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Whatever Happened to Gramsci? Some Reflections on New Legal Realism

JAN KLABBERS*

Abstract
This article responds to five pieces published in the previous issue of the *Leiden Journal of International Law*, extolling the virtues of a new legal realism. I first express some doubts as to whether an awful lot can and should be expected from yet another new approach to the study of international law; earlier approaches widely heralded have quietly disappeared from sight, sometimes without leaving much trace. Subsequently, I discuss the extent to which the new international legal realism conceptualizes its notion of empirical reality, followed by a discussion on which interests the new insights spawned by new legal realism are to serve.

Key words
European Legal Realism; incidents; International Legal Realism; le realism; new legal Realism

There was a time, at some point in the 1980s, when Antonio Gramsci was the newest new thing in social science faculties. Expectations were high: the Gramscian or neo-Gramscian perspective would help us understand the political world around us including, so it was presumed, the world of international affairs. Thirty years on, Gramsci has all but disappeared. While Gramsci inspired some excellent and insightful work on international organizations[1] and international relations generally[2], and some continue to work in the Gramscian tradition, the world of scholarship has moved on; Gramsci has been replaced by new heroes who, in turn, have already been replaced a few times themselves. New methods have been devised, new theories have been launched, new approaches have been heralded, and most of them have come to rest in the graveyard of academic fashions.

If Gramsci's is a cautionary tale, so is the short-lived existence of the 'incidents approach'. In the mid-1980s, international lawyers Michael Reisman and Andrew Willard launched a seemingly new approach to studying international law: the incidents approach. They first produced a special issue of the *Yale Journal of International Law*, comprising essays by students, some of whom have gone on to become prominent international lawyers.[3] A book was eventually published in 1988, carrying the promising subtitle 'The Law That Counts in World Politics'.[4] In effect, the incidents

* Academy Professor (Martti Ahtisaari Chair), University of Helsinki [jan.klabbers@helsinki.fi].
1 See C. N. Murphy, *International Organization and Industrial Change: Global Governance Since 1850* (1994).
2 See R. W. Cox, *Production, Power and World Order: Social Forces in the Making of History* (1987).
3 See (1984) 10 *Yale Journal of International Law* 1.
4 See W. M. Reisman and A. R. Willard (eds.), *International Incidents: The Law That Counts in World Politics* (1988).

approach espoused by Reisman and Willard was a methodologically simplified version of the New Haven approach developed by McDougal and Lasswell and, indeed, Reisman himself: simpler to apply, a bit more inductive perhaps, liberated from much of the jargon that had crept in, and yet sensitive to the political and social context in which international law was to operate. Reisman himself had high hopes, so it seemed: 'It is to be hoped … that volumes will follow on a regular basis, setting out current incidents'.[5] And he called on the many student-run journals in the US to devote a section to the genre of incidents.[6]

The initial reception was mixed. The *Yale Journal* had asked two international lawyers to provide comments. Richard Falk, himself educated at Yale, displayed a measured enthusiasm.[7] Derek Bowett, by contrast, steeped in the English tradition, was more critical.[8] Moreover, the same issue of the Journal that published the two commentaries started an 'incidents rubric' along the lines Reisman had suggested, and kicked off with a paper by Edward Kwakwa.[9] This, however, proved to be the incidents approach's last gasp of breath: while the book was reviewed with enthusiasm bordering on hyperbole by Francis Boyle, welcoming it as 'a very important contribution' and providing international lawyers with 'a new lease on life',[10] the incidents approach failed to establish itself: the book, the two commentaries, and Kwakwa's article proved to be its total output.[11] Partly this stemmed, no doubt, as Bowett perceptively observed, from the fact that international lawyers have always studied incidents and will always continue to do so, and partly from the fact that much international law often manifests itself in mundane, banal, everyday ways, far removed from incidents, accidents, and events.

