** **Reply** (19/11/2014) **to** **Edmund Chattoe-Brown**'s review of *Analytical Sociology: Actions and Networks* (published in JASSS, 17, 4, 2014), plus **my reply** (18/12/2014) to **his reply** (02/12/2014) to my reply and **his reply** [19/12/2014] to my reply to his reply –after which we agreed on not iterating further)**

******************Thread A*****************

(A) (GM –19/11/2014)

Unless I am wrong, the main criticism that recurs throughout the review is that the book is unconvincing with respect to the distinctiveness of analytical sociology. More precisely, you argue that the single chapters "don't look much different in practice from existing forms of research" (p. 2). I agree on this point. However, it seems to me that this was not the aim of the book.

Since the preface, I explain that my introductory chapter proposes a specific understanding of what analytical sociology may be and that each chapter focuses on one or some of the aspects composing the general framework. I then clarify that "the purpose behind this architecture is neither to "speak for analytical sociology" nor to polish its present boundaries. More modestly, the book is an endeavor to deepen our understanding of what analytical sociology may become in the future and to enhance the exchange between analytical sociology and other theoretical and methodological approaches" (p. xiv). In my short introduction to my own chapter, I write "The book's aim is to accumulate elements that may foster the further development of this kind of analytical sociology. *Analytical Sociology: Actions and Networks* is not intended to be about the past or the present of analytical sociology: it points to (one of) its possible future(s)" (p. 3). Finally, in the concluding section "How to read this book" to my introductory chapter, I note "In selecting the contributors, my aim was not to oblige them to adhere to the variant of analytical sociology that I have defended in the previous pages. Nor was I interested in knowing whether they accepted or rejected the label "analytical sociology." My intent was instead to collect resources for the further development of a specific understanding of analytical sociology. Setting up a research program is a collective enterprise, and we know that distributed heterogeneity helps us find better solutions and enriches our thinking (Page, 2007). This is the spirit behind *Analytical Sociology: Action and Networks*."

Thus, given the book's project, to what extent is it fair to attack the book by arguing that "most of the articles" do not "stand(ing) out as a special thing called AS"? When you state that I am "happy to allow individual chapters to "focus" on only some of these (or less charitably ignore others)" interlocking pieces, are not you caricaturizing my goal? When you say that the book "branded as AS" these articles, are not ignoring my explicitly claim that the AS I am favor does not exist yet and the book want to contribute to move the research to this direction? In the light of the above quotations, can you really claim that the following criticism apply to the way I frame the book: "AS may not make itself popular with other disciplines if it simply absorbs anything it happens to approve of (regardless of subject area or method) or condones the same weaknesses in its adherents that it appears to criticize in others."

In a word, your review gives readers the impression that I oversold the current state of analytical sociology whereas the book is very explicit about the current heterogeneity of analytical sociology –this is indeed the true starting point of my introductory chapter– as well as about the fact that most of the chapters do not fall within the definition of AS I favor. In the book's *Coda*, I even wrote: “Readers and commentators will probably complain about this heterogeneity. Even worse – given that the last modus operandi is the only one that accords with the understanding of analytical sociology discussed in my introductory essay – reviewers will probably criticize the partial mismatch between the book’s introductory essay and the rest of its content.” (p. 421). The book is all about this tension between a specific understanding of AS and what AS is not yet. I decided to stress and explicitly state this tension because I think that this is consequential for future developments. Thus, my counter-criticism to your main criticism is that your review does not make justice of the book’s goal and destroy the balance of my presentation (it is of course possible that I was not clear enough but, since others got the point – see, for instance, http://understandingsociety.blogspot.fr/2014/10/computational-models-for-social.html –I am inclined to think that the book project was spelled out with sufficient clarity).

[A1] (ECB –2/12/2014)

I think the reviewer and editor have different jobs. The editor chooses and situates the contributions. The reviewer reacts to the book as a whole. If I am right about this then the fact that you mention your aims for the book at various points in your introduction and conclusion does not actually contradict my claim that most of the content of the book looks very much like existing good quality sociology. The captain of a ship can steer it to avoid accidents but if it is heading towards the rocks, no amount of standing on the shore and shouting “everything is fine” will help!

