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How (Not) to Disappear Completely: Pedagogical Potential of Research Methods in International Relations

Can E. Mutlu
Bilkent University, Ankara, Turkey

Abstract

The question of research methods, and their role in the field, is a major source of contention for IR scholars. We can, however, discuss method, methodology, and innovation without revising or revisiting old debates. Methods do not have to be divisive, or disciplining. A frank discussion of research design, methods, and methodological preferences is essential to innovation and reproducibility. This intervention is a call for increased transparency in IR research outputs; IR theorists should not erase their own footprints from their publications and openly admit and discuss failures as productive moments in research. The act of disappearing, which has become the norm in the name of professionalised publications, robs the field of the productive pedagogical potential of research methods. The true impact of research rests in its pedagogical potential. As researchers, our job is thus to find a sensitive balance between not determining the outcome of the research from the get-go by making it all about our preferences and opinions, but also not making the impact of our preferences and opinions disappear completely. Building on this premise, this intervention discusses the significant pedagogical potential of research methods, reproducibility and discussion of failures in International Relations.

Keywords

research methods, research design, reproducibility, pedagogy, failure, scientific method

Corresponding author:
Can E. Mutlu, Department of International Relations, Bilkent University, Main Campus, A-Building, Room: A-308, Bilkent, Ankara, 06800, Turkey.
Email: canmutlu@bilkent.edu.tr

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Introduction: A Peek into the Kitchen

This is a call for increased transparency in critical International Relations (IR) research outputs; critical IR scholars should not erase their own footprints from their publications. The relationship between methodological transparency – that is, clear discussion of the preferences that impacted the research design process and the ways in which empirical information was collected and analysed through different research methods – and pedagogy – that is, reflexive engagement with the theory, practice, and praxis of research – is a significant one that should be central to discussions of research methods in critical IR.[1] The act of disappearing, which has become the norm in the name of professional(ised) publications, robs the field of the productive pedagogical potential of research methods. When a researcher removes their footsteps – preferences they made during the research design process, difficulties, failures, and shortcomings they have faced during the research phase, and the thought that went into selecting research methods to engage the empirical – they are eliminating methodological transparency, the possibility of reproducibility, and the productive pedagogical potential of research methods. Discussions of research methods facilitate attempts to reproduce research design by young researchers and students. Discussing options, preferences, and shortcomings provide a window inside the theory, practice, and praxis, or 'pedagogy,' of research. Reproducibility, in this case, refers to reproduction of research design and uses of research methods, and not the research results. The aim here is not to reproduce and/or test results of a project, as it is often the case when the term is used in discussions of positivist research methods; various sub-branches of IR do not share common epistemologies. They are not commensurable and should not be treated as such. It is nevertheless important to understand the ontological and epistemological preferences of these different approaches. The reproducibility of methods is important for the formation of young researchers; it allows them to understand the different ways in which researchers use methods to analyse the empirical.

If the research process is a kitchen where we produce the appetising meals that we serve to our community in the form of journal articles, books and chapters, then hiding our recipes does no justice to those following and supporting us materially and intellectually; colleagues, students and the general public have a right to know how the end product was produced. We need to be transparent about the steps we have taken to come to our conclusions so that others can understand the research process. Openly shared knowledge has an emancipatory power; secretive knowledge has an exclusionary and dogmatic power. Undeniably, IR is a field with many approaches, sub-approaches, and ontologically and/or epistemologically varying perspectives. The field is continuously twisting and 'turning' to seek 'newer,' or different, directions to make sense of an empirical world that is naturally messy. The so-called 'methods turn' within certain approaches

1. I would like to limit the present argument to critical approaches to IR in general for the purposes of coherence and clarity. However, the same argument can be made for mainstream IR and even different branches of social sciences.

