Friedrich Kratochwil's pragmatic search for a theory of international relations

Hidemi Suganami

Department of International Politics, University of Wales, Penglais, Aberystwyth, Ceredigion, Wales SY23 3FE, UK.
E-mail: hss@aber.ac.uk

In response to Kratochwil's focus on the problem of theory-building in international relations (IR), I argue, first, that history (or historical ways of asking/answering questions) is essential to IR, but that, once this is accepted, a host of questions regarding the nature and function of historical knowledge-claims must be incorporated into meta-IR investigations of the kind Kratochwil and others have engaged in predominantly from the perspective of the philosophy of science. Second, I accept Kratochwil's main thesis that the failure of foundationalism does not lead us to nihilism or relativism but point out that there is an easier way to express this thesis via Kuhn's later treatment of theory choice in science. Third, I briefly point to a number of uncertainties regarding Kratochwil's plea for pragmatism in IR theory-building.

Introduction

There are a number of things I find attractive in Kratochwil's (2007) article, 'Of False Promises and Safe Bets: A Plea for a Pragmatic Approach to Theory Building (the Tartu lecture)'. For one thing, I am impressed with his erudition, with the ease with which he sums up the central arguments of a wide range of authors whose works will only be read by a very small minority of scholars specializing in the study of international relations (IR) who have the inclination or aptitude to read and digest them. More substantively, I have an interest in pragmatism but it has not been clear to me whether there is any version that I can accept wholeheartedly. So, naturally, I read Kratochwil's defence of pragmatism with great interest.

But I also have some reservations about the initial assumption of the article as well as a few of its central contentions. First, I am not persuaded that 'theory building' should be such a major task in the study of IR as is seemingly assumed in Kratochwil's article. I tend to think that IR should be studied

historically (although this is not meant to suggest that history is un-theoretical or that history is the only way). Second, I am in agreement, I think, with Kratochwil when he maintains that the failure of foundationalism does not lead us to nihilism or relativism. But I think there is an easier way to reach this judgement than is found in Kratochwil's article and I shall try to outline it. Third, I am not entirely persuaded that Kratochwil's plea for pragmatism, in the context of IR, amounts to anything more than a suggestion that, if they keep on trying the way the community of IR scholars thinks they should go about building theories, then one day they may be lucky. But, of course, there may be no other way.

Theory: Meta-Theory = History: Meta-History

In one important passage, Kratochwil (2007: 2) states that he has 'much sympathy with the call for engaging with the actual problems of politics rather than with epistemological or entirely theoretically generated issues, such as, for example, that of "cooperation" — that is a puzzle only after one has bought into some of the more radical versions of realism'. His view seems to be that there are some problems that those who try to build a substantive theory of IR deal with that are not very important; they are pseudo-problems that arise only if we subscribe to some implausible theories. Kratochwil's example is that of cooperation. I assume him to be thinking here that 'how cooperation is possible, or likely, under anarchy' is a question that appears puzzling only if we subscribe in advance to a misleading assumption that under anarchy cooperation would be impossible or implausible.

Indeed, just a quick reflection is enough to make one realize that the very idea that 'anarchy necessarily makes international cooperation impossible or implausible' is quite unwarranted. What, after all, is the meaning of the word 'anarchy' that guarantees such a conclusion? If 'anarchy' means a chaotic situation in which international cooperation is impossible or implausible, then the argument is a meaningless circular word game. If, on the other hand, 'anarchy' means the decentralized institutional structure of the system of sovereign states, as I think it should, then whether, under anarchy, international cooperation can or is likely to happen is an empirical question to which the answer is 'yes, it is known to happen, in some cases to quite a remarkable degree' (Suganami 2001: 414–15). It then becomes interesting to investigate under what conditions international cooperation deepens or widens. Once we abandon a radically realist assumption and take a more realistic line, we can start investigating what these conditions may be.

In suggesting that there are some relatively implausible theories of IR that tend to raise 'entirely theoretically generated issues', pseudo-problems,

unproductive questions, Kratochwil is pointing out that there may be better theories than these; that there is a point in exploring such theories; that 'theory building' is a task for which it is worth investigating how we should approach it; and he is making 'a plea for a pragmatic approach to theory building'.

