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Utku Balaban

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*Commentary on secrets of economics
editors: an unintended ethnography
of economics*

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An unintended ethnography

There is a discipline in social sciences that claims to search and find the optimal solutions whenever there are scarce resources to fight over. [1] That discipline is economics and its claim for this capacity merged at some point in the 20th century with another and even bolder one: to be able to make (correct) predictions about economic phenomena. After these two self-declared claims gradually gained some credibility within the larger social science community for various reasons, economics began to be recognized as a discipline that fixes normative problems without a normative stance, unlike other social science disciplines such as sociology, political science, or anthropology. This century-long PR campaign established economics' political and methodological domination in the realm of social sciences.

When I was first asked to comment on the edited volume entitled *Secrets of Economics Editors*, I had no intention whatsoever to write the paragraph above. In fact, my original plan was to limit the commentary to a dull summary of the major themes tackled in the book. Having read the piece, however, I changed my mind and decided to use this opportunity to discuss the generic problems of academic publishing in economics. The reason for the change of the original plan is the fact that the book does a marvelous job in documenting those problems with the testimonies of the "gatekeepers" of the discipline (i.e. the editors) in an unintentionally ethnographic way.

Themes

As the first order of business, I would like to briefly inform the reader about the structure and content of the volume before moving on to the ideas in the piece that inspired me. The book consists of twenty-four short contributions in addition to the introduction. Each chapter reflects the experiences and opinions of the editors of academic journals serving for different audiences and/or sub-disciplines. Most of the contributions do the same job, which is to inform the potential authors, referees, and editors about what to do and what not to do when it comes to publishing, refereeing, and editing.

In fact, the overlapping issues such as how to successfully navigate the manuscript through the review process (if you are an author) or how to process a large number of manuscripts (if you are an editor) take too much space. The repetitiveness, thus, makes the piece a little bit tedious to read. A related problem is that contributions are organized in accordance with the journal's area of specialization, rather than what the contributor tells the readers about. This organizational choice makes it more difficult for the reader to catch contributors' original points. The section on "economics of sports" is a good example for the problem. There is only one contribution concerning this field. This piece by Kahane gives interesting insights about how economics of sports is a valuable field of study, yet the contribution is mostly about how the journal got started and survived; an important issue covered by other contributions in separate sections of the book. It seems the only condition that pieces of a similar content come together in the same section is that they do not fit anywhere else in terms of the field of the editor's journal. For instance, two pieces on plagiarism are given space next to each other in last section of the book, even though those editors run journals in different fields. In short, a thematic organization of the contributions in terms of the emphasis points in content could have made it easier for the contributors to deliver their messages. Editing a book on the editors is apparently not a cakewalk.

Nevertheless, styles differ of a great variety from one piece to another. Furthermore, answers for similar questions differ from each other as well. For instance, should the editor take the responsibility for the final decision or should the coeditors take a decisive role in the process? Or, to what extent should an editor rely on the comments of the referees? These are tough questions to deal with. Thus, different perspectives certainly help the reader to see the pros and cons of contrasting strategies.

Given that there are more than twenty contributions, it would be a futile effort to summarize each chapter in the piece. Thus, even though it does not do justice to individual contributions covering a great deal of subjects about the notion and practice of editorship, I will take the liberty to categorize the major themes discussed in the book in order to provide an overview for potential readers. A relatively large number of contributions focus on what I would call 'the technical issues.' A second theme discussed in a fewer number of contributions is 'the epistemological challenges.' The third overarching theme is 'the embedded tensions' pertinent to the academia and its relations with the policymaking agencies.

'Technical issues' are the quintessential problems faced by any editor running/coordinating an academic journal. Thus, the book deserves the attention of not only the economics community but also the social science community in general. One of such issues is the challenge for any editor to manage a high number of manuscripts without sacrificing much of his or her other academic responsibilities. Apparently, some of the contributors are more successful in this endeavor than the others, sometimes just because they are more organized and sometimes because their institution provides more support concerning financial and professional matters. Another technical issue is to find the 'optimal' solutions for organizational problems. Some of the editors work with a smaller team. Some of them assume most of the responsibility for the decision-making process. There are also interesting insights about how to reach, communicate, and sometimes 'handle' the referees. Even though most of the contributing editors agree that most of the authors 'behave properly', very few of them avoid giving little space for the exceptionally distasteful experiences with the 'problem personas' and many of them give their advice about how to act amidst such confrontations. A number of contributions share very useful details about how to start, maintain, or revive an academic journal.