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to critical IR embraces methods as a way to study the messy empirical in a reflexive, yet clear and rigorous way.[2]

There are clear differences between the supposed 'core' of the discipline and various 'peripheries' that either engage with the ideas carefully defended by this core or ignore them altogether to declare independence. Research methods and methodological issues are often presented as one of the main dividing lines between, and within, the core and its many peripheries. These various approaches to IR, however, have at least one thing in common: they all have distinct performativities. The so-called 'great debates' of the discipline[3] are in fact historical narratives of the interactions of these performativities that are played over and over again in an attempt to discipline the field into becoming a coherent 'discipline.' These performativities are informed either by a theoretical preference, a strong dedication to a certain method or epistemological belief system, or a self-positioning that places the author in an inside/outside relationship through attachment to a different discipline such as anthropology, economics, futurism, geography, gender studies, philosophy, or sociology, among many others.

This tribalism in IR has left the researchers working in the field unable to communicate with one another. The question of research methods, and their role in IR, is a major source of contention for scholars in our field. One of the things demonstrated by the keynote lectures of Patrick Thaddeus Jackson and Andrew Bennett at the 2014 Millennium Annual Conference, which are also included in this issue, was how seemingly incompatible the different perspectives were and how methods can be divisive. Had it not been for the fact that the two keynote lectures took place during the same conference – albeit on different nights – I would not have guessed that the scholars were talking about the same issue: methods, methodology and innovation in IR. This is partially due to the long-running positivist-pre/post/anti-positivist debate. This debate engaged with the epistemological roots of the interaction between the international and the social, and whether those roots can be quantified in datasets and generalised, or whether they are case-specific or irrelevant for the theorisation of the international. But it is also partially due to the supposedly disciplining and limiting effect of methods. This is one of the reasons why those pursuing the so-called 'methods-turn' in critical IR are facing considerable resistance within that community.

2. Mark B. Salter and Can E. Mutlu, eds, *Research Methods in Critical Security Studies: An Introduction* (New York: Routledge, 2013); Laura Shepherd, ed., *Critical Approaches to Security: An Introduction to Theories and Methods* (New York: Routledge, 2013); Claudia Aradau, Jef Huysmans, Andrew Neal and Nadine Voelkner, eds, *Critical Security Methods: New Frameworks for Analysis* (New York: Routledge, 2015); Claudia Aradau and Jef Huysmans, 'Critical Methods in International Relations: The Politics of Techniques, Devices, and Acts', *European Journal of International Relations* 20, no. 3 (2013): 596–619.
3. Luke Ashworth, 'Did the Realist-Idealist Great Debate Really Happen? A Revisionist History of International Relations', *International Relations* 16, no. 1 (2002): 33–51; Ole Weaver, 'The Rise and Fall of the Inter-paradigm Debate', in *International Theory: Positivism and Beyond*, eds Steve Smith, Ken Booth and Marysia Zalewski (Cambridge: Cambridge University Press, 1996), 149–85.

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Methods do not have to be divisive or disciplining. Reproducibility does not have to be about testing in order to assess validity and reliability of data. The way it is used in this article, reproducibility refers to the ability to demonstrate to others how we do what we do, so that those who are interested in our approach can decide for themselves if it works for their project. Reproducibility, used in this context, is not about disciplining, harmonising or standardising approaches; it is not about hypothesis testing or establishing reliability or validity. A frank discussion of research design, methods and methodological preferences is essential to innovation and reproducibility. That is the basis of science and knowledge generation. Even the word method, which stems from the Ancient Greek word *methodos*, meaning systematic course or road, implies that the concept is more about explaining the way in which the research was conducted. This, I argue, has been the original idea behind knowledge production. The knowledge production process has to be accessible and reproducible, not simply to make our findings more accessible to the general public and democratise knowledge production, but also to fulfil the pedagogical potential of research methods.

