[Peter Wilson](#)

Where are we now in the debate about the first great debate?

Book section

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Much progress has been made in understanding what happened in the early years of the field of International Relations (IR) and how it came to be represented as it did. While there may not be a consensus on how the early decades of IR should be characterized, the simplistic representations that held sway in the past are now widely rejected, and there is a far greater appreciation of the complexity of the ideational and discursive reality of the time. It is gratifying that my "Myth of the First Great Debate" (Wilson 1998) was a spur to the growth of this appreciation. But while I have contributed to this growth in various ways since its publication, particularly through my work on Woolf, Carr and Murray, I have not hitherto engaged, at least not directly, with the other main contributions to what might be called the debate about the first great debate. This is what I intend to do in this concluding chapter, with the overriding objective of discerning where we are now in this debate.

PROCESS OR PROGRESS?

Andreas Osiander contends that the central premise of early IR writers was not so much a notion of progress, but "inescapable, directional historical process" (Osiander 1998: 409-10). These writers had a directional as opposed to cyclical philosophy of history. Their great insight was that international relations were not static but evolving. The dominant

conservative/"realist" view of the unchanging nature of international life failed to take into account the huge forces for change unleashed by the industrial revolution. The world was becoming increasingly and inexorably integrated. Any understanding of modern international relations had to embrace this process and the dynamics propelling it—and also understand that while interdependence had grown almost naturally, the means of controlling it had also experienced remarkable, and largely spontaneous, growth. These were manifested in the array of official and unofficial bodies, organs, and associations, to which Woolf gave the name "international government", which had sprung up since the early nineteenth century. According to this view world politics was in transition towards greater organization. *A propos*, the League of Nations was not a revolutionary but the latest and most striking development in a deep rooted socio-economic and political process.

Osiander is right to emphasise this 'process' aspect of the thought of early IR thought. While some earlier studies of the period have eluded to it, they have arguably not given it the weight it deserves. Yet it could be contended that the distinction between progress and "inescapable directional historical progress" is somewhat specious. Is not the direction identified by these thinkers always "forward"? Does this not imply progress? Osiander himself provides evidence to this effect. He suggests, for example, that there is a philosophy of history behind Zimmern's view of international relations that bears a striking resemblance to Kant's thesis about reason and progress in *Idea for a Universal History* and *Perpetual Peace* (Osiander 1998: 420). He explicitly refers to Mitrany's view of progress, self-consciously Kantian, as an inevitable

if laborious product of tensions embedded in history (Osiander 1998: 421). He refers, to give one further example, to a chronological sequence common in early twentieth century IR thinking that is pretty explicitly progressive: involving evolution, though not always smoothly, from the power politics of the past to the cooperative interdependence of the future (Osiander 1998: 427).

An additional objection can be made. While the assumption of an "inescapable directional historical process" driven by industrial modernity was a prominent feature of the writing of the cluster of internationalists with whom Osiander is most concerned, namely Angell, Woolf, and Zimmern, it is not a prominent feature in the writing of all the significant internationalists of the time. The idea of a creative tension between economic integration and political fragmentation, between the forces unleashed by the industrial revolution and those unleashed by the French Revolution, was far from universally shared. The idea is not prominent in the writing, for example, of Philip Noel-Baker (see Lloyd 1995), David Davies (see Porter 1995, 2002), and Gilbert Murray (see Wilson 2007, 2011). The "industrial modernism" that Osiander identifies as a striking feature of "idealism"—and it should be noted here that while rejecting the traditional over-simplified image of IR idealism, Osiander asserts, *contra* Long and Wilson (1995), that it still makes sense to talk of a unified and distinct idealist paradigm—is not a universal feature. In brief, Osiander's construction of the idealist paradigm is in fact a partial reconstruction. In effect, he has dismantled the paradigm and rebuilt it with the materials he finds most to his liking, discarding those that do not fit his particular account. His "alternative narrative" to the "founding myth of the IR

discipline"[1] does not, therefore, quite hold true. Similarly, it is not quite true to say that revisionist writers, while successfully putting a 'dent' in this founding myth, have not offered an alternative narrative (Osiander 1998: 411). One alternative narrative is that there was no coherent and self-consciously idealist school of thought in the inter-war period. Rather, this period spawned a diversity of liberal schemes for war avoidance which self-styled "realists" were to erroneously, and damagingly for disciplinary development, lump into a single "idealist/utopian" box (Wilson 1995; Long 1995).

For these reasons I do not feel the need modify my original position that a core characteristic of early twentieth century liberal internationalist thinking is belief in the possibility of progressive change: belief that the world does not have to look the way that it is, and that through reason, courage, imagination and determination it is possible to arrive at a better way of being and living (Wilson 1995: 13; 1998: 10; see also Sylvest 2004, 2009). I readily concede, however, that for some writers a more peaceful and orderly future was immanent in history. In particular, it was immanent in industrial modernity, and its achievement therefore was not wholly a matter of will or "agency".