Contributors' discussion about those 'technical issues' taught me a great deal about the practicalities of the editorship. Currently, I serve as an editorial board member of a prominent social science journal in Turkey (*SBF Dergisi*) and face similar conundrums discussed in the book. Furthermore, any scholar eager to get published in a major economics journal will be very well informed by this piece about how a manuscript is navigated in twenty-three different journals. Thus, contributions about the 'technical issues' are valuable, not only because they provide practical guidance about writing, refereeing, and editing for a major economics journal, but also because potential authors, referees, and editors get to know more of the actual dynamics pertinent to the journals covered in the book.

The second major theme, which a smaller number of the contributors point to, is what I call 'the epistemological challenges.' By 'epistemological challenges', I mean the questions to be satisfactorily answered in order to justify what the economists do as a worthy academic activity. For instance, what role should the model-building play in the discipline and how much weight should the articles focusing on this enterprise be given in an academic journal vis-à-vis articles presenting new empirical work? How 'opinionated' should an editor be when it comes to making the final call? (For instance, McAfee in his *Edifying Editing* argues that being opinionated is a prerequisite for becoming a successful editor.) What should we understand by 'mainstream economics' and how should the 'non-mainstream' manuscripts be treated? What should the 'optimal' role of quantitative assessment be in academic articles? Or, is such a discussion even relevant in the field of economics?

Even though I regard the epistemological challenges as a major theme tackled in this piece, I must say, I am not surprised but disappointed by the minuscule number of contributions that deal with such questions. Few contributors of the book choose to discuss the epistemological grounds of their discipline, whereas most others sound rather pleased with their disinterest in this (in my opinion) very important part of what we do as social scientists. Nevertheless, those, who tackle such questions, do an excellent job to make the readers think further about what turns an economics article into an intellectual contribution (for instance, Adkisson's, Bromley's, and Hodgson's articles).

The third major issue is about the wide spectrum of rifts that any editor faces while running a journal. Under this heading, I skip the parts in the book about the tensions between editors and authors, since I believe they pertain to the very core of the editorship. Three other general problems amount to what I call 'the embedded tensions.'

The first one takes place between the editorial team and the company (or the association) that controls the journal. In many cases, 'owners' make strategic decisions that contradict with the management style of the editors. In some cases, editors managed to bypass, rectify, or modify such moves, while some others were apparently given no choice but resignation. Such cases are particularly illustrative of a critically important problem embedded in the business of academic publishing: most editors make an assessment of the opportunity costs brought about by their editorial responsibilities. In many cases, those costs are too high to fight for the principles and potential rewards are too small to give up on other academic

aspirations and duties. Thus, it looks like many editors simply choose to quit their editorial position when they face such tensions with the owners.

The second tension matures between different 'tiers' of academic journals. The tone of the editors of the top-tier journals is much more authoritative than the editors of 'low-ranking' journals. Those in the former group proudly emphasize how many manuscripts they 'have to' reject each year and why they turned the once exceptional option of desk rejection into a regular practice. These differences in management styles cannot be taken as distinct phenomena isolated from each other. Many of those rejected manuscripts eventually find their way to the 'low-ranking' journals. Editors of the top-tier journals do not care about the successive phases of the life course of those rejected manuscripts, because they are no longer their business. In fact, the primary factor that distinguishes between good and not-so-good journals becomes the number of rejected manuscripts, rather than the intellectual quality of the accepted papers. Under these circumstances, quantitative measures such as publication citation metrics are left as the only reliable reference to assess the intellectual value and rigor of the published pieces. Furthermore, high rates of rejections by the editors of the top-tier journals help to reproduce the ranking structure among the journals. It seems, there is a vicious cycle here and editors can potentially play a crucial role in getting the academic community out of it.

The third 'embedded tension' is about the policy consequences of academic work. As mentioned earlier, economics is at the top of the academic hierarchy in social sciences. Thus, no matter how abstract the actual outcome is, articles within the field of economics usually have a bigger impact on the actual policymaking than the material in other disciplines of social science.