The incidents approach then disappeared about as quickly as it had arrived, and one important factor contributing to its rapid disappearance was that it was quickly overshadowed by two other approaches. Amongst international lawyers, the critical approaches endorsed in David Kennedy's *International Legal Structures* and especially Martti Koskenniemi's *From Apology to Utopia*, sandwiching the incidents book in terms of timing[12], quickly came to dominate discussion and debate, and themselves inspired further offspring approaches. Emboldened by the success of the critical approach, the arrival of New Approaches to International Law (NAIL) and the 'newstream' was proclaimed, with a large and increasing role for the Third

5 See W. M. Reisman, 'International Incidents: Introduction to a New Genre in the Study of International Law', in W. M. Reisman and A. R. Willard (eds.), *International Incidents: The Law That Counts in World Politics* (1988) 3, at 23.

6 Ibid., at 23–24.

7 See R. A. Falk, 'The Validity of the Incidents Genre', (1987) 12 *Yale Journal of International Law* 376.

8 See D. W. Bowett, 'International Incidents: New Genre or New Delusion?', (1987) 12 *Yale Journal of International Law* 386.

9 See E. Kwakwa, 'South Africa's May 1986 Military Incursions Into Neighboring African States', (1987) 12 *Yale Journal of International Law* 421.

10 See F. A. Boyle, reviewing W. M. Reisman and A. R. Willard (eds), *International Incidents: The Law That Counts in World Politics*, (1989) 83 AJIL 403, at 403 and 406, respectively.

11 Faint echoes can be heard from time to time though. One illustration is G. Guillaume, *Les grandes crises internationales et le droit* (1994); another is the more ambitious 'events approach' launched a few years ago in F. Johns, R. Joyce, and S. Pahuja (eds.), *Events: The Force of International Law* (2011).

12 See D. Kennedy, *International Legal Structures* (1987) and M. Koskenniemi, *From Apology to Utopia: The Structure of International Legal Argument* (1989).

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World Approaches to International Law (TWAIL). By now, the critical approach has firmly established itself, so much so that critical work is academically taken far more seriously than doctrinal work or normative work – the 'newstream' has become the mainstream, if not politically then at least academically. Within the academy, critical scholarship is usually taken far more seriously than doctrinal scholarship.

Part of the agenda of the incidents approach was to bring international law and international relations scholarship closer together (on the somewhat dubious theory that decision-makers would pay more attention to legal advice if it were more in tune with political reality), but across the disciplinary divide a revolution of its own was staged with the publication of two major constructivist works, Nick Onuf's *World of Our Making* and especially Fritz Kratochwil's *Rules, Norms and Decisions*. Both works were first published in 1989,[13] the same year as *From Apology to Utopia* and only just after the publication of Reisman and Willard's volume.

The point of the above is not to dissect or condemn the incidents approach, Gramscianism, or any other approach for that matter, but rather to serve as a melancholic word of warning: new approaches, new methods, new genres, they come and they sometimes also go away again, or bubble under the surface only to resurface in a different guise as yet another approach emerging later. This, arguably, was the fate of the legal realism of the 1930s, which lost a bit of traction over the years but served to inspire critical legal studies and, now, the emergence of a new legal realism, as the five papers assembled in the previous issue of the *Leiden Journal of International Law* testify.

Those five papers hang together more in the abstract than in a concrete sense. Two can be read as manifestos for a new legal realism based on the old US-based legal realism and advocating empirically-orientated scholarship; this applies to the papers by Shaffer[14] and Huneeus.[15] A third is also a manifesto of sorts, but for something else: Holtermann and Madsen advocate an approach they dub New European Legal Realism, but to the extent that it is empirically-orientated, it advocates a rather different kind of empirical work.[16] Daniel Bodansky's article is perhaps best seen as placing the new legal realism in a broader perspective,[17] while Andrew Lang's contribution applies new legal realism (itself deemed reflexive) reflexively, so to speak: he uses it as a tool to investigate how others, both in scholarship and in the world trade regime, are using it.[18]

Any healthy academic discipline will see its fair share of methodological suggestions presented and will come to engage in its fair share of methodological quibbles

13 See N. G. Onuf, *World of Our Making: Rule and Rule in Social Theory and International Relations* (1989); F. V. Kratochwil, *Rules, Norms and Decisions: On the Conditions of Practical and Legal Reasoning in International Relations and Domestic Affairs* (1989).