While maintaining my original position on the character of early liberal internationalism I certainly accept Osiander's observation that post-war reconstructions of "idealism", starting with Carr, overlooked the "process dimension" of much early twentieth century internationalist thought, and underestimated the extent to which it underpinned liberal faith in progress (see e.g. Bull 1969: 33-36). But why did they fail in this regard? Osiander

does not answer this question. In the case of Carr, the author of the most famous reconstruction, the picture is complicated. It is true that the political thought of two of Carr's main utopian targets, Norman Angell and Alfred Zimmern, contained this process dimension, but Carr discounted it. Those further to the left of the political spectrum whose thought contained this dimension in abundance -- e.g. Leonard Woolf, Harold Laski, and David Mitrany -- interestingly were not condemned by Carr as utopian. This dimension was particularly strong in what later became known as new liberal, welfare or constructive internationalism (see Suganami 1989: 100-11; Sylvest 2004: 414-16). This outlook has been plausibly identified as one of three which together constitute the broad church of inter-war liberal internationalism, the other two being Hobbesianism and Cobdenism/non-interventionism (Long 1991: 293-99). While logically separable, in the thought of actual thinkers of the period these strands of liberal internationalism were often intertwined, their salience being issue-specific and conditioned by circumstance. That is, depending on the issue at stake, or the prevailing political circumstances, political thinkers and publicists such as Angell, Woolf, or Zimmern, would frame their broadly liberal, progressive, League internationalist argument in terms of one strand or another. The Hobbesian strand was most prominent at times of acute international crisis. The Cobdenite strand was most prominent in international economic matters, reflecting *inter alia* the free trade and "open door" orthodoxy of the League. The third welfare internationalist strand was most prominent when it came to matters of post-war reconstruction and (Osiander makes this point most persuasively) in opposing traditional (i.e. realist/conservative/sceptical)

doctrines of foreign policy and attitudes towards international reform. These three stands were not part of a carefully delineated, methodologically rigorous, and systematically constructed international political ideology, but rather discursive strategies used as the need arose by a variety of peace-, reform- and League-orientated thinkers. Used, it should be said, quite naturally. Not "weapons of choice" in an armory as much as increasingly well rehearsed responses to a range of practical and intellectual problems. With regard to Carr, however, the fact that needs to be stressed is that he shared this "process" view of history, paradoxically maybe given his realist reputation. This goes some way in explaining why Carr understood utopianism—contrary to subsequent portrayals—in terms of the first two strands but definitely not the process-orientated, welfare internationalist, third. Carr did not condemn, in other words, all forms of liberal internationalism. He not only happily embraced but arguably was the most able[2] champion of the welfare internationalist form. So by this necessarily complicated route we can begin to see that Carr did not overlook the "process dimension" in his treatment of idealism/utopianism. Rather he never conceived idealism/utopianism in terms of this particular understanding of the historical process. For Carr, in other words, this was not a "dimension" of idealism/utopianism at all. On the contrary it was a fact of modern economic and social life that any realistic plan for international reform (for which his *Twenty Years' Crisis* cleared the way and laid the foundations, and his *Conditions of Peace* erected the superstructure) needed to thoroughly take into account (see Wilson, 2001).

One final point is worth making about Osiander's account. He demonstrates that early twentieth century IR thinkers were conversant with the way of thinking that would later be called realism. Realism did not come at them in 1939 as a bolt from the blue, as a consequence, as it is sometimes suggested, of some kind of Kuhnian scientific anomaly (see Vasquez 1983: 13-19). Far from it, they were thoroughly aware of this outlook (characterising it in terms of the international anarchy, state sovereignty, endemic war, and the balance of power); they had names for it (e.g. 'the diplomatic orthodoxy'; see Osiander 1989: 414; Wilson 2003: 32); and much of their work was addressed implicitly or explicitly in opposition to it. For this reason, argues Osiander, the so-called first great debate between idealism and realism does deserve its name. The work of early twentieth century IR authors consisted in part of "an ongoing, explicit or implicit dialogue with the position later labelled Realist" (Osainder 1998: 415). What Osiander is referring to here, however, is not so much a first great debate in the discipline of IR between two well-defined, opposing, and self-conscious schools of international thought. Rather he is referring to what Carr called the twin elements of international political life, utopianism and realism, which are always present when the relations between independent political communities were discussed. It is the dialogue between those who accept the international anarchy as an immutable fact of life and work within its constraints, and those who seek to transcend it or at least substantially mitigate its most damaging effects e.g. great power war. It is idealism/utopianism vs. realism in this broad sense, whose protagonists include Paine, Kant and Cobden on the one side, and Pufendorf, Burke, von Gentz on the other, to which Osiander, it seems to me, is referring. Whether

this is an accurate and constructive way of thinking about the history of international political thought need not detain us. The fact is that Osiander detects and draws out from the early twentieth century writings this largely implicit contest on international relations and applies the "great debate" notion to it. In doing this he perhaps confuses what Carr and others have seen as a perennial contestation in the theory and practice of international relations with a specific debate between two schools in the fledgling discipline of IR.

CRITICAL INTERNAL DISCURSIVE HISTORY

A number of important points about the field of IR during its formative decades were made also in a 1998 piece by Brian Schmidt, summarising the research on the inter-war period presented in his major book (1998a). He traces the field back to the late nineteenth century and demonstrates that in these early years it was a good deal more realist than the conventional IR image suggests. Prior to World War I, the field was dominated by a juristic theory of the state that had its roots in the writings of Hegel, Fichte, Treitschke, and Bluntschli. This resulted in an outlook that was sceptical about the existence and efficacy of international law; pessimistic about the prospects of world order reform; positive about the role of sovereignty; and accepting of the anarchical nature of international society (Schmidt 1998b: 439-443; see also Schmidt 1998a: 43-76). The dominant discourse in the interwar period, according to Schmidt, was the pluralist rejection of this juristic theory of the state. This led to a more optimistic assessment of the role that both international law and organization could play in international relations.