(A1) (GM –18/12/2014)

I agree that the reviewer and the editor have different jobs. I also agree that the reviewer has to evaluate the book as a whole. I think however that a *good reviewer* should not ignore what the editor says. As I wrote in my original objection, my criticism is that you did not discuss explicitly at any point my honest evaluation of the current state of AS and my own project with this book. Even, in the book’s conclusion, I anticipated and discussed the type of criticism you formulated. This is not mentioned either in your review. As a consequence, your review is not accurate and gives the impression that I am overselling AS. Your (very nice) “ship” metaphor makes the same mistake: it ignores the purpose of the book project and caricatures the way I frame my arguments.

[A1] (ECB –19/12/2014) I’m afraid we are arguing in circles here. I don’t ignore your introduction and conclusion (surely a “good” reviewer would say exactly what you want about the book!) What I say is “Despite what Manzo says in his extensive and thought provoking introduction …”

[A2] (ECB –2/12/2014)

There are also practical problems with your stated goals which link to my other points. The more generous you are about what AS (and other concepts like MI) can include, the less convincing are claims that these are distinctive positions. This generosity also makes it much harder for a reader to use existing research (which looks very like the better parts of what we already have) to construct novel approaches in AS. It is one thing to say: “Here is a plan. Build a house.” It is something very different to say: “Here are bricks. Build a house.” If what you want to do is provide raw materials for better AS in the future, are the articles in Manzo (ed.) really innovative enough to expect that to happen? This also links to the club criticism. Arguably, “Chains of

Affection" by Bearman, Moody and Stovel is more analytical (in the sense of covering more of your elements of AS) than anything in your book but it is already published. If you want to give people ingredients then why not produce a reader rather than something that has to be (for relatively contingent reasons of publishing economics and academic credit) mostly original? My concern is then that the people who become examples are not necessarily those doing the best work (by which I mean not objectively best but best to demonstrate and develop AS) but merely those who are available to write a new chapter or want to support the AS group at a moment in time.

(A2) (GM –18/12/2014)

The distinction between "plan" and "bricks" is a good point. Figure 1 in chapter 1 provides *both* a plan and a set of bricks. The combination of both generates a specific variant of AS. Contrary to what you say, this variant is not generous at all. For instance, it is based on a critical stance against limitations of statistical models, takes distance form a certain kind of rational choice theory, gives priority to specific forms of formal models when the design and study of models of mechanisms is at stake. This suffices to generate strong resistances among a large part of mainstream empirical research and theory in sociology (and within AS itself). As to single chapters, as I said, they only aim to illustrate specific bricks (or specific aspects of them). Why did not I take more direct and complete illustration of my "plan/bricks" proposal? The reason is simple: because full applications of my figure 1 are still rare. This is discussed on page 39, where Bearman et alii's paper you mention is quoted as a partial example among these rare applications. The aim of the book is to contribute to stimulate more papers along these lines (meaning figure 1).

[A2] (ECB –19/12/2014) I'm afraid that this "defence" may be why reviewers are needed. Whatever you say about your introduction you cannot take the rest of the authors in the book "with you" if the content of what they write doesn't want to go. I raise two specific concerns about this in the review. One is whether (to anyone but the editor) the chapters really look like something distinctive. The other (which we haven't discussed) is whether despite the very "positive" label of "analytical", some of these chapters recapitulate exactly the faults that their "parent" methods do. Let analytical mean *really* critical not just the kind of criticisms that statisticians are prepared to accept about their own work!

***************Thread B***************

(B) (GM –19/11/2014)

To my reading, a second, more specific criticism you address to the book is that single chapters do not prove that they "are rigorous in a distinctive way (rather than just being more rigorous than average) (…)" –similarly you claim toward the end of the review that you are not sure that "we can delimit "good research" in general terms and trying to do so risks creating a self-regarding club rather than a self-critical field". Again I completely agree (see also my point G).

I am surprised you raised this objection against the book, however. I know that some supporters of analytical sociology defend(ed) an imperialistic and normative stance. I discuss this on page 5, where, again, I acknowledge the variety of points of view existing within analytical sociology. Then I reject this view and I humbly present my own framework as one "particular variant of analytical sociology" (p. 6). Where did you read that I speak of "good sociology"? The reason why I did not follow this normative orientation is that I do not know any metric to assess "good sociology". That is why I proposed to regard AS a "research program" in the Lakatos's sense, i.e. as a set of heuristics. The power of these heuristics can only be judged *a posteriori* and over time. To me, this is the contrary of defining "rigor" or "goodness" *a priori* in a normative way. Without a doubt I may have missed some statements in the single chapters (if so, please point me to them) but, to my memory, no chapter defends this view of analytical sociology as a gold standard for research in sociology.