14 See G. Shaffer, 'The New Legal Realist Approach to International Law', (2015) 28 LJIL 189.

15 See A. Huneeus, 'Human Rights between Jurisprudence and Social Science', (2015) 28 LJIL 255.

16 See J. v. H. Holtermann and M. R. Madsen, 'European New Legal Realism and International Law: How to Make International Law Intelligible', (2015) 28 LJIL 211.

17 See D. Bodansky, 'Legal Realism and Its Discontents', (2015) 28 LJIL 267.

18 See A. Lang, 'New Legal Realism, Empiricism and Scientism: The Relative Objectivity of Law and Social Science', (2015) 28 LJIL 231.

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and bickering.[19] Over the last thirty years or so, students of international affairs have not only encountered constructivism, the incidents approach, critical legal studies, and NAIL and TWAIL, but have been offered a veritable smorgasbord of contending approaches. A Marxist approach to international law has been endorsed, next to Global Administrative Law. A feminist approach exists side by side with the empiricist turn. International law has been said to be best studied from a constitutionalist angle or at least as public law, while others have, by contrast, proposed a managerial approach, and for a few intense years everyone and their uncle was engaged in regime theory. Brunnée and Toope recently provided an interactional approach[20], whereas the approach most quickly gaining ground across the Atlantic is a loosely law and economics-derived type of approach. From a greater distance, we have been urged to take notice of Lacan, Foucault, Derrida, Habermas, Badiou, Bourdieu, and indeed Gramsci, and probably dozens of others too. Even Althusser, unceremoniously dismissed by late-Marxist political scientists, has been discovered by a new generation of international lawyers on the left.[21] And to top it all, many have been the pleas for interdisciplinary work or at least for using techniques and methods borrowed from other social sciences and the humanities, including the heralded historiographical turn in international law. Students of international law and international politics are often urged to produce interdisciplinary work, usually along rationalist lines,[22] sometimes also more subtly along broadly constructivist lines.[23] The irony is, it would seem, that all of these approaches are eminently capable of producing both brilliant insights and excruciatingly poor scholarship. No matter what method is used, international legal scholarship tends to come in three forms: good, bad, and indifferent (the latter is, fortunately perhaps, by far the largest category).

I have both a lot of sympathy for the new legal realism discussed in the previous issue of this Journal, and rather low expectations. Gregory Shaffer suggests that one of the hallmarks of the new legal realism is a reluctance to accept anything as dogmatic: empirical results need constantly to be confirmed and re-confirmed. This is an appealing pragmatism owing much to common sense, as is the more philosophically minded pragmatism of Dewey and James in its insistence on starting analysis by asking questions that have some bearing on the real world. There is a caveat here though, or a nuance perhaps: the real world can be a very different place depending on whether one looks at it through, say, Beth Simmons' eyes, or those of Michel Foucault. Both can be said to look at the real world; both can be said to do (or have done) empirical work, and very good empirical work, and yet their worlds are, quite literally, worlds apart.

19 International lawyers have generally not done so very overtly: few books are devoted to methodological issues, and even then, not always in compelling fashion. While O. Corten, *Méthodologie du droit international public* (2009) is indeed a guide on methodological issues involved in research, M. Bos, *A Methodology of International Law* (1984) is best seen not as methodology but as espousing a theory of sources.

20 See J. Brunnée and S. J. Toope, *Legitimacy and Legality in International Law: An Interactional Account* (2010).

21 See, e.g., F. Johns, *Non-legality in International Law: Unruly Law* (2013).

22 See the hefty volume by J. L. Dunoff and M. A. Pollack (eds.), *Interdisciplinary Perspectives on International Law and International Relations: The State of the Art* (2013).