Pluralist international lawyers made their case primarily on empirical grounds. They argued that the juristic theory was not corroborated by state practice. Internationally as well as domestically it could be observed that there existed a body of rules widely recognised as enjoying the status of law. Their argument for extending the rule of law internationally, contrary to the conventional image of "idealism" in IR, was based on a positivist conception of law (Schmidt 1998b: 443-48; see also Schmidt 1998a: 151-87). With regard to international organization, Schmidt demonstrates that interwar scholars did not see it as a panacea for international ills based on the assumption of a natural harmony of interests. They did not believe the international anarchy could be easily transcended. While they supported the League of Nations, they were not uncritical of it. Their approach was essentially ameliorative and based on acceptance of the continued existence of plurality of self-interested states, and an awareness of the obstacles to international cooperation presented by national sovereignty (Schmidt 1998b: 449-52). For these reasons Schmidt concludes that the academic practice of IR during the interwar period cannot be construed as idealistic, and in line with Booth (1991, 1996), Long and Wilson (1995) and Little (1996), he contends that stigmatising the field in this way had damaging consequences for subsequent theorising. He also concludes that the sharp distinction conventionally drawn in IR between "idealist" and "realist" phases is false. The IR field of the late 1930s and 1940s was marked not by a clean break between one kind of theorising and another but by a shift of emphasis, away from international law and organization towards international politics and the dynamics of power.

Schmidt's method of analysis is "critical internal discursive history". He defines his subject as the "discursive practices of the early field of IR", which he conceives as "taking place within the institutional setting of the American discipline of political science" (Schmidt 1998b: 434). His object is to "reconstruct as accurately as possible the evolution of the discrete conversation of academic international relations" between those who "self-consciously and institutionally thought of themselves" of contributing to the professional study of international relations in its formative years. The sources he examines are journal articles, textbooks, professional conference papers, manuscripts, and other "discursive artefacts of the field's past" (Schmidt 1998b: 439; Schmidt 1998a: 1-14, 37-42). This is a legitimate way of circumscribing, sociologically and geographically, a potentially vast and daunting field of study. It is important to be aware, however, of three limitations. Firstly, one wonders whether it can be usefully extended to other contexts, particularly the geographical location of the most vibrant and extensive "conversation" about international relations during the interwar years, the UK. The UK conversation was a broad public one. There were insufficient academic posts, departments, and journals in the field of IR more formally defined to permit the kind of discrete discursive activity to which Schmidt alludes. In brief, IR was insufficiently institutionalised. Its infancy as a branch of specialist academic learning was perhaps even greater than Carr, one of its early professors, suggested (Carr 1939: 3-15). Until the 1950s, the formal IR component of (in Wight's sense) "international theory" was small. While journals such as *The Round Table* and *International Affairs* existed, from 1910 and 1922 respectively, it is significant that until the 1950s and

1960s the vast majority of contributors to them were not professional students of international relations—not in the sense of personnel who "self-consciously and institutionally thought of themselves" of contributing to the professional study of international relations. The first unequivocally IR journal in the UK was *Millennium: Journal of International Studies* which dates from 1971 followed by the *British Journal* (later *Review*) *of International Studies* in 1975. The professional association which established the latter journal, the British International Studies Association (BISA), was also founded in 1975—though it is important to note that this association has always conceived itself as multi-disciplinary, and the annual conference it has run since that time has always been conceived as a multi-disciplinary forum for debate about the subject. Before then the International Studies Conferences were held, initially under the auspices of the International Institute for International Cooperation, from 1928 to 1950 (see Long 2006), and the 'Bailey Conferences' of which 14 were held until they were superseded by the BISA Conference. The point of all this is that the field of IR is hard to define in the early British context. Schmidt's method of analysis makes more sense in the American context. The professional study of international relations in the US has from the outset "resided in, and been influenced by, the American discipline of political science" (Schmidt 1998b: 434). For the most part, American IR is a sub-field of Political Science. Chairs in political science in US universities date back to the 1850s and departments back to the 1880s. The American Political Science Association (APSA) was founded in 1903 with international law, imperialism, and sovereignty among its most important topics of discussion (Schmidt 1998b: 439-42). While the International Studies Association (ISA)

was not founded until 1959, the discursive artefacts pertaining to international relations accumulated within political science were by that time substantial. Most importantly, the institutional locale of nearly all the main contributors to intellectual debate about international relations in the US in the period of our concern was political science. But this does not encompass all the main contributors. The second limitation of Schmidt's method is that important contributors such as Addams, Lippmann and Niebuhr, by dint of there non-involvement in political science, are excluded from the picture. A third limitation is that there may be some artificiality in relating the history of a social subject such as IR purely in terms of its internal conversations, relationships and dynamics. External events have often played an important role in the generation of ideas, and the strengthening of one position *vis-à-vis* another (see Bell 2001: 120-21)—though Schmidt is right to distance himself from the straightforward cause and effect relationship sometimes posited.

One general point follows from these observations. The implicit or explicit sociological and geographical scope of the subject to be investigated (whether this is the *discipline* of IR or the *field* or something broader such as "international theory" or "international thought") has an important bearing on what is discovered and what conclusions are reached. Perhaps insufficient attention has thus far been given to this in the debate about the first great debate (and the intellectual/disciplinary history of IR more generally). Mixing these things up has certainly led to a lot of confusion (see e.g. the discussion of Olson and Groom (1991: 46-134) in Osiander 1989: 411, and Wilson 2003: 14-15).