Under the rubric of 'theory', Kratochwil (2007: 6) has in mind two different kinds: one offering causal explanations and the other constitutive ones. The former seeks to isolate causal mechanisms that underlie the ways the world works and works out under relevant conditions. The latter I find elusive. Kratochwil (2007: 1) explains it as follows: 'To show how things fit and are part and parcel of a larger order is as much an explanation as it shows a causal connection between two phenomena, a point that has surfaced in the recent debates about constitutive explanations'. He also refers to Alexander Wendt's *Social Theory of International Politics* (1999) as an exemplary text regarding the two kinds of explanation.

But not only do I fail to find convincing evidence of specifically 'constitutive', in contradistinction to 'causal', theorizing in Wendt's theory of collective identity formation, which is the central substantive theme of his book, but I believe his discussion of the causal and constitutive explanations is in need of a conceptual overhaul.[1] Kratochwil (2007: 9) suggests that Max Weber's 'interest in ideal types and typologies (rather than generalizations) indicate that virtually all of Weber's explanations are of constitutive rather than efficient causal type'. If this is indeed so, I suppose there would be much room for 'constitutive theorizing' in the field of IR, but, unfortunately, Kratochwil's argument here is not sufficiently well developed to enable me to engage with it further. I suspect, however, that the English School's representation of world politics as combining three ideal-typical social relations (of the international system, international society, and world society) and its stress on international society as showing 'how things fit and are part and parcel of a larger order' might be a good candidate for 'constitutive theory' in conformity with Kratochwil's brief characterization of it.[2]

What has been established so far is that, according to Kratochwil, there are in the study of IR causal theorizing and theorizing of a somewhat different nature, which he calls 'constitutive'; that, according to him, some theories are worse than others in producing unfruitful pseudo-problems; and that therefore we should try to find an approach which will enable us to produce relatively better ones.

At this point, I need to say I am curious about Kratochwil's apparent neglect of historical comprehension as an approach to the study of international politics. I am not, of course, suggesting that a search for a wide range of causal mechanisms underlying how nations behave or how world politics work out is unimportant; nor is it my view that a highly systematic representation of world politics as is found, for instance, in the English School's writings on the

structural and the functional aspects of world politics is insignificant. But I also believe that many questions we raise about IR are, or had better be treated as, historical questions requiring answers by historical methods and that it will not do to ignore this point.

Of course, Kratochwil will not deny that history provides an important approach to the study of IR; it may simply be that, in this particular article, he is focusing his attention on theoretical approaches. Still, I am dissatisfied with the general tendency of philosophically minded IR scholars, who engage critically with the nature of knowledge-claims in IR, to think of their subject more or less exclusively as a social science (and not history) and to engage in philosophical discussions from the perspective of the philosophy of science/ social science (paying little or no attention to the debates in the philosophy of history).[3] I think Kratochwil's article may be taken as an illustration of this general tendency; it certainly reinforces it.[4] So, let me say a few words about how I see history and the philosophy of history fitting into IR and meta-IR discussions.

My basic position is simple. Against those who think of IR as being in some sense distinct from history, I maintain that history is in fact central to IR. I do not mean that history supplies raw data that IR scholars are somehow meant to process or that, as is often said, history is IR's laboratory. In fact, I mean the opposite of this. I think history is central to IR because the subject-matter of IR has a history/histories; the subject-matter is always already historical. So, a-historical approaches are likely to mislead. For example, a war of today and a war of yesterday are not two random instances of a category called 'war' because the war of yesterday, and our knowledge of it, is likely to have some impact on the war of today. Or an instance of peace between two liberal democracies in one historical period may not be the same phenomenon as another instance of peace even between the same two liberal democracies, let alone between another set of liberal democracies in another period.

So, in the first place I am advocating that history, in the sense of historical ways of asking questions and answering them, is beneficial in addressing questions that arise in IR. Not allowing a historical mode of comprehension as a central part of the repertoire of IR is to impoverish IR unnecessarily. However, I also want to argue that, once we allow for a historical mode of comprehension to operate in substantive IR, we must also allow for a critique of history to take root in meta-IR.