23 See F. V. Kratochwil, *The Status of Law in World Society: Meditations on the Role and Rule of Law* (2014).

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II

On a positivist notion of science (assuming that law fits that label), new methods or approaches replace the old ones in order to come to an improved understanding of the world around us. This no doubt also plays a role in the study of international law and international politics: new genres, new labels, new approaches, are often thought to deliver, actually or potentially, better, more robust or more compelling explanations of what goes on around us.

Still, descriptions and understandings come with blind spots: they will highlight some facts and circumstances, while obscuring others. Sometimes this process takes place in bad faith, when people are so keen to espouse their world-view that unsupportive facts are wilfully ignored or defined away. Usually, however, it follows from a chosen method's particular premises or structure: game theory, for instance, typically starts out by studying situations involving two players of equal standing. The setting can then be expanded so as to include reiterative games, and even a multitude of players of equal standing, but it would be difficult to devise a rigorous game theoretical model which could include players of different standing. It is in all likelihood no coincidence that a game theoretical account of international law which would accommodate the role played by international organizations, non-state actors such as Amnesty International or the Catholic Church, and moral entrepreneurs and political leaders, has yet to be developed.[24] Such a model would look more like the real world, but would be less powerful: what it gains in representation, it loses in parsimony. The result is that studies using game theoretical models are able to generate intriguing and perhaps even compelling hypotheses, but these often remain to be tested.

What applies to game theory also applies to other approaches beyond the purely doctrinal study of international law: law and economics inspired scholarship, critical legal studies, and others are all capable of opening vistas that would otherwise remain closed. That makes them exciting, but not yet compelling: they still need to be tested. Often enough, however, the testing is not engaged in: it is both easier and sexier to engage in abstract modelling. Some law and economics scholars make a habit of churning out dozens of scholarly articles per year precisely because they focus on analytical outcomes rather than empirical verification. And much the same applies to critical legal scholarship: it is easy with a few strokes of the pen to dismiss legal rules as indeterminate; it involves a lot of hard work though to demonstrate with any given rule that it is also experienced as indeterminate by those who work with it. Koskenniemi did the hard work in his *From Apology to Utopia* with respect

24 This, incidentally, is what I had in mind when I wrote a decade ago that using game theory for analysing the law of treaties could only entrench state interests: I was not pointing out that there is something problematic about knowing that treaties involve state interests, as Bodansky's somewhat hasty rendition suggests, but rather that a game-theoretical analysis will typically only focus on treaties between states (ignoring other actors because they do not fit the model and therewith strengthen the position of states vis-à-vis those other actors), and only on contractual treaties, therewith ignoring or undermining law-making treaties because they do not fit the model. See J. Klabbers, 'The Relative Autonomy of International Law or The Forgotten Politics of Interdisciplinarity', (2004–2005) 1 *Journal of International Law and International Relations* 35, at 39–40.

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to sovereignty and sources; Kennedy likewise in *International Legal Structures* with respect to jurisdiction and other topics, and others have done so with respect to yet other topics.[25] But Kennedy surely had a point when he expressed some dismay at the circumstance that the main hypotheses of the critical movement are rarely further investigated.[26]

Methodological debates involve world-views and political opinions: they are exercises in power. This is by no means a novel observation but it is useful to remember this, also when endorsing one's own preferred approach: remembering that method is power ideally induces some humility in scholars. Scholarship, one might say, demands a virtuous approach to method, to reading and writing and thinking, regardless of the particular method.[27] With this in mind, the tendency of law and economics scholars to preach to their own parish, and the tendency of the critical legal studies movement to organize meetings and conferences only open to other critical scholars, strikes as regrettable. Such moves may be understandable in terms of institution-building and creating or maintaining distinct schools of thought; they may even be welcomed in order to inspire younger scholars and provide them with an academic embrace, but in the long run they may spiral out of control. As Teubner reminds us (arguably in a different context), every self-referential system runs the risk of spinning out of control and biting its own tail; every system needs to be open to outside interference every now and then, and needs to expose itself to what he refers to as 'constitutional irritants'.[28]