WHIGGISH REALIST DISCIPLINARY HISTORY

Building on the work on Dryzek and Leonard (1988), Cameron Thies argues

that disciplinary history can never be neutral. It always involves a process of

legitimation and deligitimation. It also plays a large hand in identity

construction. In particular, those writing the history affirm their identity by

demonstrating how their perspective "is rationally superior to all competitors

past and present" (Thies 2002: 149). Disciplinary histories tend to be either

Whiggish or sceptical. Sceptics are dismissive towards the past, Whigs

condescending. The Whigs view the past from the perspective of the

concerns of the present. They ignore the contextual nature of the concerns of

the past, and view their approach as best by virtue of its ability to explain the

present. According to Thies, the story of the first great debate is a Whiggish

story told by realists (Thies 2002: 150; see also Ashworth 2006: 308). "[T]he

realists created a unified, nameable paradigm ('idealism') that they could use

as a straw man to demonstrate the utility and progressive nature of their own

approach, and provide a basis for their own disciplinary history" (Thies 2002:

173). The function of this history, involving the victory of "realism" over

"idealism" was to legitimate realism and deligitimate idealism.

There is much in Thies' account that adds to, or corroborates, existing

knowledge. He is right to assert that a "multiplicity of discourses" concerning

international relations ran through the inter-war period (Thies 2002: 154), and

that "idealism" did not exist in a unified form (Thies 2002: 171). He brings to

the surface the long-submerged ideas of *inter alia* Edwin Ginn (founder of the

World Peace Foundation and publisher of *International Organization*), Clyde

Eagleton, Ely Culbertson (federalist and pioneer of contract bridge), and

C.E.M. Joad. He convincingly shows the extent to which they were aware of

the obstacles in the path of their preferred vision of world order, and the

gradualism that they not only envisaged but recommended. In this sense they

were considerably more realistic than the self-styled realists were prepared to

concede.[3] He rightly points out that Osiander, rather than rejecting the idealist

paradigm as a realist invention, accepts its existence and reconstructs its

content. In doing this, however, Osiander mirrors realists by "repackaging the

past to fit the present" (Thies 2002: 154). As with the initial realist

construction, Osiander's reconstruction could be seen as an exercise in

presentist justification—an implicit or explicit attempt to strengthen a *neo-*

idealist research paradigm by rescuing the reputation of the idealists that

preceded it and tracing its roots back to the formative years of the field.

Finally, Thies' convincingly applies IR Dryzek and Leonard's insight that

"Whiggish historians" often point to a few "precursors" of their paradigm who

nearly "got it right" and co-opt them into their paradigm. He gives as examples

"utopian realists" or "liberal realists" such as Carr, Herz and Wright (Thies

2002: 152). In doing so he implicitly problematises Carr's realism, a move not

always adequately made in the revisionist literature (see e.g. Osiander 1998:

422-23).

There are, however, a number of problems with Thies' account. First, he

states that "[i]n order to understand the realist-'idealist' debate we must begin

with an excavation of the 'idealist' writings of the inter-war period" (Thies 2002: 152). But oddly no reference is made to the first systematic attempt to excavate these writings (Long and Wilson 1995). Instead, Thies relies on Quincy Wright's *The Study of International Relations*, published a full 40 years earlier. Had Thies consulted the Long and Wilson volume he would have avoided some errors. For example, his focus is on disciplinary history. He understands the first great debate as a disciplinary debate. Those inhabiting the (defeated) idealist paradigm are habitually referred to as "scholars". Yet if the notion of a first great debate between idealism and realism is to make any sense it has to be acknowledged that this was, as mentioned above, a broader public debate, certainly as far as Britain is concerned. Among its leading protagonists were Angell, a journalist turned professional writer and political publicist, and Woolf, a colonial civil servant turned publisher, writer and Labour party intellectual. Of course one does not need to be an academic to be a scholar, but Thies uses the terms interchangeably. While certainly intellectuals, Angell and Woolf among others were neither academics nor scholars (see Ceadel 2009). The object of their work was not knowledge-generation but public persuasion. Throughout his article, Thies frequently refers to his specimens of interwar thought as "scholars" (e.g. pp. 159, 161, 162).[4] But how many of them were in any meaningful sense scholars? Some biographical information of the more obscure or forgotten figures would have helped. But the point here is that Thies missed an opportunity to stress that his "diverse discourses" of inter-war thinking were conducted among a remarkably diverse group of interlocutors—publicists, peace campaigners, journalists, politicians, public servants, and the occasional academic.

Secondly Thies worries, methodologically, whether his "random sampling approach" to inter-war writing will lead to the exclusion of some mainstream scholars. He uses "mainstream" to refer to "those scholars from the past currently identified as representative of thought" during the period (Thies 2002: 153-4). The problem is that among the figures from the past currently identified as representative of the thought of the period are some notable non-scholars. Thies projects backwards disciplinary activity to a time when consciousness of such activity in a field understood as IR barely existed. He thus decontextualises the discourse under study when the very objective of this kind of historiographical work is to contextualise it more thoroughly.

Thirdly, even if we accept that the first great debate is a story told by realists, the idea that it is a Whiggish story seems to me to be based on a thin interpretation of what Butterfield originally had in mind (Butterfield 1965). It is true that Carr, Herz, Wolfers, Morgenthau and others saw utopianism/idealism/liberalism/moralism (as they variously conceived it) as flawed, and realism (or their particular version of it) as a step in the right direction. It is also true that they made little attempt to contextualise the ideas, beliefs, and assertions they were criticising, and that their primary concern was to establish the superiority of their own position, judged in terms of its ability to explain the present. But Butterfield's understanding of the Whig interpretation of history goes well beyond this, and there are some senses in which the application of this label to mid-twentieth century realism is singularly inapt. The richness of this understanding defies simple summary, but

essentially the Whig historian conceives his subject in terms of the progressive unfolding of some currently valued principle such as liberty. He traces a line back through "anticipations" of the present to its "roots" in the past, identifying along the way the friends and enemies of progress toward his ideal, the universal validity of which he does not stop to question (Butterfield 1965: 5-12). Historical data are extracted from their context in the service of an extraneous principle. In the process one of the most difficult historical jobs, the separation of what is important from what is unimportant in the countless mass of facts, is rendered simple. The Whig conception of history is linear when in fact, according to Butterfield, history is a labyrinth of multiple causes and multiple effects, a product of a complex series of interactions in which outcomes are not really outcomes but mediations—mediations between conflicting wills and forces that are rarely the direct product of intentions (Butterfield 1965: 44-5). In brief, the Whig historian generalizes, judges, abridges, simplifies, and abstracts. He (and in Butterfield the historian is always a "he") imposes a pattern on history according to some unquestioned abstract principle. In particular he "organises the whole course of centuries on what is really a directing principle of progress" (Butterfield 1965: 101; see also Schmidt 1998a: 16-21; Sylvest 2004: 411-14).