Let me explain. I am in favour of acknowledging what history can do for IR; but this comes with a new set of issues for once we acknowledge history's centrality in IR we must also take note of the epistemological and political critique of historical knowledge-claims in engaging critically with IR's knowledge-claims. In short, history will help IR address its central, substantive issues, but if IR is to take advantage of history in that way, then it must also

seriously take up the existing epistemological and political critique of history with which IR scholars have so far been on the whole relatively unfamiliar. My view is that an article such as Kratochwil's has to be balanced by an effort to bring closer to IR an epistemological and political critique of history (and what usually goes with it, the narrative mode of comprehension).

What I mean by a historical mode of comprehension can be expounded very simply as follows. Historical explanation, whether offered by a professional historian or an IR specialist, typically aims to give an improved understanding of how a segment of the world moved on from where it was at one time to where it came to be at another, for example, how the Cold War developed and ended. It does this partly by explicitly or implicitly confirming what we think we already know, partly by challenging what we thought we knew and partly by filling in the gaps in our knowledge which we wanted filled or did not know existed.

All of this is done by supplying four kinds of information at a maximum. The first concerns how the relevant segment of the world was to start with, or the initial conditions. The other three roughly correspond to what Clausewitz called 'a remarkable trinity' in discussing the nature of war:[5] (a) information concerning the occurrence of some historically significant chance coincidences (i.e., those that can be argued to have had an impact on the subsequent course of events); (b) information concerning a variety of relevant human interventions and non-interventions (and not only what they were, but how they came to take place, which usually involves a reasoned assertion concerning what went on in the minds of the relevant actors); and (c) information concerning some mechanistic forces in operation (i.e., knowledge of how it is that things tend to work out the way they did in this instance) — all contributing towards a more satisfactory synoptic view, grasping together, or a 'com-prehension', of the specific path which the process of transition took.[6]

There are some intriguing 'meta' questions that arise with respect to this mode of comprehension. The linear narrative seems to be a standard way that enables our comprehension of a process of historical transition, but is it a necessary instrument? What influences which of the maximum of four explanatory ingredients are to be selected, highlighted and combined? When we understand a particular narrative, what makes it followable and intelligible? In what ways, if any, do historical representations correspond to the segment of the world that they are supposed to represent? The world may be represented in different ways (so that there may always be more than one historical account to give), but can a single narrative be interpreted also in diverse ways, such that there may potentially be more than one story told in any act of story-telling? How do some, or some types of, historical explanations come to be more broadly accepted in a society, and what are the social or political functions of various modes of historical writing and of

history as such?[7] These are intriguing questions that tend to be neglected when too much stress is placed on IR as a social science and, accordingly, meta-IR as a branch of the philosophy of the social sciences, as is often done in philosophical discussions of the nature of knowledge-claims in IR; and I see Kratochwil's article as an instance of this general tendency.

Thomas Kuhn on Theory Choice

Still, the focus of Kratochwil's article is on theory building in IR, not a historical comprehension of world politics. I should also focus on what he is centrally concerned with, rather than what he excludes from his purview in his article. In one of the key passages of the article, Kratochwil (2007: 4) states:

> [Q]uite contrary to the charges of some of the high priests or ayatollahs of mainstream political science that a critical stance towards the traditional conception of 'science' has ultimately to lead 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 or philosophy.

Kratochwil is saying here that even though in the field of IR some theories can be argued to be better than others, the criteria of assessment cannot be established by looking into other supposedly more rigorous fields of study, such as physics. His basic contention is that there are no universally valid and trans-historically established criteria by which to assess the truth or verisimilitude of any given theory but that this does not mean that 'anything goes'.

But I think there is a simpler and perhaps more effective way of showing this than is done — in my view somewhat circuitously — in Kratochwil's article. Here I lean on Thomas Kuhn's discussion of theory choice in science.