[B] (ECB –2/12/2014)

I did not mean to imply that you were pushing a particular view of good sociology. But there are widespread debates about what good sociology is and my concern is that the style of argument in AS may not be productive in promoting it. To use an example you discuss (and I worry about in my review), people take differing positions on MI and it does not seem to me that these positions are well supported or that the debate is particularly useful. If you say, or even suggest, that MI (however generously defined) is part of AS then you are excluding anyone who may be non-MI and saying that their work can't be analytical. (It is a problem also that analytical in the everyday use is something that at least some people aspire to be. If you were promoting something called "Anal Sociology" you wouldn't have to worry that you would upset anybody by exclusion! This is what economics does with the word "rational".) We want all sociologists (even if we don't share their epistemological positions) to do the best work they can within their methodology/epistemology and it may be that non-MI sociologists will eventually lead us to models that are still analytical but not MI. (Certainly people who believe in MI will not because they won't even look.) But my core point is that it does not really seem to me that AS *has* to be MI and all that comes from that (even in a weak form) is to exclude people based only on a theoretical conjecture. This is not diplomatic or useful!

(B) (GM –18/12/2014)

We can agree on the fact that there are different strategies to be analytic, i.e. to be clear about concepts and conceive explanation as a form of decomposition/dissection. In my *Analytical Sociology and Its Critics* (European Journal of Sociology, 2010), for instance, I reconstructed in what sense Talcott Parsons had his own project for an "analytical sociology". In the book you reviewed, I defined my own conception of what "analytical sociology" is. As I said in my original objection, I do not propose this as a normative standard for good sociology, which also implies that my goal is not to include the largest number of adherents or supporters. My understanding of AS is conceived as a research program: in ten or twenty years, we will see if this research program will be able to generate good empirical, causal knowledge. In the meantime, I do not care if the assumptions on which the research program is based exclude this or that scholar. Why should I care, given that my aim is not to colonize the entire discipline?

[B] (ECB –2/12/2014) I worry that we are missing each other on this point but it may be inevitable. I can only say again that I don't think that the structures you make on what analytic sociology is/should be are necessary or helpful. You are welcome to disagree and if I am right it is AS that will suffer not me. (But as with A2 above, in a review all I can really do is make a claim and back it up with concrete examples. I did that.)

***************Thread C***************

(C) (GM –19/11/2014)

A third important point that you raised concerns the distinctiveness of ABM compared to analytical sociology. If I read you correctly, your argument is that it is much easier to see the specificity of a paper built on ABM than a paper labeled "analytical sociology". Again (given the current state of "analytical sociology") I fully agree.

The problem however is whether or not an entire research perspective can be founded, and derive its specificity, from a tool. In my opinion, the answer is negative. No matter how powerful and flexible it is, a tool is a tool. A research program is something more. In particular, to me, a research program is a coherent set of theoretical, epistemological, ontological, and theoretical statements (or guesses, according to Lakatos). As I tried to argue in my "Analytical Sociology and Its Critics" (*European Journal of Sociology*, 2010), "analytical sociology" is something specific because it combines a set of proposals at these different levels. Compared to "the generative methodology laid out by Gilbert and Troitzsch (2005) and Epstein and Axtell (1996)" (your own words) obviously there are similarities and overlapping –again this is explicitly acknowledged in my introductory chapter (and elsewhere in my writings –but I do not want to bother you with my CV ☺). However, analytical sociology also contains an explicit reflection on elements (like mechanisms and causality, theory of action, or structure and networks) that remain (at least for now) in the back of the writings of the leading figures of the ABM community. You seem to consider that these are "contentious and possibly irresolvable issues" (see also my point E) whereas I consider that these are fundamental issues that we should continue to think about if we want to get the best from our formal methods. This seems a difference between us and that is perfectly fine with me.

[C] (ECB –2/12/2014)

I am not sure that the distinction between tool and perspective is as clear as you suggest. Also, given the concerns I raise about your book, the actual practice of AS and ABM seem to support my view. AS is very afraid of being misinterpreted, argues a lot about ontological and epistemological issues that are hard to resolve and finds it hard to decide what it is and who is in it. ABM produces some really dreadful research but does not spend a lot of time worrying about philosophy. People seldom feel the need to try and deny that their research is ABM and are often confident that misinterpretations of what they do are simply factually wrong (and they will argue that in a practical or empirical – rather than philosophical – way). I do not mind ABM being just a tool (if you want to say that) because actually we all seem more or less happy to do what we do and agree what that is (though much of it is needlessly bad – but to misquote an old saying "90% of ABM is crud but then 90% of everything is crud.") and thoughtful practitioners can see clear limitations of the existing method and possible solutions to work on for the future. I am not so sure that AS is so happy either inside or out. Maybe just a tool is not such a bad thing to be after all! (And in fact, based on the generally endorsed ABM methodology, we use a tool that has a very clear relationship to specific data even if the methodology is not always observed. We don't need to build epistemological preconceptions in to give the approach substance. The substance comes from data – broadly construed.)