This paper builds on a central argument: clarity and transparency of discussions on research design and methods is central to the reproducibility of our approaches to research. This is the pedagogical potential of research methods. Building on this premise, this article has two sections. The first section looks at the scientific method and a side of it that often gets overlooked in arguments for and against it in IR: its pedagogical potential. The section provides a discussion on pedagogy. This discussion is followed by an engagement with the productive pedagogical potential of research methods and reproducibility for critical IR. The second section looks at a particular challenge of research: failure. I argue that the omission of discussions of failure from publications within the context of debates surrounding transparency and reproducibility of methods has negative effects on pedagogical potential of methods. To address the significant omission of failures and other shortcomings in the research process from most accounts of IR research, I ask the question: why do we not discuss both the disruptive and productive effects failure has on our research process?

Method, Pedagogy and Science

The world is not at the centre of the solar system, with every celestial object within our system circling around it. We, the humans, did not always know this to be the case. We now know this fact. We do not know this simply because we can Google it or read about it elsewhere, or simply because Galileo said so himself. We know this, because we know 'how' to measure the absence of the 'stellar parallax,' or the apparent change in position of objects due to a change in observation location. In other words, more significant than the fact that Galileo proved that the Earth is not at the centre of the solar system was his ability to teach others how they can observe the same phenomenon themselves.

Knowledge generation is driven by at least two factors: curiosity and an understanding of the conditions of possibility for a legitimate knowledge claim. In order to first identify the movement of the Earth and other observable objects visible from his telescope, Galileo had to be curious about the Earth's place in the solar system. But he also had to know about Aristarchus of Samos, Copernicus, and Kepler's theories and methods. He had to

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have known about different methods to make his case, and present it to the public. In doing so, he also had to present a way for others to come to the same conclusions.

Making knowledge (accessible to) public is an important part of knowledge production. Knowledge should not be generated exclusively, or for the exclusive consumption of a circle that is 'in the know.' Furthermore, it is not just knowledge claims, but also the means of knowledge production that should be accessible to the public. The issues of access and accessibility have become the Achilles heel of a majority of works coming out of reflexive and post-positivist approaches to IR. Often, we simply do not know how a researcher arrives at their conclusions. We might agree with the conclusions, or not, but conclusions only give us an understanding of the findings and their impact within the limited context of the work. They do not provide us with an insight into understanding the research process, the preferences, and the difficulties faced by the researcher. That is a lost opportunity, in the pedagogical sense.

Pedagogy, in general, refers to the theory, practice and praxis of education. In this article, however, I focus on a particular kind of pedagogy: reflexive or critical pedagogy of research training. Understanding the theory, practice and praxis of research design and methods is a central part of any research process; we must understand and be aware of our preferences and the choices that are available to us, and how methods create the conditions of possibility for our research findings. 'Problem-based learning' (PBL) is a valuable pedagogical approach that places the student, or in this case, the researcher, at the centre of the learning process.[4] The PBL approach has become popular in post-secondary education classrooms, starting with medical sciences. As part of this pedagogical approach, through a problem, students identify what they already know, and what they need to know to solve the problem at hand. In social sciences, this allows students to make connections between the issue-at-hand and the larger trends and theories they learn as part of their curriculum; it allows students to engage the material in relation to the empirical and do so in a reflexive manner. The PBL approach presents a valuable insight into the research design and methods for researchers. If we start thinking about the research process as problem, or as a process that could be further problematised through our discussion of it, then we can allow other researchers to make connections between our research and theirs, and engage both the project-at-hand and the process of research reflexively to draw lessons. This requires a clear and open discussion of the preferences that went into the research design process and the reflection on the research method selection and the kinds of possibilities and constraints that come with those selections. In return, the proliferation of discussions of reproducibility will increase the productive pedagogical potential of research methods in critical IR scholarship.

Limited engagement with method in a large portion of post-positivist and/or 'reflexive' research is driven by (a) satisfaction of curiosity alone and (b) fear of the disciplining drive of methods. Curiosity, however, represents only the first stage of research: the stage when we decide on which questions to ask and what to look for. This is where we define our 'puzzle', or the 'problematique' that we will try to conceptualise, theorise

4. Kurt Burch, 'A Primer on Problem-Based Learning for International Relations Course', I*nternational Studies Perspectives* 1, no. 1 (2000): 31–44.