III

Any approach priding itself on its empirical focus must find some way to come to terms with what it views as 'the empirical'. Strikingly though, the five articles contain very little on what 'empirical' stands for, despite the proclaimed empirical orientation of the new legal realists.[29] The empirical, after all, is not an innocent, self-evident notion. For one thing, facts are typically embedded in chains of events: in the social world, things rarely happen just out of the blue. Rough empirical material somehow and inevitably will be filtered: out of the chain of events, which event or fact or set of facts will be focused on, and why? Second, while it is one thing to say that facts are just 'out there', in the social world they only acquire meaning through interpretation, and interpretation as such will be based on underlying presumptions and assumptions. Moreover, often even the vocabulary to describe the facts in non-evaluative terms is absent. As good an example as any is trying to describe the edifice constructed by Israel on its territory and subject to discussion by the International

25 See, e.g., Heiskanen's excellent dissection of the monism-dualism discussion in V. Heiskanen, *International Legal Topics* (1992).

26 In a piece dedicated to the reissuing of Koskenniemi's *From Apology to Utopia*, Kennedy writes that the book is 'rarely challenged or deeply engaged', and that 'it has been tempting to treat the book as a given, a rock to be digested or maneuvered around, rather than a provocation to engage or revise'. See D. Kennedy, 'The Last Treatise, Project and Person', (2006) 7 *German Law Journal* 982, at 991.

27 See, e.g., E. Sosa, *A Virtue Epistemology: Apt Belief and Reflective Knowledge* (2007).

28 See G. Teubner, *Constitutional Fragments: Societal Constitutionalism and Globalization* (2012).

29 I am trying to resist the urge to capitalize the term, as capitalization often implies reification.

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Court of Justice a decade ago: calling it a 'wall' will release all sorts of connotations, while calling it a 'fence' will stem from and evoke a different set of connotations.[30]

And then there is the problem of aggregation. A court may often be asked to pronounce on a single fact or closely related small set of facts, but empirically-orientated legal scholarship will, in all likelihood, be more interested in aggregated facts: not in single cases, but in patterns on a national or international scale. This requires that data are brought together, and doing so comes with at least two potential issues. One is that facts tend to become 'flattened' when aggregated. Aggregation requires measurement, and measurement often entails making broad categories which will encompass several distinct kinds of facts under the heading of a single kind. The act of drawing up the categories, then, assumes vital importance: if the researcher wishes to know how often something occurs, he or she will have to classify which acts count, and will have to count them in disregard of any particular circumstances. And in such a case, empirical work inevitably ends up including some facts and excluding others.

The second problematic issue with aggregation is one of representation: how to frame the aggregate data? The problem is inherent in Disraeli's famous condemnation of 'lies, damned lies, and statistics': numbers can represent all sorts of things, and again, meaning often depends on context. Huneeus' paper unwittingly provides a manifestation of the problem when she writes that while the Inter-American Court orders states to punish perpetrators in 'roughly 75% of its judgments, only one has ever received compliance'. In doing so she mixes absolute and relative numbers in an uncomfortable way: the single case of compliance looks dismal if the Court has issued hundreds of judgments, but does not look nearly as bad, all things considered, if the Court has only issued a handful of judgments.[31]

The above is premised on an identification of empirical scholarship with facts, and while it is generally the case that empirical scholarship will be strongly focused on facts, this may come in gradations. Holtermann and Madsen seem hardly troubled by any conception of what 'facts' means: they think, following Bourdieu, in terms of fields and actors doing and saying things and responding to one another. There is no particular ambition here of quantifying facts and, indeed, not even much of a focus on facts: to the Bourdieuian, an impression can be as empirical as an occurrence. The question then presents itself whether the New European Legal Realism they discuss and advocate can indeed be considered as a version of legal realism without stretching that term beyond the breaking point. Holtermann and Madsen clearly seem to think so: what the various legal realisms have in common is that they are

30 See J. Klabbers, 'Intervention, Armed Intervention, Armed Attack, Threat to Peace, Act of Aggression, and Threat or Use of Force – What's the Difference', in M. Weller (ed.), *Oxford Handbook of the Use of Force in International Law* (forthcoming).