(C) (GM –18/12/2014)

My sense is that you portray the ABM field in a sweeten way. In fact there are deep and constant tensions among supporters of different types of models (abstract versus empirically-ground, for instance) and, as debates on model replication show, what good practices are is still under development and controversial. Tensions also exist among ABM users coming from different disciplines. The definition itself of what an ABM (or, more fundamentally, an "object") raises substantially debates among computer scientists. This internal heterogeneity aside, the ABM community is essentially built on a specific (class of) technique(s) and decided to constitute as a specific community. Thus, despite the existence of considerable internal debates, it is not surprising that it generates fewer misunderstandings and requires less effort to explain outside what it is all about (by contrast, outside, people intensely debates what the real value/nature of the knowledge produced by ABM –like numerous papers among philosophers show). By contrast, as I said, AS relies on a complex web of elements relating to various sub-field (epistemology, theory, methodology, ontology). In addition, AS chose to remain within mainstream sociology and has the ambition to discuss with the variety of perspectives composing it. As a consequence, debates on AS identity are complex and making it understood (and accepted) take longer.

[C] (ECB –19/12/2014) My simplest response is that if you can "advertise" AS then I can do the same for ABM but actually I disagree substantively. You are completely right about the tensions and I have enjoyed such debates myself. But people are not writing of ABM: "What is the point of this?" or "There is nothing new here". They *do* seem to be writing this of AS. (They write plenty of other negative things about ABM, many of which I actually agree with and try to "meet" as criticisms though I also admit I am not typical in ABM. I will forgive myself one citation to my own work on this matter: http://www.socresonline.org.uk/19/1/16.html. I am still well in credit relative to your self-citation!) I'm afraid I can't really see the point in a discussion of whether AS has a "harder time" being an area than ABM based on its strategy for development or whether ABM has an "easy time" because it is a method.

***************Thread D***************

(D) (GM –19/11/2014)

A fourth objection that you addressed to the book concerns a more specific point related to ABM. You claim: "(…) the present volume perpetuates the inaccurate view that ABM was developed almost exclusively by Americans and ignores the considerable independent role of European ABM research in developing many of the ideas presented here. Given that AS has now apparently decided that ABM is a key part of its approach, it seems surprisingly uninformed about what was happening in the field before it received the AS seal of approval.)".

I am really curious to know more about the origin of this criticism. Neither my introductory chapter nor the book as a whole are about agent-based modelling per se, thus I did not judge necessary to write about the history of ABM and the internal complexity of communities (in different disciplines) developing around this methodology. This space and thematic limitation notwithstanding, my section 1.9 explicitly builds (also) on a large set of papers and books from the ABM European community. To limit to some examples: 1/ technical definitions of SMA and "objects" comes from the British computer scientist M. Wooldridge; 2/ technical (and less technical) presentations from Jacques Ferber, Dirk Helbing, J. Epstein (I agree, he is not European but leading figure in the ABM European community) or Gilbert are quoted; 3/ for disciplines different from sociology, like geography or demography, leading European ABM modelers are quoted (see footnote 12); 4/ European philosophers of science thinking about ABM are also mentioned; 5/ several papers published in JASSS on the problems of verification and validation (as well on the tool problems) by well-know authors in this community are quoted (see footnote 13 and 19); 6/ on specific theoretical points, like the criticisms of representative agents, leading European figures like Alan Kirman are quoted; 7/ the section 1.10 on the communication between ABM and data, authors like Railsback and Grimm, Edmonds, or, from economics, Fagiolo are also quoted. Thus, given the specific topic of the book (in which, I repeat, ABM is only one piece of a larger project), why is this insufficient (and "surprisingly uninformed")?

This said, your objection raise a more general problem. Indeed, your statement seems to complain about the fact that the European ABM community is not recognized enough within American sociology (using, or starting to use, simulation and ABM in particular). I think that you are right. From a bibliometric point of view, the nice analysis by Squazzoni and Casnici (JASSS, 2013, 16, 1, 20) seems to me a direct empirical proof of this fact.