This means power and power comes with dilemmas: while the unquestionable hegemony of economics in social sciences entrusts a great deal of influence in the hands of the economists, it also makes them more vulnerable to the nonacademic power struggles. In this regard, Bromley's experience in the aftermath of the *Exxon Valdez* oil spill case in 1989 is illustrative of how such exogenous pressure potentially retards the conventional ways to process the manuscript in a 'norm-free', 'scientific' or 'non-ideological' way (Pick whichever term you like. I long ago personally discredited the relevance of each one of these adjectives when it comes to saying anything about human beings or the society in general). In this particular case, pressure by the interest groups (of course, mostly by oil companies) manipulated the content of manuscripts, because most of the members of this specialized academic community were already financially and/or professionally

connected to one of the parties involved in the actual conflict. Moreover, referees could not be trusted either, since they came from the same pool as the authors. In fact, they had vested interests in deciding what was to be published and what not. Even though few contributors in the piece point to this problem, I believe such issues pertain to other fields of economics as well; in particular to those dealing with the financial, industrial, and regional aspects of policymaking.

Ideas

Secrets of Economics Editors is a valuable piece, not just because it illustrates the centrality of these (and other) themes for the academic publishing in economics, but also because of the way those themes are covered. The book's title is carefully chosen: we are talking about 'secrets', because economists do not like talking about the problems in their discipline as much as, for instance, anthropologists do. I believe the primary reason for this reluctance is that economics is the king of the jungle of social sciences.

There is an 'economics' for almost everything including democracy (Downs 1957). One reason for the contagion of the dominant epistemological stance of economics to other disciplines is the strong belief in what we can call 'optimalism': the notion that all social problems can be fixed by finding an optimal point between conflicting parties or diverging choices. Apparently some believe, there is no other way but to depend on an 'optimalist' way of thinking to struggle with the omnipresent challenges brought about by the chaotic market relations in a capitalist world. I do not share this belief, as I think the core of most of our social problems in the contemporary society derives from irresolvable conflicts, the primary of which takes place between social classes.

Nevertheless, some may also argue that we need this way of thinking to be represented in economics as well as in other disciplines, since it looks like it is one of the common ways of comprehending the social phenomena. I am just fine with this stance, unless the notion of 'optimalism' dominates other ways of thinking both in economics and other disciplines. However, this is exactly what is happening in contemporary academia. Some resist it and become 'homeless' in the academic world, as Hodgson complains in the book with respect to his personal experience. Some others choose to embrace it and jump on the bandwagon.

This is certainly not the right place to pursue a comprehensive discussion about the production of knowledge in economics, yet still a particular consequence of this problem is closely linked to the way academic publishing is organized in economics: colonization of other disciplines by economics seems to put a major burden on the economists as much as it empowers them. Power comes with responsibility; the responsibility for legitimizing what you do and how you do it. With such an immense power, that burden grows to the degree that economists quit talking about the substance, relevance, and consequences of their intellectual efforts and output.

This piece is, thus, particularly valuable, because editors of major economics journals are asked if the current system of publishing is working. As the contributions are basically personal testimonies with apparently limited intervention (e.g. two typos caught my eye on pages 55 and 330), the observer's effect (i.e. editors of this volume) on the subject (i.e. the contributing editors) is also limited. In fact, the book can be regarded as a surprisingly transparent ethnographic piece that tells about 'the backstage' of one of the key socialization processes among the economists, if we are to use Erving Goffman's terminology (1959). Thus, I would like to present this piece as an unintentionally ethnographic work that illustrates some of the internal conflicts within economics and among economists.

And what we see may not be pleasant at all. First of all, economics editors lack self-confidence. As noted above, editors of the top-tier journals are proud of the process, not the actual product. The terms 'Type 1 and Type 2 errors' are commonly used in these contributions. In fact, these editors put themselves in the position of 'observers' and treat the pool of manuscripts as a 'population' amenable to statistical analysis. Testimonies reveal that some of manuscripts they rejected gained later so much popularity that they helped their authors to become Nobel laureates. However, these are regarded as 'deviant cases' (or Type 1 errors) that somehow illustrate how the current system actually operates with a great success. In fact, stories of such 'cases' are mentioned in order to justify (minor and negligible) 'statistical errors' in the process. Editors simply argue that they answer to 'the higher authority' of statistics in order to keep themselves immune to the criticism that they fail to fulfill their intellectual duty to distinguish between good and not-so-good manuscripts.