In an important essay, 'Objectivity, Value Judgment, and Theory Choice', printed in his *The Essential Tension*, Kuhn (1977) recounts how he concluded his earlier controversial book, *The Structure of Scientific Revolutions* (1962), and how his critics reacted to it. He had famously argued that whether scientists should abandon one time-honoured theory or paradigm in favour of another was not decidable by 'proof'. A new theory or paradigm gains preponderance through techniques of persuasion in a situation in which there is no proof but argument and counterargument (1977: 321). But this gives rise to a difficult puzzle: 'why, in the absence of binding criteria for scientific choice, both the number of solved scientific problems and the precision of individual

problem solutions should increase so markedly with the passage of time' (1977: 320). On this issue, Kuhn (1977: 320) offered the following comments:

[There are] a number of characteristics that scientists share by virtue of the training which licenses their membership in one or another community of specialists. In the absence of criteria able to dictate the choice of each individual [scientist] … we do well to trust the collective judgement of scientists trained in this way.

He added (Kuhn 1977: 321):

'What better criterion could there be,' I asked rhetorically [in *The Structure of Scientific Revolutions*], 'than the decision of the scientific group?'

But a number of philosophers reacted critically to this suggestion and accused Kuhn of making theory choice 'a matter of mob psychology' for, in their view, Kuhn was advocating that 'the decision of a scientific group to adopt a new paradigm cannot be based on good reasons of any kind, factual or otherwise' (1977: 321).

In 'Objectivity, Value Judgment, and Theory Choice', Kuhn rebuts such accusations. His position is very straightforward. Any given scientific community has a number of criteria that its members take seriously in making decisions regarding theory choices. Kuhn lists five as indicative of a good scientific theory (in the community he belongs): 'accuracy, consistency, scope, simplicity, and fruitfulness' (1977: 322). But, crucially, he argues that individually the criteria are imprecise — scientists may legitimately differ among themselves about their application to concrete cases — and there is no algorithm that tells them how much weight to give to each criterion (Kuhn 1977: 322–25). Therefore, even though individual scientists will pay attention to these (and perhaps some other) criteria in making up their own minds, there is no formula that enables the community to decide which way to go. This, Kuhn (1977: 330–31) says, shows theory choice to be essentially a value judgement; it is not arbitrary in as much as it is guided and shaped by a set of values, but it is also not a determinate process dictating 'rational, unanimous choice' (1977: 326). Kuhn adds that this is not just a description of what goes on in a scientific community at a time of choosing theory, but the lack of success on the part of the philosophers of science in arriving at an algorithm has made them abandon their search. Yet this is not a bad thing at all, according to Kuhn (1977: 330):

The considerable effectiveness of such criteria [i.e., accuracy, simplicity and the like, in the light of which scientists engage in theory choice] does not … depend on their being sufficiently articulated to dictate the choice of each individual who subscribes to them. Indeed, if they were articulated to that

extent, a behaviour mechanism fundamental to scientific advance would cease to function. What the tradition sees as eliminable imperfections in its rules of choice I take to be in part responses to the essential nature of science.

Before a scientific community should come to accept a new theory, Kuhn says it has to be tested over time by those working within it as well as those working within its traditional rival. He further explains (Kuhn 1977: 332):

> Such a mode of development ... *requires* a decision process which permits rational men to disagree, and such disagreement would be barred by the shared algorithm which philosophers have generally sought. If it were at hand, all conforming scientists would make the same decision at the same time. With standards for acceptance set too low, they would move from one attractive global viewpoint to another, never giving traditional theory an opportunity to supply equivalent attractions. With standards set higher, no one satisfying the criterion of rationality would be inclined to try out the new theory, to articulate it in ways which showed its fruitfulness or displayed its accuracy and scope. I doubt that science would survive the change.