My interpretation of this fact is simple. Leading figures within what you call the "European ABM research" are not able to speak to the core of sociology, many of them because they do not have the skills (they come from different disciplines) whereas others are simply not interested in doing so. The creation of specialized journals goes in that direction. This choice follows more the logic of building a new community than making efforts to penetrate the mainstream. This choice is perfectly wise and defendable but one of its possible downsides is precisely to reduce the probability that the mainstream is able to appreciate the quality and the relevance of the work done. The project of analytical sociology, at least as I understand it, is different. The goal is not to create a sub-community but to modify little by little the way descriptions and explanations are conceived in the sociological mainstream. One consequence of this understanding of analytical sociology is that one must be able to communicate and dialogue with the mainstream, and at the same time add something even marginally new. This is an additional motivation behind the architecture I decided to set for my book.

[D] (ECB –2/12/2014)

The simplest thing to say about this is that (again) I meant the book as a whole not specifically your contributions. It would be arrogant of me to expect more coverage of ABM but where it is raised the references do create this cumulative impression. For example on page 30, the first edition of Gilbert and Troitzsch predates all those references except Ferber – which is arguably MAS and therefore not always social especially back then – only quoting a much later book by Gilbert (and even the second edition of G+T was 2005) makes it look as if Gilbert comes very late to all this. Hedström cites no ABM at all (pp. 69-70) so I suppose he cannot be accused of anti-European bias, but it is not true that there are no ABM bearing on the modelling of rationality/MI which are relevant to what AS can do: http://link.springer.com/article/10.1007%2Fs11299-009-0060-7.) Wikström's ideas overlap significantly with Malleson *et al.* (http://www.sciencedirect.com/science/article/pii/S0198971509000787) but again no ABM citations at all. Kroneberg has missed an early "European" ABM specifically on variable rationality (http://jasss.soc.surrey.ac.uk/2/2/2.html). I will go on if you want … Of course, on a case-by-case basis you cannot expect perfect citation but cumulatively (as I said) the impression caused by non-citation and replication of standard AS community references (like Epstein and Axtell or Macy and Willer which are often American) is quite misleading. Citation is partly just what you happened to read but should also be things you *ought* to have read. (This book predates Epstein and Axtell by two years: http://www.amazon.co.uk/Simulating-Societies-Computer-Simulation-Phenomena/dp/1857280822/ but almost nobody gives precedence to its ideas just because the publisher was obscure.) This is not a new phenomenon in AS either. Hedström uses simulation in Hedstrom and Swedberg (though it is not listed in the index!) but cites only two other models (both American) and nothing relevant to simulation technique. He certainly didn't invent simulation and neither did the two people he cited in regard to his discussion! By contrast, Weiss and Sen (https://www7.informatik.tu-muenchen.de/~weissg/LNAI-1042/) predates Hedström and Swedberg by a couple of years as regards multi-agent learning of which his model is arguably an example (though this is not an ideal case because MAS/ABM were less well defined as areas then.)

(D) (GM –18/12/2014)

I see your point on the cumulative patterns of citations. However, the interpretation you gave these patterns is problematic because non-citation (as you admit yourself) can be intentional or not, and, when intentional, can express different things. First, as a preliminary remark, let me note that, from a cumulative point of view, you should be happy with my book because, even though concentrated in my chapter, citations of work from the ABM community (broadly understood) are by far more frequent than in previous classical references on AS. Second, it is unclear to me why you require specific contributors (like Hedström, Wikström or Kroneberg) to cite ABM work. Their chapters are not about ABM. I would see the critique as pertinent if these authors were building an ABM *without* referring to relevant ABM literature from the European-based community. But their chapter is not about this, thus why should they have quoted the work you mention? Third, how systematically people within the ABM field look for and quote papers from the mainstream sociology (and AS in particular)? If there is symmetry in non-citation behaviour I would interpret this as a mutual ignorance (in the sense of not knowing) rather than as an intentional behaviour based of lack of recognition of the relevance of research done in the other field. Finally, as to Gilbert, it seems to me that again you over-interpret my citation behavior. The reason I quote Gilbert 2007 is because I wanted to provide reference to a "more general treatment" specifically devoted to ABM whereas Gilbert and Troitzsch (1999 [2005]) also concerns other simulation techniques. If you are interested in seeing the place that I personally assign Nigel Gilbert in the field, you may have a look at my *Variables, Mechanisms and Simulations* (Revue Française de Sociologie, 2007) where, by the way, also Gilbert/Doran 1994 is quoted.