A lot more could be added to this summary of Butterfield's subtle and richly textured understanding of his "Whig interpretation". It serves to demonstrate, however, that to describe the realist story of the first great debate as a Whiggish story is tenuous at best. Firstly, Butterfield's understanding relates to the interpretation of centuries, to epochs, perhaps the whole of human

history, not to a parochial debate, spanning a small number of years, of minor significance in the grand historical scheme of things.[5] Secondly, one does not see in the realist story the progressive unfolding of some currently valued principle, but simply criticism of a group of writers on a variety of grounds. Thirdly, it might be true that realists simplified a complex ideational reality and took a short cut through this reality to arrive at their preferred position. But this might be seen as part and parcel of political discourse, particularly at times of extreme crisis. *A propos* the "idealists" themselves were not above such tactics. It would in a way be an insult to their intelligence to assume that they were (see Wilson 2000: 183-93). Fourthly, Butterfield's essay is a critique of a trend in the professional study of history. It seems odd to apply it to a group of non-historians (though Carr was later to become one) whose intention was not to write history at all, but to demonstrate the validity of a set of propositions concerning power, cooperation, peace, and international order. Finally, in terms of the accounts of, or assumptions about, the actual historical process one could argue that it was the "idealists," not the realists, who were the Whigs. It was they, and certainly not the realists, who tended to tell the story of modern international relations in terms of the gradual unfolding of progress—even if the subtlety with which they did this has tended, from Carr onwards, to be underestimated. That is why applying the Whig label to realists, even though one can understand what Thies is getting at, is ultimately inapposite. They were primarily *critics*, though not unqualified (see e.g. Cozette 2008; Scheuerman 2008), of the idea of history as progress. By applying the Whig label, Thies traps himself in a set of linguistic associations that leads to some highly inapposite assertions e.g. that the Whiggish realists

contended than "realism has proven more progressive than 'idealism'" (Thies 2002: 162). In certain respects an *advance* on "idealism" by all means, but "more progressive" carries with it a bundle of inappropriate connotations.

PERFUNCTORY TRADITIONALISM?

The central point of Joel Quirk and Darshan Vigneswaran's analysis is that the first great debate is a retrospective creation designed to serve certain meta-theoretical purposes. Its persistence is a result of "perfunctory traditionalism" (Quirk and Vigneswaran 2005: 107)—the tendency of the discipline to continually reproduce convenient orthodoxies. Their case is made in a broader historiographical analysis of how the story of the field in terms of a series of "great debates" took root. With regard to the first great debate they argue that, given what Wilson (1998) revealed regarding the "volley of responses" Carr's *The Twenty Years' Crisis* provoked, it would be inaccurate to conclude that this debate is entirely mythical. The first great debate, they contend, is a partially valid way of describing what took place in and around IR in the late 1930s and early 1940s. It is a half-truth and a caricature rather than a complete fiction (Quirk and Vigneswaran 2005: 91).[6] They accept that Carr played "a key role in popularising a divide between realism and utopianism" but argue that the primary source of the idea of a great debate between them is not located in the inter-war or wartime periods. In fact it is of much more recent origin.

Quirk and Vigneswaran's main contribution is to reveal how the notion of a great debate about American foreign policy morphed from the publication of Morgenthau's *In Defense of the National Interest* in 1951, through the writings of Tannenbaum, Niebuhr, Wolfers, Herz, Waldo, Wright, Fox and Kaplan into a "First Great IR Debate" between idealism and realism of the 1920s and 1930s. The principal architects of this half-truth, or semi-myth, were Holsti, Banks, Lapid and Maghroori in their surveys of the field of the 1970s and 1980s. In these surveys this debate was pushed back at least a decade from original references to it. Moreover, "idealism" and "realism" became coherent and sharply dichotomised schools or traditions of thought within the discipline of IR. The focus thus became narrower (the discipline of IR rather than wider public debate on foreign affairs), the temporal location shifted (from the 1950s to the 1920s and 30s), and the categories hardened (losing the qualifications and doubts about an idealist-realist dichotomy expressed by among others Wright, Waldo, and Fox) (Quirk and Vigneswaran 2005: 99-103; see also Ashwoth 1999: 126-9). The notion of a series of great debates thereby entered into disciplinary folklore by virtue of a series of brief accounts the main concern of which was not historical accuracy but the "situation and validation certain metatheoretical contests" (e.g. science versus tradition, problem-solving theory versus critical theory, positivism versus post-positivism) (Quirk and Vigneswaran 2005: 103). Quirk and Vigneswaran thus show that while realists had a hand in the construction of the notion of a great interwar IR debate between idealism and realism in which the latter were victorious, the construction of this "edifice" was far from a realist conspiracy (thus corroborating Wilson 1998: 14-15). They show how intellectual

exchanges of the 1920s-1940s became detached from their historical and intellectual moorings and were represented, misleadingly, as an "analogue of later contests" (Quirk and Vigneswaran 2005: 94). While, for example, the contributions to "second" and "third" great debates could be assessed by reference to works and ideas internal to the IR discipline, such a framework was inappropriate when it came to earlier "scholarship."