31 At least three additional issues are involved in her example. First, how does she define the unit 'judgment'? Does it include interlocutory measures or advisory opinions? Does she distinguish between judgments on the merits and judgments on jurisdiction or admissibility? Are (possibly) separate decisions on costs included? Second, what does 'roughly 75%' mean? Is the real number 74, 82% and just cosmetically improved upon? Or does 'roughly 75%' signify anything between, say, 68% and 82%? Third, how do other courts fare? The figures as presented look dismal, but perhaps they are typical for human rights courts.

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all 'seeking a rigorous empirical science of law'. Even so, clearly the term 'empirical' means different things to different people.[32]

For international lawyers in particular, an important question relating to empirical work is which parts of international law to focus on, as international law as a whole is too broad a field to cover in any meaningful way: statements such as Henkin's classic about international law being observed by almost all states almost all of the time[33] remain impressionist at best, and are not terribly specific. An empirical focus might zoom in more closely, but then the question arises, on what? Typically, empirical work concentrates on issues of great political salience, presumably on the basis of the idea that if international law can be seen to work on such topics as human rights, investment protection, or armed conflict, then it can work anywhere.

Nonetheless, there is an issue of selection bias here: focusing on issues of great political salience often takes place at the expense of issues of less salience, yet here too international law is present. Every time a plane lands after an international flight is international law applied, and every time a postcard reaches an addressee located abroad is international law applied. Put more starkly, so starkly perhaps as to invite ridicule: Is every day that goes by without Russia invading Finland a victory for international law? If a Russian invasion would signify non-compliance, should the absence of invasion then be seen as compliance? And how to qualify minor transgressions, such as the unauthorized presence of Russian planes in Finland's air space? Either way, an empirical orientation on international law would have to include mundane settings and non-events as well, and not limit itself to issues of great political salience or moments of crisis.[34] Yet, empirical work on aviation law or postal regulation seems to be scarce or non-existent, as is empirical work on the non-event. And even that qualification itself is dubious: surely, the safe landing of an aircraft is an event of sorts. It may be mundane, but is pretty relevant to the passengers on board.

Moreover, there is a risk that empirical scholarship ends up concentrating rather too much on compliance,[35] yet important parts of international law cannot be captured in terms of compliance alone. The law of treaties, e.g. as a body of rules facilitates the collective efforts of states and international organizations, and as is often acknowledged, most of the rules of the Vienna Convention are residual, default rules, from which states (and organizations) are free to depart if they so wish. Hence, a discussion of the law of treaties in terms of compliance makes little sense. Something similar applies to the various principles of state jurisdiction: they

32 It is hardly a coincidence that Madsen often resorts to interviews with participants or to contextual reading of judgments: this too counts as empirical. For examples, see respectively S. Caserta and M. R. Madsen, *Between Community Law and Common Law: The Rise of the Caribbean Court of Justice at the Intersection of Regional Integration and Post-Colonial Legacies*, iCourts Working Paper 2014/10, and M. R. Madsen, 'The Protracted Institutionalization of the Strasbourg Court: From Legal Diplomacy to Integrationist Jurisprudence', in J. Christoffersen and M. R. Madsen (eds.), *The European Court of Human Rights between Law and Politics* (2011) 43.

33 See L. Henkin, *How Nations Behave* (1979), 47.

34 By way of analogy, political scientists started to realize in the 1960s that power is not just exercised through formal decision-making processes, but also through the blocking of formal decision-making and even by keeping potential issues off the table. For a good overview, see S. Lukes, *Power: A Radical View* (1974).

35 See, e.g., G. Shaffer and T. Ginsburg, 'The Empirical Turn in International Legal Scholarship', (2012) 106 AJIL 1.

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facilitate things, but cannot very well be analysed in terms of compliance. Moreover, a focus on compliance closes off other avenues for interdisciplinary inspiration, leaving constructivist international relations scholarship in the dark.