[D] (ECB –19/12/2014) Don't forget that this was only a passing comment in my review. Part of the reason it was a passing comment was that, as you say, I can't legitimately accuse specific authors of failing to cite what they should because I can't infer their motivations. However, I can make the point that cumulatively, the impression is misleading. I simply don't agree with the point that an article that is not "about" ABM does not need to cite ABM. Can you imagine making the claim that a qualitative article about divorce did not need to cite relevant statistical research? I am pleased with your book (just not as pleased as you are – see "minor points" below) but I'm afraid I'm not impressed that AS spent a long time ignoring ABM (even when it was plainly relevant particularly in terms of methodology) and is now suddenly recasting the method to suit its own vision still without careful credit to what ABM already knew. This is again a kind of unnecessary exclusion likely to cause hostility.

***************Thread E***************

(E) (GM –19/11/2014)

A fifth objection concerns the role that I assign "methodological individualism" and "social networks". If I understood correctly your criticism, you dislike posing these elements *a priori* as necessary features of a theoretical model. In this respect, you argue, ABM is more general than AS because "In ABM one just has to decide if such features are appropriate to modeling a particular domain and if not, leave them out". Here again I have the sense that you caricature a little my presentation.

First of all, I constantly speak of a "prototypical, generic structure of a generative model", which obviously means that the specific content of a model will depend on the specific *explananda* under scrutiny –this is also clear from my open definition of what a mechanism is, where the specific content of "entities, properties, and activities" is let unspecified and explicitly said depending on what is modeled. Second, with respect to methodological individualism, I stressed that several variants exist and argue in favor of a general version according to which structures and actions are dynamically related and constantly feedback in each other. If you combine this understanding of MI with my open definition of what an entity is, I do not see how you can argue that it is possible to separate an agent-based model from MI. In its most generic form, an ABM is anything but a series of dynamic loops between at least two levels of analysis. In essence, this is my definition of MI, thus I do not see in what sense this definition restrict AS compared to the "generative strategy in ABM". Third, with respect to "social networks", my section 1.8 is not exclusively about "social networks" but about "structural interdependency", which I define broadly as containing several forms of interdependence, some of which are network-mediated. Thus I did not suggest that social networks are the only form of interdependence AS focuses on, nor that every AS-inspired ABM must include social networks. Again, like with MI, if you appreciate my definition of interdependence, I do not see how you can argue that this element restrict AS compared to the "generative strategy in ABM".

[E] (ECB –2/12/2014)

I think, in a sense, your response here illustrates my concern. If it is so hard to establish what MI is (or the range of things it might be) it seems really risky to make it a core part of your approach. This is risky both strategically (why exclude potential allies for nothing more than an epistemological conjecture?) and empirically. (We do not want have to say a piece of research is not analytical because it deals with a phenomena we need to model collective representations to understand.) I certainly agree that ABM often incorporates MI but ABM neither says that this is what makes an ABM an ABM, nor does it say that it is technically necessary to make an ABM function. (By contrast to the way that RCT, for example, is obliged to insist on certain kinds of preferences for analytical reasons.) I suspect if we could clarify what a non-MI model would need to look like (and part of the problem with the existing debate is that it hasn't seemed to help with this) then ABM could probably build it. ABM is not creating a hostage to fortune as the expression has it (though it has plenty of other faults!)

(E) (GM –18/12/2014)

I agree that it is hard to orientate among the variety of forms of methodological individualism that were proposed by sociologists and philosophers. However, the variant I proposed is clearly defined and includes without any ambiguity "collective representations" and many other emergent supra-individual phenomena. My sense is that this definition may be much more consensual than usual more reductionist versions but, if you judge that this is still not the case, as I said earlier, to me it is not a concern that some do not recognize in the specific form of AS I like. And again I do not see why one should bother about "exclusion" as long as one accepts that a plurality of strategies exist to achieve analytical rigor (this is my case, even though equal analytical rigor does not necessarily mean equal explanatory dept). As to ABM flexibility for building models that escape traditional assumptions of narrow forms of MI, I agree. This was the sense of stressing on page 33 the capacity of ABM to implement mechanisms bridging several levels of abstractions.

[E] (ECB –19/12/2014) Again, this "defence" just amplifies my concern. I don't really mind how you (or AS) define MI. It does not hurt me. My point is just that it doesn't help AS and I say why in the review. But if your definition of MI includes "collective representations" then you might want to think about why it is so important to defend MI as a research strategy at all. You also might want to think about whether taking so many of my comments as unjustified and therefore to be resisted is actually part of the problem AS faces. As I said in the review ABM does not need to take positions on controversial issues to give it substance. We aren't a religion so we don't need a creed!