Still, the puzzle remains: how is it that science progresses, 'repeatedly producing powerful new techniques for prediction and control?' (Kuhn 1977: 332). I find Kuhn's reply both disarming and robust:

> To that question, unfortunately, I have no answer at all, but that is only another way of saying that I make no claim to have solved the problem of induction. *If science did progress by virtue of some shared and binding algorithm of choice, I would be equally at a loss to explain its success.* The lacuna is one I feel acutely, but its presence does not differentiate my position from the tradition (Kuhn 1977: 333; emphasis added).[8]

I have summarized Kuhn's argument at length for two reasons: (1) like Kratochwil, he is also arguing that there is no secure ground on whose basis a correct decision can be taken regarding theory choice, but that it does not follow that 'anything goes' — for the choice is made and cannot but be made with reference to a communally accepted set of values which, however, are historically variable (Kuhn 1977: 335); and (2) I find Kuhn's argument more economical and effective than what I have read and understood in Kratochwil's article.

In the previous section, I drew attention to the importance of historical comprehension as an approach to IR. I want to conclude this section by suggesting how Kuhn's line of thinking regarding 'theory choice' may, with due

modifications, apply to 'history choice' or to the problem of choosing between competing historical narratives.

If we are offering a narrative account of how a segment of the world moved on from where it was to where it came to be, I think we are making a claim that the narrative is true of the world. This does not mean that the world is already broken up into segments, each shaped as a narrative which it is the historian's task to uncover (Mink 1978). But it does mean that a narrative explanation we offer is sensitive to what we justifiably believe to have established about the world. If a given narrative contains many beliefs about the world which we find are not justified, we will complain that it contains many 'factual errors'. It is a small step from here to entertain the idea that there are more, or less, true narrative explanations of a historical process leading to a particular outcome.

Let us imagine two narrative explanations, N1 and N2, concerning the occurrence of one and the same event. Generally speaking, practising historians appear committed to the view that their collective aim is, on the basis of available evidence, to reconstruct the past as it actually was (or to approximate to that goal as far as possible). It may be a small step from here to the belief that a progressive approximation to the historical truth is possible and further that it is possible to tell which of the two versions, in this case N1 and N2, are more true.

It would, of course, be quite absurd to suggest that there is no such thing as evidence which plausibly supports or contradicts a hypothesis concerning the occurrence of some past event, or that the historical profession does not take evidence seriously with utmost care. But I am not persuaded that it makes much sense to judge the relative verisimilitude of N1 and N2. This is for the very simple reason that they may not be, and are in fact quite unlikely to be, addressing an identical set of questions even if they are, as in the present case, trying to explain the occurrence of one and the same event. What is quite likely, for instance, is that N1 and N2 start at different beginnings.

If a historian H1 gives N1 in response to a set of questions Q1 and H2 gives N2 in response to Q2 (where Q1 and Q2 are not identical but overlap), what should be evaluated is not the relative verisimilitude of N1 and N2 in any simple sense. In assessing their relative merits, we would ask which of them addresses more of the more important questions and which of them answers more of them or a larger proportion of them more satisfactorily. Satisfactoriness here is to some extent a function of the persuasive use of evidence, but is not exhausted by it; how much significance is accorded to various causal factors is not reducible to the issue of evidence/factual accuracy, as Dray (1978), among others, demonstrated. It is also clear that what counts as a more important question is not reducible to a matter of fact; it has far more to do with what point the historian wishes to make about the case, and this is likely to be an issue of politics and ethics.

34

By the time other possible standards of judgements are brought in — for example, relative aesthetic merits, the degree to which they succeed in revising received views — it is easy to see that N1 and N2 may have various relative strengths and weaknesses which cannot be added up in accordance with any easily agreed-upon algorithmic formula. This, of course, does not mean that we cannot assess the qualities of a historical work individually by a set of criteria held to be important by practising historians or a wider community of readers. But it seems unlikely that we find a super-criterion, that should be acceptable to all, in the light of which the relative importance of the several criteria are in turn to be determined. The absence of an overarching algorithmic formula in the community of historians, who nonetheless accept a set of values as indicative of good history, parallels the situation described by Kuhn with respect to scientific communities. In following the path of historical comprehension as a means of understanding world politics, we cannot invoke any single standard of truth in whose light we may assess the validity or otherwise of any given account; but this does not mean 'anything goes'. Following Kratochwil, we may conclude that the failure of foundationalism does not lead us to nihilism or relativism in science *or in history*.

Why a Safe Bet?