There is a problem, however, with this formulation. Much of this "scholarship," as I have argued, was not scholarship in any meaningful sense. Akin to Thies, Quirk and Vigneswaran get themselves caught in a linguistic trap, in this instance by using a term denoting the very academic/disciplinary dialogue from which they want to break free. As with Thies they refer to Angell as a "scholar"—consolidating again by their choice of words the very image they are trying to unsettle. In no shape or form, as we have seen, was Angell a scholar. They also refer to the first debate in terms of "scholars attacking their colleagues' works" (Quirk and Vigneswaran 2005: 104-5)—again cementing the very image of a *disciplinary* debate that the substance of their article seeks to erode. They are right to say that to properly comprehend the international thought of the 1920-1940s a wider socio-political and foreign policy context needs to be invoked (thus echoing e.g. Long 1995; Wilson 1996, 2003; Sylvest 2004); and that there are dangers in seeking to conduct disciplinary historiography in terms of an exclusively internalist approach (e.g. Schmidt's critical internal discursive history), particularly when it comes to the inter-war period (Quirk and Vigneswaran 2005: 94). But given their understanding of inter-war debates about international relations as not for the

most part *disciplinary* debates, it seems odd that that they maintain as their object of study *disciplinary* history. It should logically be something broader, such as "history of international thought." To a degree, therefore, they seem to have slipped into the very "perfunctory traditionalism" they so skilfully expose. Like Thies, by the vocabulary they use they project backwards a disciplinary consciousness to a period when this consciousness was at best embryonic—certainly as far as the UK is concerned.

THE REALISM OF IDEALISM?

In Ashworth's view what is striking, when revisiting works on international affairs in the inter-war period, is not an "idealist-realist" debate, but a debate about: "does capitalism cause war?": appeasement versus collective security; and (in the US) intervention versus abstention. He finds little evidence that a realist-idealist debate "occurred at all in the form in which modern IR writers suppose" (Ashworth 2002: 34-5). There was, for example, virtually no discussion of the relative merits of an "idealist" versus a "realist" approach in any of the IR and political science journals of the time. In Ashworth's view the "idealist-realist dichotomy" is a post-WWII realist construction (Ashworth 2002: 48; see also Ashworth 1999: 106-29).

One of Ashworth's contributions is to correct a major misunderstanding in the IR literature: that it was the "idealists" who were the appeasers not the realists. He shows that the latter, because of their respect for power and their greater inclination towards pragmatism, were the more inclined towards

appeasing the dictators—though Morgenthau's stance towards them toughened over time and his close intellectual ally, Niebuhr, was highly critical of the Munich agreement (Ashworth 2002: 42; Ashworth 1999: 118). The "idealists," i.e. those who supported the League and collective security, were early (from at least 1931) objectors to the means and ends of the fascist Powers. It is their uncompromising stand against fascism, compared with the more pragmatic stance of, say, Carr (see e.g. Haslam 1999: 57-80; Wilson 2000: 184-5), that has proved over time to have been the more realistic. There is, however, one problem with Ashworth's analysis of appeasement that is worth pointing out. While he acknowledges that some critics of appeasement consistently opposed increased expenditure on armaments and offered alternatives that were far from cogent, and thus cannot escape some culpability for the sorry state that followed, he fails to consider the responsibility of internationalists such as Angell, Zimmern, Murray and Woolf in laying the psychological conditions of appeasement in the 1920s. Here I am thinking of their anti-war rhetoric, the euphemisms they employed for war (e.g. 'enforcement', 'sanctions', 'common action against aggression'), and their failure to spell out in relatively good times the true cost in terms of blood and money that a functioning system of collective security would entail (see e.g. Wilson 2003: 68-73; Sylvest 2004: 420-23).

Another contribution Ashworth makes is to show how the terms idealism and realism entered into public debate on foreign policy in the late 1930s. Those advocating collective security through the League were increasingly branded "idealists" by those arguing for a return to a traditional (and for them more

realistic) approach to security involving military alliances, secret diplomacy and the balance of power (Ashworth 2006: 293-8). But according to Ashworth this "name-calling" (and it might be added, "branding") in public debate did not reach the level of academic discourse until Carr's use of "realism" in 1939. Carr, and later Morgenthau, took a label used in popular debate and "gave it a metaphysics" (Ashworth 2002: 46-7; Ashworth 1999: 125). Under the name "realism," that is, they took a well established essentially nineteenth century nationalist outlook on foreign policy and constructed a more elaborate general theory around it. It can be questioned, however, whether this description applies to Carr as much as it does to Morgenthau. My main criticism, indeed, of Ashworth's contribution concerns not so much his representation of "idealism," but his representation of realism. In his desire for restorative justice he arguably takes too much away from the realists, Carr in particular. He states, for example, that Carr and Morgenthau's reasons for opposing a liberal/utopian view of the world coincide (Ashworth 1999: 111; Ashworth 2002: 35). Apart from at a basic level e.g. they both felt that the liberals/idealists/utopians underestimated the factor of power, I am not sure this is true. Carr's opposition was based on a view of history; Morgenthau's on a view of Man. For Carr utopianism was the ideology of a self-interested class designed to perpetuate an economically obsolescent social and political order. For Morgenthau 'idealism' was a political outlook, well intentioned but dangerously blind to the fearful and power-seeking nature of Man. Carr's rejection is rooted in historical materialism, Morgenthau's in pre-rationalist political philosophy (see Murray 1997: 47-69). I am not sure it can be maintained that Carr, like Morgenthau, believed in certain inescapable "laws