***************Thread F***************

(F) (GM –19/11/2014)

A sixth important point you raise is whether or not "traditional statistics can or can't be AS". Here the critique you address against the book is that "Manzo suggests not but what several contributors actually do and the apparent inconsistency is not addressed".

In this respect, let me first remark that the "inconsistency" arise from the fact that you ignore (or are dissatisfied with) the book's project (see point A above). Contributors that rely on statistical methods do not pretend to do AS along the lines I design in my introduction, and, for my part, I do not present them as doing AS. Thus, there is no "inconsistency" because chapters based only on statistics are explicitly and honestly presented as contributing to a specific aspect of the larger AS project I favor, namely how use creative data and statistics to describe *explananda* in a proper way.

This said, apart from the introductory chapter in which I explain why a statistical model cannot design a mechanism, thus, since AS is characterized by being about mechanism modeling, by definition, implying that statistics alone cannot be AS, it is not true that the "apparent inconsistency is not addressed". In the book's *Coda*, specifically on pages 421-422, I summarize the different ways in which different chapters deal with mechanism modeling and testing, and clearly deliver the reader with two *modus operandi*. Then, coherently with the spirit of the book, which, as I said above (see point B) is not "normative" and "imperialistic", I let the reader to appreciate what strategy is the most direct and powerful of the two.

[F] (ECB –2/12/2014)

It is not very helpful in a debate I know but I think I just don't agree with you here. Even if your book is intended to be raw material for future AS (rather than really AS itself) then the problem of whether statistics can be AS (or not) does not go away and needs resolution. From an ABM perspective (but also from a qualitative perspective) I think I could argue that there are things that statistics will never do and that quantitative approaches simply cannot be reconciled with certain aspects of other approaches. Agency would be the biggest single example. I'm afraid I think saying that the contributors don't necessarily agree with your view – or that I have misunderstood your aims – not only doesn't solve the problem (or address the contradiction) but also indirectly weakens the claim that there is a coherent AS approach. What is it about the statistical chapters that you include which makes them "AS type" statistics?

(F) (GM –18/12/2014)

You write "the problem of whether statistics can be AS (or not) does not go away and needs resolution". This problem is explicitly addressed in the section "Description" as well as on pp. 15-16. I explain why statistics alone cannot help directly design and study models of mechanisms. Thus statistics alone cannot be AS (as I defined it). However, Figure 1 gives a specific role statistics within (my understanding of) AS, namely to provide fine-grained and rigorous descriptions of data patterns/trends to be explained as well as (one possible form of) empirical calibration and validation of ABMs. Thus, since statistics is an important brick, it is perfectly coherent that some chapters of the book deal, are exclusively based on, this specific brick.

[F] (ECB –19/12/2014) As above, I fear we are arguing in a circle. IMO what you say in the introduction is not enough to deal with the contradictions raised by the chapter content and I try to illustrate that concern in the review with (for example) discussion of Wikstrom and Gonzalez-Bailon *et al.* If that is not enough for you then we must agree to differ but bear in mind that I have no incentive to be harsher to this book than I think it deserves. (I only damage my reputation with a "show off dismissive" or poorly justified review.)

***************Thread G***************

(G) (GM –19/11/2014)

According to my reading, the final objection that you address to the book is that it contributes to create a "self-regarding club rather than a self-critical field". You think that we should favor instead a perspective that "can bridge the gaps that exist between the different approaches". I must confess that I found this objection more surprising than convincing.

I wrote this book precisely to increase the dialogue between AS and other perspectives. Apart from my own introductory chapters, which builds on a variety of different sociological and methodological traditions in sociology and elsewhere (from pragmatism to socio-physics), each short introduction to each chapter was precisely written "In the spirit of knowledge accumulation, these chapter outlines also serve the purpose of connecting the chapter with previous programmatic manifestos of analytical sociology, as well as with other sociological approaches." (p. 40). The second part of the book's *Coda* is all about theoretical integration and summarizes the different approaches (mainly, in network analysis and mathematical sociology with which several chapter exchanged). Finally, please consider the first (long) paragraph on page 37 in which I argue in favor of the integration of several perspectives relying on different methods from qualitative interviews to ABM going thought experiments, which seems to me very similar to, not to say the same as, what you argue in the sentence before the last of your review.