In the introductory part of his article, Kratochwil (2007: 4) wrote:

> I shall argue for a pragmatic turn in theorizing in the concluding section. I shall do so *not* in the hope of having now found a new foundation after the failure of the epistemological project ['providing us with the *fundameta inconcussa*' (2007: 11)], but with the understanding that such a turn represents a *good bet* in pursuing our research while remaining attentive to the importance of meta-theoretical issues that arise in its course.

My final question concerns Kratochwil's reason for his assertion that a pragmatic turn 'represents a *good bet* in both pursuing our research while remaining attentive to the importance of meta-theoretical issues that arise in its course.' Why is it a good bet?

Is Kratochwil thinking, like Kuhn, that in the absence of universally valid and trans-historically applicable criteria, 'we do well to trust the collective judgement of' the community of IR theorists — for what better criterion could there be than the decision of those theorists themselves? (Kuhn 1977: 321). Given the enormous success of science in securing a marked increase with the passage of time in 'both the number of solved scientific problems and the precision of individual problem solutions' (Kuhn 1977: 320), Kuhn's

confidence in the ability of the scientific community to make more progress may perhaps be warranted. His 'bet' seems intelligible. What of Kratochwil's?

It is not clear to me what Kratochwil's reasons are for supporting pragmatism as a good bet with specific reference to theory building in the study of IR. Here are some of the key points I understand from his article.

By 'pragmatism' he means 'an approach to knowledge that takes its departure from acting rather than from reason itself or from being speculation' (Kratochwil 2007: note 8). Inasmuch as it eschews foundationalism, he says it avoids 'some false starts' (2007: 11). He writes:

> Even if the most rigorous and secure system of thought turns out not to be contradiction free, this revolutionary realization does not prevent mathe-maticians from going to solve problems and most of us have to act most of the time without having the privilege of basing our decisions on secure universally valid knowledge… Letting go of unrealizable plans and notions that lead us 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 (Kratochwil 2007: 11).

'Do not concern yourself with the fundamental uncertainties of your knowledge claims, you can still go on producing such claims, and so you should,' Kratochwil says. But why 'go on'? What can we obtain by going on? Here his answer is quite categorical: not 'a simple accumulation of more and more true facts' (2007: 12) about the world out there existing independently of our knowledge claims, but 'progress' in 'being able to formulate new questions that *could not even be asked previously*' (2007: 12). Kratochwil gives much weight to the criterion of 'fruitfulness' in theory choice. But progress in theory building, in which increasingly more fruitful theories are formulated and accepted, is a social process. In a formulation strongly reminiscent of Kuhn's line, Kratochwil (2007: 12) writes:

> Pragmatism recognizes that science as a process of knowledge production is a social practice determined by rules in which scientists are not only constitutive for the definitions of problems (rather than simply lifting a veil over nature), they also debate questions which seem 'undecidable' and they have to 'weigh' the evidence, instead of being able to rely on the bi-valence principle of logic as an automatic truth finder. To that extent the critical element of the epistemological project is retained, only that the 'court' which Kant believed to be reason itself, consists of the practitioners themselves.

He adds that the court of the practitioners, as in judicial decision-making, will not yield 'determinate and unique decisions' but without thereby 'justifying the inference that this proves the arbitrariness of law' (2007: 12).

But what does Kratochwil really think that following the practices of a scientific community — a community of IR scientific theorists in particular — can yield? Has there been progress in this community, along what path, and is the story of progress so persuasive as to give us confidence in Kratochwil's bet — beyond accepting it, negatively, as effectively the only way because there is no viable alternative? I wish he had offered a fuller account here but, of course, its absence is no proof of its non-existence.

There is, however, one important observation that Kratochwil makes which must not go unnoticed — especially given my earlier remark that, by treating IR essentially as science, he neglects its intrinsic historical dimension. He considers it as a merit of pragmatism that the knowledge claims it encourages the scientific community to produce are not presented as having a-historical universal validity. But does this simply mean that the science of IR has a history so that no IR theory, produced at any particular time, can have trans-historical validity, or does it mean that since the subject-matter of IR, world politics, has a history, non-historical ways of cognition are of limited value? The former is about the historicity of theory, the latter about the historicity of the object of theorizing. I accept both. Does Kratochwil?