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and understand. It is, however, the researcher's duty to explain their process, preferences, logic and conclusions clearly and directly. It is one thing to admit that the social and the political are messy concepts,[5] or that the reality is far too complex to be modelled or forecasted. It is, however, a completely different thing to say that all methods, or call for methodological transparency, require a move towards further disciplining the field and curbing creativity. After all, there is a method to every process.

The attempt here is not to discipline the field through formalised methods, or curb its potential for creativity; this is merely a call for increased transparency into the research process for the sake of making knowledge accessible to the public. The motivating desire behind this move is to understand how the sausage is made. Rather than saying there is only one way to make a sausage, the aim is to bridge the ever-increasing divide between the researcher and the reader. In return, the motive for this move to make the knowledge-making process accessible and transparent, or reproducible, is the belief that understanding the process is pedagogically significant for the reasons I have discussed above in relation to the PBL approach.

There is an existing model for this already, which I briefly discussed above. When combined, questions of science and method often lead to discussions of the scientific method. The scientific method refers to a general set of techniques and practices of investigating phenomena that result in the (re)production of knowledge either through discovery, or interrogation, and correction of existing knowledge. The scientific method in IR, however, has a particularly 'disciplining' legacy in favour of the positivist method as it operates under the assumption that reality can speak for itself through hypothesis testing. In this approach, an existing knowledge base is rarely – if ever – stable.

On the one hand, the scientific method requires a questioning of dogma or the existing knowledge base. As such, it has an emancipatory function against dogmatic knowledge claims. On the other hand, there is nothing inherently scientific about a particular method or group of methods. That is an exclusionary understanding of science that goes against scientific method's core principles. 'Science' is not something people wearing lab coats do using microscopes, or by crunching numbers. Numbers, formulas, charts and statistics are not inherently 'scientific' by design; certainly not more scientific than archival research, ethnography, mapping or any other post-positivist research method.

Positivist methods are not more 'scientific' than post-positivist methods. However, when compared to positivist methods, post-positivist methods lack a direct engagement with the topic of reproducibility. This is a confusion that comes at a pedagogical expense. More so than hypothesis testing, or openness to failure, what makes scientific method emancipatory and democratic – in the more general sense of the word – is that it allows readers to retrace and repeat the steps of the author, not only to check against mistakes and confusions in the text, but also to learn how the research was conducted in the first place. There is a great deal of pedagogical potential in that. Students, or other researchers, can benefit from knowing or understanding the various stages of the research process that led to the publication they are reading.

5. John Law, *After Method: Mess in Social Science Method* (New York: Routledge, 2004).

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There are obvious differences between the social sciences and natural sciences and our predispositions towards their respective findings. For social scientists, data can never be raw; it is always selectively collected, mined, or drafted for someone and some purpose. For natural scientists, data can supposedly speak for itself. As one recent edited volume so aptly put it, in relation to the social sciences: '*Raw Data is an Oxymoron*',[6] an unattainable goal. This is because, from the moment we design means/methods/processes to collect data to the moment that we start analysing the data collected, the process is driven by our intentions and preferences. We know 'what kind of data' we want, and why.

Even though, for reflexive scholars, the 'real world' is not something that speaks naturally for itself, there are certain pre-determined 'proofs', 'benchmarks' or 'standards' that instruct the researcher when to stop researching, or when to be satisfied with our findings. As students, we establish these during the much dreaded 'proposal defences'; as researchers, we establish these more quietly or internally, or lay them out clearly in grant proposals for a 'broader audience' to understand our goals and objectives. After the research process is completed, these unspoken standards get evaluated in peer-review for book manuscripts, or articles where methods sections are removed because they are considered to be unnecessary. In conferences, we present findings or theoretical innovations to our peers without discussing methods or processes. The aim of these moments of public interactions thus becomes all about the findings on empirical and theoretical conclusions. This is, once again, a missed opportunity.