(In passing, in this sentence, you also seem to object that the book contributes to organize the debates around "insubstantial epistemological or philosophical positions" rather than "actual research problems", which seems to me a little awkward given that this is the first book on analytical sociology in which the majority of chapters contains analysis of specific phenomena)

[G] (ECB –2/12/2014)

My concern here is partly technical and partly political (and it may be that I split these points badly in the review). As I said above I think I could argue why, in fact, existing styles of qualitative and quantitative research won't and can't be reconciled even by the framework of AS but might be (IMO) by ABM. (As you say, I will resist reading you my CV!) Thus one reason, I fear, why you haven't got examples of that kind of research (which ticks all the boxes in the AS framework at once) is that it simply can't be done within existing methodologies. (Time – or you – may prove me wrong!) Politically, as I already suggested, if your goal is to promote AS then the best work and the best workers need to be shown. If a piece of statistics is in the book not because it is better than other statistics or "more" AS but just because it belongs to someone who supports AS (or is a friend of someone who does) then the danger is that AS will just look like "what me and my friends say it is" even if that is not your intention. (And I thought it would look churlish to say it in the review but if you took away anyone who does not have a short network path connection to Nuffield College there would be very little analytical sociology left!) This risk is made even greater by your generosity in defining AS which does not allow others to know objectively what makes possible contributions to the field eligible or ineligible. To me, many of the chapters don't look distinctively AS at all. Do you get to outvote me just because you are the editor? Perhaps you are not completely impartial? (By contrast I have no trouble saying what is or isn't an ABM – because it is just a tool maybe – when reviewing for JASSS regardless of whether I happen to love or hate the actual research/researcher.)

(G) (GM –18/12/2014)

On the technical side, since ABM is a central piece of my AS definition (see chapter 1), if you think that ABM can contribute to integrate different approaches and kinds of data, you should accept that the kind of AS I propose should also in principle be capable of such integration. On the politically side, I agree. As I have said earlier, however, the difficulty here is that the best work, meaning papers that "tick all the boxes", to take your expression, exemplifying something that does not exist yet cannot be found easily. And, but I am repeating myself, contrary to what you suggested in the review and suggest again here, I did not present the "incomplete" chapters as "distinctively AS". Finally, I find empirically inaccurate your "network" argument. Ideas spread thought teaching and scientific collaborations, thus obviously research centers in which leading figures of a given perspective are (or were) active are more likely to produce students/scholars interested in these ideas (would not this perhaps be true for the ABM field either?). This said, a better knowledge of the history and current developments of analytical sociology would help to see how its frontiers extend far beyond (temporally and spatially) the Nuffield College you seem to focus on (in this respect, my article *Analytical Sociology and Its Critics* may help).

***************Minor Points***************

(ECB –2/12/2014) Finally, I notice in passing that you don't want to criticise (or challenge) any of the nice things I say about the book! Confirmation bias?

(GM –18/12/2014) I did not find such things! The only nice things I found concerned two remarks on my seriousness in the editorial work. I acknowledged this in my original e-mail (namely, I wrote "I am also glad that you regarded the way I edit the volume professional and serious, and would like to thank you for acknowledging this twice in the review"). Thus, I do not think my reading was driven by a confirmation bias.

(ECB –19/12/2014) It is important that there is no confusion between not saying nice things and not saying the things you would like me to say! Here are the positive things I say: "my impression is that some excellent research is being done", "nearly all contributions to this book are well ahead of far too much sociology in terms of clarity and intellectual value", "The point is not that these are weak or uninteresting chapters, very far from it", "the article by González-Bailon et al. contains excellent data and intriguing analysis", "Mitschele's carefully argued article on witch trials", "Apart from the direct interest of the excellent simulation contributions to JASSS readers", "Taken simply as a set of chapters, this collection is considerably better than average (with JASSS readers being likely to benefit from reading many chapters and at least skimming most). It is also well presented and organized and the editor has clearly taken his job very seriously with good results." (I may even have missed a couple.)

(GM –19/11/2014) Why the book is quoted as "WileyBlackwell: Hoboken, NJ"?

(ECB –2/12/2014) To be honest I don't know. I can't remember now if I provided these details (though I doubt it as my book clearly says Chichester: Wiley) or if they came from the JASSS reviewing database. Flam?

(GM –19/11/2014) Hedström and Swedberg's book on Social Mechanisms was published in 1998, not in 1995.

(ECB –2/12/2014) Sorry. My mistake. I had in my mind lots of dates around that time regarded who preceded who in publication and my fingers must have slipped.