IV

As with any methodological program or manifesto, it might be useful to ask the classic question: *cui bono*? On the assumption that all methods will illuminate some things and obscure others, it may be expected that a new methodological proposal will endorse some agenda at the expense of other agendas. This applies quite openly to the proclaimed new legal realism, as part of the agenda is precisely to be open and upfront about these things. Bodansky, in his contribution, is indeed very open: for him, the law helps to fight environmental issues, and a new realist approach would be better suited than most alternatives and can serve a 'progressive politics', although he is generous enough to allow for the importance of doctrinal studies of international environmental law. Shaffer is markedly less open: while his framing paper *cum manifesto* suggests that the point of empirical work is 'to inform action' and ´to inform pragmatic, purposive interventions', he leaves unsaid whose action and interventions he has in mind. Elsewhere, he suggests (as co-author) that empirical work might result in greater effectiveness, but again without specifying to whose benefit – the political question is systematically dodged.[36]

Alexandra Huneeus makes clear that hers is a quest for 'more effective policy suggestions based on empirical observation', with human rights legal scholarship being hoped to be 'more useful and productive'. Since much of her paper addresses issues of compliance, hers seems to be engaged scholarship for human rights. That is not quite as straightforward as it may sound though, in that human rights courts themselves have proved quite adept at finding ways to limit the restrictions human rights law may impose on states. And if that is the case, then compliance may not be all that difficult.

Interdisciplinarity calls have always been heard. The well-known Amsterdam-based lawyer Joannes van der Linden could write in 1806 that the proper study of law would not be possible without a decent grounding in philosophy, in particular logic and rhetoric, as well as mathematics and a solid command of the relevant language. And a few years earlier, in 1798, a professor of natural law at the University of Groningen wrote that 'the sciences are so closely intermingled with each other that one cannot exist without the help of the others'.[37]

Insightful as these propositions are, there is always the risk of going overboard and reducing law to either logic, or economics, or politics, or something else. While Shaffer's manifesto is reasonable and subtle enough, elsewhere one cannot help

36 See ibid., at 43. Note that the quest for increased effectiveness presupposes that the contents of international claw are considered normatively desirable. Again, this political question is systematically dodged.

37 It sounds better in archaic Dutch: 'De weetenschappen zyn zo naauw met elkanderen verknogt, dat d'eene buiten behulp van d'andere niet kan bestaan'. The professor concerned was Frederik Adolf van der Marck, and his words and those of Van der Linden are quoted in J. Wiarda, 'Toespraak', in J. Wiarda et al., *Volkenrecht en wereldvrede* (1963) 7, at 7–8.

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but note that the preponderance of works that he cites as examples of empirical scholarship in international law are, really, studies performed by social scientists and economists or, less often, by lawyers with a strong social science or economics background and bent. Take these out of the equation, and it seems there is little empirical work being done by international lawyers, which suggests that the notion of 'empirical' may be skewed. It is probably no coincidence that the two central questions in that article relate to the production of law (the why rather than the how) and the conditions for its success – neither of these are invariably regarded as legal questions.

Still, any new approach that can help shed light on ignored issues is in principle to be welcomed, all the more so as it will help identify blind spots, and this applies to the New Legal Realism as well. In good hands, it can no doubt produce good scholarship; in bad hands, it will produce bad scholarship, and there is a decent chance that much of the work will turn out to be fairly indifferent. But what is vital is not so much the creation of yet another approach with its own set of practitioners, preachers, disciples, and apostles, but rather that the lines of communication between various approaches are not closed off. The rationalist has much to learn from critical studies and vice versa; the feminist can learn from the positivist as well as the other way around, and surely, the new legal realism too can contribute a thing or two. Expectations need not be overblown: the new legal realism will probably not provide international lawyers with a new lease on life, but if scholarship is a matter mostly of muddling through, then all help is welcome.