One way out of this is empiricism. We need to be reflexive about the empirical and through the theoretical, and not be reflexive only in theory, through theory. What makes Bourdieu, Foucault or Latour increasingly appealing to IR scholars is not simply their theoretical wit or intellectual significance for other fields that are becoming increasingly significant for IR scholars. Their relevance is in part due to their emphasis on the empirical, their methodological rigour, and their ability to make their method clear and reproducible. Bourdieu's discussion of French academia,[7] Foucault's discussion of the historical transformations of various forms of disciplining and punishments,[8] or Latour and Woolgar's account of the everyday practices of lab workers[9] all have at least one thing in common: they present a clear account of their research process. Reading these masterpieces is important not just because of their findings, but also to understand how they arrived at the conclusions they present. 'Field analysis', 'genealogy', 'archeology' and 'actor-network theory' present reproducible methods. This is partly why they are so popular in critical IR. They present methods that are reproducible. The conclusions of Bourdieu, Foucault and Latour's research should only be seen secondary to their discussion of methods. That is to say, the conclusions of these three authors would not have had the same impact without their clear reflections on field analysis and mapping, genealogy/archeology or participant observation/actor-network analysis, respectively.

6. Lisa Gitelman, ed., *Raw Data Is an Oxymoron* (Cambridge, MA: MIT Press: 2013).
7. Pierre Bourdieu, *Homo Academicus* (Stanford, CA: Stanford University Press, 1988).
8. Michel Foucault, *Discipline and Punish: The Birth of Prison* (New York: Random House, 1995).
9. Bruno Latour and Steve Woolgar, *Laboratory Life: The Construction of Scientific Facts* (Princeton, NJ: Princeton University Press, 1986).

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Providing room for methodological reflections, in the form of a discussion of research methods and preferences in academic publications, has a pedagogical potential that is currently missing in contemporary approaches to reflexive IR. This potential is being overlooked in the name of creativity and anti-disciplinarity. In this section of the essay, I discussed an overlooked aspect of the scientific method beyond formal methods and hypothesis testing. The scientific method has an established tradition for making not only findings, but also methods public. This, I argue, has a great pedagogical potential. The objective here, as such, is not to further discipline the field to make it resemble natural sciences, or make it somehow more uniform, but to seek increased transparency in the research process for the sake of making knowledge accessible to the public. Building on this idea, the next section looks at a particular form of omission that if reversed can benefit the field: discussing failure.

The Missing Link in Methods: Failure, Method, Pedagogy

Reading IR publications as an outsider to the field, one would think that the research in our field is almost universally without any disruptions or failures, as if the researcher sets their eyes on a project and pursues it without facing any difficulties. If you ask researchers, however, this is, more often than not, not the case. Failure, in relation to research, could refer to a number of instances. Failure, in general, refers to the inability to meet a desired or intended objective. The key word in this definition is intent. When used in that sense of the word in relation to the research process, failure is the inability to meet intended research results; this happens especially when the empirical world does not overlap with our arguments, expectations and intuitions as researchers. Failure, in that sense, could refer to the researcher's inability to access the necessary information or data, or their inability to find proof for their argument. We can also speak of other kinds of research 'failures', such as emotional, material or personal failures that affect the research process one way or the other. Inability to acquire grant funding, or the inability to cope with the stresses of field research, the inability to ensure safety in the field, or the inability to receive permission to conduct interviews, access archives or research sites. We can interpret these 'inabilities' as 'failures' in the pejorative sense, or we can interpret them as junctions or opportunities that every researcher goes through.