This is not clear from his article itself. But his basic thinking may be discerned from an earlier article which he co-authored with Rey Koslowski — even though he may have altered his position considerably in the intervening years. Kratochwil's line appears to be (or was) that no theory of international politics applies to all periods and that, in particular, constructivism is better suited than neorealism to the task of illuminating the period of rapid change between 1989 and 1991 (Koslowski and Kratochwil 1995). Thus, according to Kratochwil, theories of international politics are time-bound in the sense that they do not explain international politics 'as such' but that different theories illuminate different historical periods of international politics. If therefore Kratochwil is telling the community of IR theorists to abandon a search for a transhistorically valid theory of international politics, to stop forcing one theory to try to explain international politics 'as such', to be historically sensitive, and to explore a new theory when they seem to face a new reality, then his position comes close to accepting the latter of the two positions outlined in the previous paragraph (as well as accepting the former). But, of course, he is not abandoning a theoretical approach as such; in the article co-authored with Koslowski, he is writing a 'history' of the demise of the Soviet Empire specifically (and narrowly) from the constructivist perspective as 'a case study' to show that 'international politics is not an autonomous sphere but *always* part of the larger endeavour of institutionalizing both identities and political communities as well as their interactions' (Koslowski and Kratochwil 1995: 158; emphasis added).

One noteworthy feature of this line of thinking is that it appears to commit Kratochwil to an ontological position — that there is a historically specific reality of international politics out there to which different theories approximate to varying degrees. But this is not consistent with the position he spells out in the article under discussion. Perhaps he has moved on.

Conclusion

If I were to have written an article under the same title as Kratochwil's, 'Of False Promises and Safe Bets', I might well have been tempted to argue that the idea of theory building by a pragmatic approach, if not necessarily a false promise, was not clearly a safe bet. I might have argued that history offered a 'safer bet' in deepening our understanding of world politics, but I would have wanted a different subtitle: 'a plea for a meta-historical approach to the study of world politics'. Such an article, I have suggested, should balance the general tendency of those who engage critically with knowledge claims in IR to see IR as a branch of the social sciences and to treat the philosophy of science/social science as the main source of ideas and arguments. In my judgement, an epistemological and political critique of history should complement a work such as Kratochwil's.

Notes

1 See Suganami (2006) for a detailed discussion of these points.

2 See Linklater and Suganami (2006) for a detailed exposition of the key contentions of the English School.

3 Hollis and Smith (1990) is a good example of this tendency. According to them, IR should avoid 'collapsing into a fragmented Diplomatic History which lacks all rhyme and reason' (1990: 194).

4 See, however, my discussion, towards the end of this contribution, of Kratochwil's important acknowledgement of this historicity of theory.

5 For Clausewitz (1976: 89), the phenomenon of war was 'a remarkable trinity' — of blind natural force, the play of chance, and subordination to reason as an instrument of policy.

6 Historical comprehension — or 'com-prehension' as Paul Ricouer reminds us (1984: 76) — is grasping together of a given set of events and/or actions, conceived of as an intelligible whole, or a not-so-unintelligible whole.

7 Ankersmit (1986) stresses the constructed and opaque nature of historical narratives. Fasolt (2004) persuasively demonstrates the intrinsically political nature of historical representations. Mink (1978) points to the linear narrative as a distinct mode of understanding while Veyne (1984) stresses the importance of plot as a mode of comprehension. White (1973, 1978, 1987) discusses many issues relating to the politics of historical representation in considerable depth. None of the works cited here, however, deals with the series of questions listed in the text in a comprehensive manner.

8 Newton-Smith (1981) offered a realist defence of the rationality of science in his attempt to fill the lacuna.

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About the Author

Hidemi Suganami is Professor of International Politics at the University of Wales, Aberystwyth. His publications include *The Domestic Analogy and World Order Proposals* (1989), *On the Causes of War* (1996) and, with Andrew Linklater, *The English School of International Relations* (2006). He is currently studying issues arising in the philosophy of history and their implications for IR.