The same problem of the lack of self-confidence applies to the editors of the 'low-ranking' journals as well but only more seriously. For this 'population', the challenge is pertinent to not just the overarching epistemological weaknesses of the discipline but also their status in the hierarchy in general. There is a visible effort in the related contributions to justify what their sub-discipline does does

actually matter. However, even the most critical piece in the volume (i.e. Hodgson's) avoids investigating the reasons why economics as an intellectual discipline suffers from such rigid stratification. In his opinion, if non-mainstream contributions do not rank highly in the publication citation metrics, that is the problem of those contributions, not the way the discipline was historically established as a hierarchy-prone field of study.

Embarking on the same idea, it was surprising for me to see how the attempts to replicate the results of the academic papers were systematically rejected by the economics community. Dewald and Anderson's experience is in this regard heuristic: their project to replicate the results of the manuscripts submitted to their journal was a short-lived one and, as far as I understand, only half of the papers published in their journal could pass the replication process. I certainly do not imply any of those papers lacks rigor, yet such failing attempts, it seems, put a cloud of doubt on the alleged objectivity of the economics as a 'science.' Just like any other academic discipline, economics suffers from problems about rigor and objectivity: no more, no less... However, I must emphasize that none of the contributors bother to pursue such a discussion on these grounds.

Another insightful observation is about the lack of a systematic discussion about where economics as a discipline is heading to. Adkisson's contribution is in this regard exceptional, because he is at pains to convince the reader that 'dry holes' (i.e. articles that do not get a significant number of citations) may actually serve well both for the discipline and policymaking purposes. Most of the other related remarks are about the organizational issues such as the prospective role of online publishing, rather than the academic and nonacademic factors that affect the content published in those journals. The unwillingness to discuss the future of the discipline seems to derive from the misguided position that overlaps the question of what is good for the journal with the question of what is good for the discipline. Accordingly, the reluctance of potential authors, many of whom are young scholars, to bring about a novel and bold approach is regarded as a matter of personal flaws of those scholars. The question whether the current structure of publishing (and, hence, job seeking and promotion) may lead to that reluctance is not posed virtually in any of these contributions.

Similar questions ensue: does the current structure of publishing boost creativity? How can the current structure be transformed in order to value the quality of the publications, rather than the quantity? How can the dominant 'mainstream' theories be rendered more vulnerable to the intellectual attacks by novices and heterodox

economists? In overall terms, even the terminology (e.g. the aforementioned 'Type 1 and 2 errors' rhetoric) points to a non-democratic discursive area. Nevertheless, I must admit that the community of economics editors is more diverse in their opinions about how to run a journal than I imagined. However, it is still difficult to regard this diversity as a sign of a meritocratic academic environment. It looks like differing opinions mainly result from the hierarchical structure of the discipline. Two broad axes situate individual editors in different positions. The first axis is about the sub-discipline of the journal. Editors of the journals specialized in more popular areas such as finance and monetary policy differ from the editors in less popular areas in opinion. The second axis is about where the journal stands in the ranking system.

As I noted earlier, my overall impression is that editors of the journals at the top of the hierarchy along with these two axes act in a more decisive or bluntly more authoritarian way than the rest of 'the population.' To some degree, this is a both understandable and just attitude: at the end of the day, they are given an important task and they are responsible for the outcome. Thus, to limit the initiative of the coeditors and other members of the team enables them to make quick decisions. Similarly, fast desk rejections do not mean an unjust treatment for potential authors, who are expected to accept 'the rules of the game' by sending their manuscript to those popular journals. As there are other outlets for their manuscripts, these authors do not have much to complain about, even when they receive a rejection letter without being given the chance for double-blind review.