of behaviour … established by human nature" (Ashworth 2002: 42); or like

Morgenthau and Wight that he believed in "the universality of the balance of

power, and … [the] cyclical recurrence of history" (Ashworth 2002: 42). In

fairness Ashworth recognises that Carr and Morgenthau "differ on crucial

points" (Ashworth 2002: 35), and that there is an important progressive

dimension to Carr's thinking (Ashworth 2006: 299; cf. Navon 2001: 612), But

generally he glosses over the diversity of realism, paying insufficient attention

to the radical aspects of Carr, and the extent to which Carr's essentially

historicist argument against the "utopians" differed from Morgenthau's

essentially deontological argument (Molloy 2006: 15-34). It is also true that

Ashworth acknowledges that Carr's realism was in some ways hedged. He

says, for example, that he "moved beyond his realist critique" with the

publication of *Nationalism and After* in 1945 (Ashworth 2002: 38). But this

movement began in fact as early as Chapter 6 of his 14 chapter *The Twenty*

Years' Crisis. This is one of the factors that makes Carr a problematic realist

according to all recent understandings of that term in IR (see e.g. Wilson

2001; Molloy 2006: 51-74). Ashworth succumbs to the temptation of the

idealists themselves when confronting Carr's multifaceted thesis—that of

trying to squeeze it into an ideologically convenient vessel.

Yet there are some aspects of Ashworth's representation of "idealism," along

with that of realism, that require modification. It could be argued that in

correcting some of the misrepresentations of "idealism," and seeking to

restore its reputation, Ashworth overlooks some of its significant

shortcomings. Taking one of the three charges Carr levelled at

idealism/utopianism that I identified in my book on Woolf, Ashworth disputes

that utopians "pay little attention to facts and analysis of cause and effect,

devoting their energies instead to the elaboration of visionary projects for the

attainment of ends which they have in view" (Wilson 2003: 20). According to

Ashworth, many so called utopians during the interwar period paid much

attention to facts and they were deeply concerned with cause and effect. They

also spent little time elaborating visionary projects. The charge is therefore

without foundation (Ashworth 2006: 301). It is more accurate to say, however,

not that the charge is without foundation but that it is an exaggeration. It is

true that they rarely put forward blueprints for change, though there are

exceptions even among the five figures (Angell, Woolf, Noel-Baker, Brailsford,

and Mitrany) to whom Ashworth gives his attention (e.g. Woolf's 1944 *The*

International Post-War Settlement). To the extent they can be considered

utopians they are "process" rather than "end-point" utopians (Booth 1991). It

is also true that they produced works that were empirically detailed, and

contained considerable research, e.g. Woolf, *Empire and Commerce in Africa*

(1919) and Noel-Baker, *The Private Manufacture of Armaments* (1925). But it

is important to note that even these highly factual books were not written in

the spirit of detached, scientific enquiry. The huge amount of empirical

material contained within their pages was gathered to confirm conclusions

already reached, namely that the primary motive for European imperialism in

Africa was economic exploitation, and private arms manufacturers had a

vested interest in war and were actively engaged in bringing it about. These

authors were no doubt interested in relations of cause and effect but not in the

rigorous and detached exploration of them. They were first and foremost

advocates not analysts. While they were engaged in research they were not scholars.

A further charge of Carr is that utopians failed to appreciate the self interested character of their thought, and in particular that their espousal of universal interests amounted to nothing more than the promotion and defence of a particular status quo (see Wilson 2003: 20). According to Ashworth one part of this charge can be easily dismissed. Rather than promoting and defending the status quo his five "idealist" thinkers were "deeply critical" of it (Ashworth 2006: 303). They were all in favour of radical changes to the way international affairs were conducted. This is true but only up to a point. So many interwar progressives were like shop owners cognisant of the fact that an overhaul of the business, perhaps radical, was long overdue but at no time willing to contemplate a change in ownership. Firstly, few of them questioned (Toynbee is an exception here) the continued hegemony of the West and more narrowly the Anglosphere (as it is now called) in world politics. Indeed, much of what they said was designed, consciously or consciously, to keep Britain in the front line of nations—to maintain, that is, the influence of Britain, its ideas, values and institutions, in a world the centre of gravity of which was shifting. This applies to Carr's brand of internationalism as much does to those of his liberal critics. Secondly, all of these men, even those critical of colonialism, possessed an imperial mindset. They believed that what they said mattered, and that the opinions of forward thinking British intellectuals such as themselves represented the most advanced thinking in the world. At no time did it occur to them that their prescriptions for world order, whether functional

or federal, might be an expression of hegemonic decline, and that the eminent good sense they exhibited might not appear quite so eminent to Powers on the rise. Thirdly, all of these figures argued for international government in one form or another. But is international government the ideology of declining power? It is at least a valid question that no one until Carr thought to ask. So, while I agree with Ashworth that many "idealists" were unhappy with the *status quo* and argued for extensive international reform, their position was compromised by a pronounced ethno- and for the most part anglo-centricism. They too often failed to enquire whether their common sense about the peace, or cooperation, or security, or trade, or Germany, or the League, or sanctions, was a sense common in other parts of the world (see e.g. Wilson 2011: 901-9). While I agree that it is important to rescue "idealism"/liberal internationalism from the gross distortions and misrepresentations of the past, we should not do this at the cost of glossing over its quite considerable shortcomings.

