in the field of IR who has not directly engaged with pragmatism; question which is apparent, yet often unspoken: where can I see traces of pragmatism in IR? What does it mean to be pragmatist? I hope that the previous presentation gave some sort of hint which I now try to clarify more explicitly. Pragmatism is above all philosophy, may be, more narrowly, it is a philosophy of science. It is a way of looking at science. It also can be grasp as a way how to give recent science of IR a mantle that is fashionable in the recent world that is characteristic both by 'mature' quest for ever growing pluralism and freedom while secretly maintaining its vain 'childish' hope for certainty and sense of security. Since this mantle very well suits to another philosophy, that of scientific realism, it is important to keep in mind those fundamental differences. Where scientific realism sees knowledge ever closely getting to universal reality, pragmatism sees historically conditioned, useful and practical knowledge. Where scientific realism sees burdening social conditions which scientific knowledge can help to lift off, pragmatism sees complex conditions that scientific knowledge can help to navigate through (if asked to do so).

Pragmatism is a philosophy of science that trusts the scientific community, but not to the extent that it takes the scientific development as 'natural'. It is a philosophy of science that trusts human beings and society in general but not to the extent that it would refuse to be at hand and to use its best possible means in order to provide assistance. Also, by constantly engaging with normative issues in practice, pragmatism is supposed to understand the normative value of various stakes and choices. Therefore, the notion of responsibility does not desert pragmatism, instead it should be inherent to it. In this regard, and it is not surprising, pragmatism get suspiciously close to the "classical" IR theory, if this is to be understood in terms of H. Morgenthau, M. Wight or H. Bull.

Thus, to the rhetorical question in the title of this presentation: Intrinsic Value: Trojan Horse or Savior we can conclude – both.

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