However, the question I posed earlier still lingers: how do the journals then emerge as agents to provoke ideas novel for the discipline rather than the ideas that the editors see fit for their journal? Contributions in the book reveal that editors of the popular journals are satisfied with the role of gatekeeper and spend a substantial effort to justify the significance of this role. Personally, I am not convinced by this attitude. I believe editors of such journals can and should do a better job, since they enjoy compelling power to shape the entire discipline by defining what is mainstream and what is not.

Even though this may not immediately sound a related issue to the potential audience of this commentary, I believe the fact that a considerable number of contributions complain about the linguistic inadequacy of the scholars, who are not native speakers in English, is particularly illustrative of this problem. For instance, Hodgson's aforementioned contribution is admirable in terms of its critique of the publication policies of the mainstream journals. Hodgson calls himself a

'homeless', because he could not find an appropriate teaching position in economics departments and had to teach at business schools. I personally sympathize with his decision about this conundrum between intellectual ideals and practical realities.

However, when it comes to the non-native authors, his attitude begins to look much more like those, who do not appreciate his own work. In his opinion, countries that accept 'that English has become the dominant global language... must develop and institutionalize academic competence in the English language.' This suggestion comes from a scholar, who has 'never written an academic article in a language other than English.' No matter how disturbing those two pages Hodgson spends to make his point are or how he justifies aligning his criticisms for the mainstream economics with this unrealistic expectation, I still respect for this point for the sake of its honesty: many other contributions in this book make exactly the same point time and again, albeit with a much quieter and subtler tone.

Once again, the problem here, I believe, is not fairness: as Hodgson points out, those, who decline the English-dominated structure, always have the opportunity to publish their work in a different language. They pay the price for it, yet that is apparently not the problem of the economics editors of the journals publishing articles in English.

That is fine within the context of argumentation (and let's call it fair for a moment), yet the structural problem cannot be resolved this way: apparently, the number of manuscripts by non-native authors has been continuously growing for the past decades. Some of them may have a good point. As these authors put extra effort into submitting their manuscript to an 'English only' journal, that personal decision is also probably an important one for them. Taking all of these factors into account, the question becomes more of what the contributors of this book did to level the playing field during their tenure. Apparently nothing, because none of the editors, who emphasize this problem, mention any meaningful attempt on their part to ease the process for those potential authors. They are rather comfortable with putting all the blame on those (plausibly talented) economists, whose major deficiency may be not to excel in their academic field but to fail to overcome the language barrier. The most disturbing part is that economics is probably the only discipline among social sciences that requires minimal linguistic skills thanks to the increasing significance of the quantitative research in recent years. Given that some of the editors served for their journals for decades, such a careless attitude becomes even less tolerable. In short, it is more likely for the manuscripts by non-native scholars to end up as 'Type 1 errors' and more 'Type 1 errors' mean fewer new ideas.

To sum up, my general impression from the book is that most of the economics editors see their journal as an organizational structure, rather than an institution. Editors see it that way and want to keep it that way. The reluctance to help the non-native speakers, thus, illustrates a vivid symptom of the self-perception of the economics editors with regard to their editorial responsibilities. It seems that they see moving their journal up in the hierarchical structure as a much more important task than opening up the discipline to different voices regardless of their theoretical approach or geographical origin. Thus, they safely keep themselves immune to the criticisms about the lack of intellectual diversity in the discipline, since such issues are 'not their problem.' Consequently, many economics journals fail to serve as agents that influence the long-term transformation of the discipline and encourage the intellectual confrontations along with a well-informed set of epistemological choices.

In my opinion, rigor should be a criterion not just applicable to individual articles or books, but also relevant for the entire discipline. And rigor in this sense means the falsifiability of the perspectives (not just individual and isolated arguments), which can be realized only if the diversity of perspectives is assured within the discipline. In this regard, *Secrets of Economics Editors* is a heuristic contribution that illustrates the strengths and weaknesses not only in the academic publishing process but also in the intellectual field of economics. I am not sure if economists will be able to make good use of this book given its ethnographic content, but I hope social scientists in other disciplines will...

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Endnotes

[1] Szenberg, Michael and Lall Ramrattan (eds) (2014), *Secrets of Economics Editors*, Cambridge, MA and London, England: The MIT Press.

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Utku Balaban is a faculty member at the Department of Labor Economics and Industrial Relations, Faculty of Political Science, Ankara University (Turkey) (balaban@ankara.edu.tr).