Empirical research provides endless challenges. It is practically impossible to try to envision these 'failures' at the research design stage. We can try to anticipate them and try to address them to the best of our abilities, but nevertheless, we are very likely to face difficulties that we cannot control. Not discussing these 'failures' or representing our research project as single uninterrupted progress without junctions, difficulties or failures, presents a distorted image of the research process. Failure, in the sense it is used here, does not simply refer to a point of shortcoming of the researcher or the research process, far from it. Failures are an organic part of the research process. Rather than ignoring them, or representing the concept of failure in the pejorative sense, we must regard it as a productive opportunity. Even the best-designed research projects, with clearly identified data collection sources and methods and with plenty of resources, might face unforeseen problems; researchers might not get travel visas, or face other forms of access-related issues, the archive they plan to use might be destroyed, a key

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person to interview might pass away, or there might be more personal/material issues, such as family-related issues or financial difficulties, that affect the research process. There are numerous forms of disruptions or failures that shape research and we cannot account for them at the research design phase.

There is, however, no reason to think of disruptions and failures in the pejorative sense. We hide failure to show coherence and continuity. We shy away from discussing failure because it is seen as a sign of a bad research(er). Failures, however, often do have a productive impact on a research project. As the saying goes, 'necessity is the mother of innovation'. By ignoring, or omitting, instances of failure in the final research outputs, we construct a representation of the field in which the researcher has constant control over the empirical. This goes completely against the idea that the social and political are messy; a principle that is gaining more of a foothold in critical IR. The representation of the 'researcher-in-control' presents an imagination of the research process in which the researcher knows and controls the final destination and the empirical is just the means through which to reach that destination.

Earlier, I have suggested that in social sciences data does not speak for itself and that data collection is always for someone and for some purpose. While this holds true, this does not mean that research in the social sciences is a process where we try to retrieve and fit appropriate data into an existing framework to make an already established conclusion 'work'. Even if data does not speak for itself, we should let the research process guide us to our conclusions rather than use the research process to arrive at previously determined conclusions. Failure, however, is an inherent part of letting the 'research process guide us'. Failure is often presented as something to recover from or something to correct, but failures are also learning moments; we need to learn from them. Failures and disruptions can either become the basis of new projects, or the things we learn from them can be incorporated into the existing project. If we are not discussing failures, disruptions or challenges publicly, what we are doing is helping keep the worst secret in IR: we all fail somehow.

Failure is a major part of scientific method. Researchers are not only able to learn from the failures of those preceding them, but can also learn from their own failures. It is highly unlikely that today managers at Pfizer look back at Viagra and think of it as the failed hypertension drug that it is. Failure in IR has the potential to lead to similarly interesting new openings in research in two ways. First, failure can force us to rethink what is not working in the project and revisit our assumptions or understandings of our topic of research. Secondly, we can internalize the failure in such a way to inform our future research. My inability to receive a travel visa to the 'field' to do research during my PhD forced me to rethink my project and change things around. In the short term that was an utter disappointment and a major roadblock during an already stressful time. In the long term, however, this made me think more about global visa regimes and how they work, how we respond to them, what factors determine a positive or a negative response to visa applications, or how the risk factors in the process are determined.

Similarly, Luis Lobo-Guerrero's[10] discussion of research failures surrounding the archival research that he conducted for a chapter in his book *Insurance and War: Political*

10. Luis Lobo-Guerrero, 'Archives', in *Research Methods in Critical Security Studies: An Introduction*, eds Mark B. Salter and Can E. Mutlu (New York: Routledge, 2013), 122–5.

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Economy of Marine Security and Risk[11] is an interesting example of a frank discussion of research failures and their productive potential. In his reflection, Lobo-Guerrero discusses how he coped with the fact that the particular archive he hoped to study – the Lloyd's of London's historical archive – was 'lost in numerous fires over the centuries'.[12] His ensuing discussion of the alternative strategies he developed and the techniques he pursued – understanding the logic of classification to find particular documents he was seeking, photographing the documents he found, among others[13] – in order to gather similar data from an alternative source – the UK National Archives – is a good example of how researchers can explain the process through which they tackled research failures. Reading Lobo-Guerrero's reflections on the productive potential of failure provides a valuable insight for researchers working on archival research and challenges associated with this kind of research. Such a discussion makes a direct contribution to research methods pedagogy.