A final point on Ashworth's contribution it is important to mention is that he does not, in common with other contributors to the debate, separate carefully enough disciplinary history from the broader history of international thought. With regard to many of his observations, Ashworth moves rather casually from 'interwar IR', 'British IR', 'English-speaking IR', and 'the work of IR scholars' (and similar formulations), to the more general 'interwar literature' and 'writers on international affairs in Britain' (Ashworth 2006: 292, 294, 298, 300, 305; see also Ashworth 2002: 24, 34-6). The problem is highlighted in the conclusion, entitled 'Understanding interwar IR', to his main article

(Ashworth 2006: 305-8). In this conclusion Ashworth makes many valuable points about how to think about debates in Britain about war and peace and related matters during the interwar years. But he presents this as an analysis, or revisionist history, of the IR discipline. He mentions specifically eleven contributors to these debates, but of these only two, Laski and Carr, were academics (Carr post-1936), and only one (Carr) an IR academic (pre-1946). Noel-Baker, it is true, was an ex-IR academic, and Mitrany a quasi-academic (moving for the most part in policy/think tank circles). But the point here is that Ashworth's history is only tangentially *disciplinary* history. It is primarily a broader history of international thought.[7]

CONCLUSION

In conclusion, I would like to make three general observations about what we know now as a result of the research of the last decade or so, and what we can do better to enhance our research findings in the future. First, several important things about the early history of the field are now well established. We know that "idealism/utopianism" did not exist as a distinct school of thought during the inter-war period. No group of thinkers ever identified with this label. Many thinkers of the period had ideals and believed that their achievement was to some degree achievable. "A logic of the realizable ideal," as Sylvest (2004: 427) has nicely put it, informed many of their arguments. But these ideals, except in a very broad sense e.g. "peace," varied considerably, as did the methods and processes posited for their achievement. This being said, we know that the idea of a first great debate is

not a complete fiction. There were lively exchanges on international matters throughout the 1920s and 1930s. These primarily concerned specific issues of foreign policy, but they also involved wider and deeper questions about international peace, order, justice, cooperation and conflict. Liberal internationalists of various kinds argued against conservatives, some of whom saw themselves as "realists," and socialists of various kinds argued against both. But there was never a time when two well defined ideological positions did battle. This idea is a *post hoc* creation of the 1970s and 1980s. While the idea of a first great debate has a toe-hold in reality it is best understood as a retrospective invention the function of which is not historical veracity but the legitimation of certain contemporary theories and approaches (see Bell 2003: 154; Bell 2001: 120; Bell 2009: 4-9). From the point of view of historical accuracy we need in the future to be much more wary, to paraphrase Sylvest (2009: 13), of unreflectively projecting current academic concerns, boundaries, and debates back on the people or period under scrutiny.

Secondly, greater sensitivity is required to the differences between the two principal intellectual contexts within which debate of a broad theoretical kind took place about international relations during the inter-war period. In the US, exchanges took place inside and outside Political Science, but there is no doubt that Political Science was host to, and provided an institutional focal point for, substantive theoretical debate to a degree that far exceeded the UK. In the UK the debate was a broader social and political debate. While several prominent professional students of IR were involved, it took place in a wide variety of institutional settings and through a wide variety of media. While IR

was making big strides it is important to register that it was tangential to the substantive debates on international matters that took place in the UK during this period. This is why we need to exercise more caution before labelling these debates "IR debates." More focus is needed on whether our subject is the discipline/professional field of IR or international theory/international political thought more broadly. If both, then more care needs to be taken not to mistake one for the other or unwittingly elide them. Most of us involved in reappraising early-mid twentieth century international thought have fallen prey to this error of mixing up disciplinary with wider discourses.

Finally, in amending the misrepresentation of "idealism" we need to be careful not to misrepresent "realism". As international theorists (in the Wightian sense) and historians of the field we have to be mindful of not substituting a caricature of realism for the one of idealism that has done so much damage not only to historical understanding but more generally to imaginative and productive thinking about our subject.

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[1] It is not entirely clear what Osiander means here. There are four possibilities: 1) IR arose as a reaction to the terrible experiences of WW1 and the passionate desire to prevent its recurrence; 2) that the early years of IR were dominated by a coherent idealist school of thought; 3) that realism demonstrated that the premises of idealism were fundamentally flawed; 4) two or three of the above.

[2] Osiander's assertion that Carr's style of argument 'is no more rigorous than that of those whom he criticised' (Osiander 1998: 429) seems to me wide of the mark. Although the flaws in Carr's analysis in *The Twenty Years' Crisis* and elsewhere are

now well known (see e.g. Jones 1998; Cox 2000), he brought to bear a level of scientific detachment and analytical depth hitherto unprecedented in IR.

[3] Thies fails to identify, however, the domestic analogy that informs so much of their work, perhaps unaware of Suganami's important 1989 study.

[4] A mistake fallen into by even as meticulous a scholar as Sylvest. While noting that the 'distinction between academia and politics … is hard to discern in interwar debates on international politics', Sylvest (2004: 410-13) makes the distinction even harder to discern by repeatedly referring to Woolf and Angell as 'scholars'.

[5] I do not wish to imply here, of course, that parochial things cannot be important nor that the debate about foreign policy and international relations in the 1930s and 1940s is not important in a narrower context.

[6] Though they confusingly later describe it more uncompromisingly as a 'myth' (pp.105, 107) and an 'egregious misrepresentation' (p. 105).

[7] In his earlier work he makes his case regarding the 'folklore' of an inter-war IR idealist-realist great debate with reference to principally two figures, Angell and Woolf, neither of whom were scholars or academics. Only one of the figures regularly cited, Zimmern, was an IR academic (Ashworth 1999: 106-29). Despite this all the generalizations made refer to the *discipline* of IR (see esp. pp. 107-8) with only one reference to something broader and more accurate ('the contemporary international affairs literature' p. 126).