The omission of failure from our research outputs in the name of 'professionalism' results in the sterilisation of the research process. Had Lobo-Guerrero not discussed his failures, he would have missed an opportunity to make a pedagogical contribution to the field. Such omissions create an unhealthy self-image problem for the field. The image of what a successful project should look like is unattainable. To fall within the standards established by that image, we either compromise on the empirical or make it all theoretical. The preconceived ideas of 'normal' and 'successful' projects reflect this omission, as there is no room to discuss failures or disruptions in the process. Those are only left to be shared in private conversations. These omissions show the already disciplined nature of the field. We can address these issues within the larger question of discussing methods, preferences and processes. We must consider discussing failures as part of research methods, as the implications of failure have a direct effect on research methods. This goes hand-in-hand with the call for increased transparency in research processes that I presented at the beginning of this intervention. Discussion of failures, methods and even failed methods, has a pedagogical potential that is yet to be addressed in reflexive IR. Without such discussion, the answer to '*Quo Vadis IR?*', the theme of this year's Millennium conference, can only be: nowhere.

Conclusions: Innovation without Transparency?

We can discuss method, methodology and, in particular, innovation without giving the impression that we are revising the old debates or – even worse – chasing our own tails. Not everything that 'needs' to be said in our field has been said by those that came before us. What we cannot do, however, is to talk of innovation without discussing reproducibility in the context of methods. To know where we are going next, we need to know where we have been before. To know that, we need to know how we got there. Understanding and embracing method, not as a disciplinary power play, or an instructional manual, but

11. Luis Lobo-Guerrero, *Insurance and War: Political Economy of Marine Security and Risk* (New York: Routledge, 2012).
12. Lobo-Guerrero, 'Archives', 122.
13. Ibid., 123.

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as discussion of 'how one has done something', has a productive pedagogical potential. That potential for reproducibility or retraceability remains unfilled in critical, or reflexive, IR. To be able to discuss innovation, we need to be able to reflect on the field as a whole, and the approaches used in research projects that constitute the field, including its successes as well as failures.

We cannot pursue innovation without embracing failure first. We cannot pursue innovation without learning from past challenges and disruptions. Given the current state of the field, with very limited discussion of failure, and almost no discussion of methods, there is no chance to speak of innovation coming from inside the field. As such, what we see is innovation coming in the form of interdisciplinary connections from anthropology, geography, history, philosophy, sociology, and science and technology studies, among others. This in and of itself should not be a source of concern for the field. Cross-pollination is the way to keep the field diverse and healthy. However, these 'plug-and-play' approaches to interdisciplinarity result in a failed opportunity to reflect on how these methods and innovations fit in our field. We seem to accept the outcomes of their debates and discussions without reflecting on them much. That is, once again, a failed pedagogical opportunity.

If research is both ethical and political, as I believe that it is, then we need to reflect on the ethical and political implications of these moves to incorporate other approaches and discuss these preferences in detail in our writing. After all, research is conducted to inform the public and those following after us. Without any food crumbs, however, we are not establishing much of a path to be followed. The true impact of research rests in its pedagogical potential. As a researcher, our job is thus to find a sensitive balance between not predetermining the outcome of the research by making it about our preferences and opinions, but also not making our preferences and opinions disappear completely from it.

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Author Biography

Can E. Mutlu is an Assistant Professor of International Relations at the Bilkent University in Ankara, Turkey. His research interests intersect three areas – security, science and technology, and international political sociology – with a specific focus on emerging research methods. He is the co-editor of *Critical Methods in Security Studies: An Introduction*. His recent research appears in *Comparative European Politics, European Journal of Social Theory, Eurasia Border Review, Environment and Planning D: Society and Space and the Review of International Studies, Critical Studies on Security*, and *International Political Sociology*.

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