

On Struggle and Competition in Scientific Fields

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Abstract

What is the difference between struggling for achievements and competing for success? What is the effect of competitions on a scientific field? What are the specific implications on TOC? In this opinionated essay, I address these questions and related issues, while rooting the discussion in an appropriate theoretical framework.

On the style of this essay. This opinionated essay refers to the sociology of scientific fields, and specifically to the current state of the Theory of Computation (TOC). It offers little novelty (if any at all)¹ with respect to the sociology of science, and is directed to TOC readers. These facts beg the question of style of exposition. On the one hand, it seems more adequate to use a sociological style (which fits the actual contents of the essay), but on the other hand it seems better to use a style that is more familiar to the intended TOC reader. I faced the same dilemma when writing a previous essay of a similar nature (see Note 43), and my previous choice to stay away from the familiar TOC style seems to have caused some misunderstanding. In taking the opposite choice here (especially in Section 2.1), I hope to generate less misunderstanding (and maybe also less disagreement).²

Regarding my footnotes. This essay contains even more footnote text than my typical technical papers (which are often criticized for that feature). All these footnotes can be skipped in reading. They typically provide either further justification (or clarification) for the main text or a wider perspective.

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¹I do not know the sociological literature well enough to evaluate the novelty of the current essay. My impression is that its main contribution is in the concrete treatment of the notion of competition (including all parameters associated with this notion). In particular, although Bourdieu discusses competitions at great length (cf., Note 6), as far as I could see, he does not provide a clear definition of this term (although he obviously distinguishes it from struggle). In fact, this impression was the starting point of the current essay.

²But if less misunderstanding would lead to more disagreement, then I'm OK with this, since I prefer disagreement over misunderstanding.

1 Introduction

The notions of struggle and competition are often confused. Ditto regarding achievement and success. Of course, my issue is not with the semantics of (the colloquial meaning of) these words, but rather with fundamentally different situations which can be identified by referring to these words.

By *struggle* I mean an inherent conflict between different people who attempt to *achieve* various goals and positions relative to a given setting.³ That is, the achievements determine the outcome of the struggle, and the focus of this situation is on the achievements. In contrast, by *competitions* I mean artificial constructs that are defined on top of the basic setting, while not being inherent to it, and *success* refers to winning these competitions. That is, success is determined by the outcome of the competition, and the focus of this situation is on the competition. (Thus, while the main agent of an achievement is the set of workers that accomplish it, the main agent in a competition is the set of judges that determine its outcome.)⁴

Of course, once competitions are introduced into a give setting, the setting changes; that is, a new setting emerges (or, indeed, is constructed) in which these competitions are an inherent part. Still, in some cases – most notably in scientific fields, one may articulate in what sense the original (or basic) setting is better than the modified setting (i.e., the setting modified by competitions). These issues as well as related ones are the topic of the current essay.

The core of the essay is Section 2.1, which provides a theoretical framework in which all these notions are discussed. This framework is used in Sections 2.2 and 2.3, which revisit familiar issues such as the evolution of the FOCS/STOC conferences, the effects of awards, and why is excessive competition bad. In particular, I trace several negative social phenomena in TOC to the growing dominance of various competitions in TOC. In Section 3, I discuss the possibility of reversing the course of this evolution and reducing the dominance of competitions in TOC. (Indeed, I have expressed similar opinions regarding the evolution of FOCS/STOC and awards in the past, but I feel that the framework presented in Section 2.1 provides a better articulation of these opinions as well as a wider perspective on them.)⁵

My analysis is actually an interpretation (or an application) of Bourdieu’s general theory of fields and his specific theory of scientific fields.⁶ Loosely speaking, a field is a relatively autonomous part of the general society, defined by specific interests and activities, and having its own structure including values, norms, hierarchy, and internal struggles over positions in this hierarchy. This hierarchy is reflected in the field’s symbolic capital (e.g., prestige and positions held). What makes the field relatively autonomous is that its structure and struggles are not determined by positions or other aspects of the general society. In particular, the conversion between symbolic capital and

³My emphasis in the term “struggle” is on a great investment of effort and energy towards achieving some goals. The struggle does not necessarily involve a direct interaction between people, let alone an interaction of a confrontational nature.

⁴Hence, I think it is inappropriate to congratulate a person for winning a competition (although it may be adequate to express joy at the benefits that the winner may enjoy). As for congratulations, they may be due to the judges (for making a good decision). I first heard this opinion from Leonid Levin.

⁵Indeed, my first, initial, and most important goal in writing this essay is to clarify to myself and to other interested readers a few issues that are quite central to our professional life. My hope that I may contribute to a change in hearts, and then to a change in reality, only comes second. In any case, I am not rushing to the conclusions; this essay is mostly a contemplation of the relevant issues.

⁶The general theory of fields occupied many of Bourdieu’s works (cf., e.g. *Questions de Sociologie*, Minuit, 1981). For an analysis of scientific fields, see his paper “The specificity of the scientific field and the social conditions of the progress of reason” in *Soc. Sci. Inform.*, 14 (6), pp. 19-47, 1975. For a brief review of some aspects of these theories, see the Appendix. Regarding the questionable novelty of my own essay, see Note 1.

economic capital is far from immediate (although it is not impossible).

The analysis of a field starts with the identification of its products and its clients. What is specific to scientific fields is that the clients for the field's products are typically the producers themselves. This has far reaching consequences for the analysis of the field, and is the reason that the analysis of scientific field deserve special attention from a sociological point of view.

2 The Theoretical Treatment

In accordance with scientific tradition, terms that are extensively used in this essay are explicitly defined before their first essential use. Specifically, these terms are defined in sentences in which they appear in (light) bold-face. I wish to stress that these definitions, rather than the colloquial meaning of these terms, are what matters. Nevertheless (and in accordance with standard practice), when selecting specific terms as targets of my definitions, I did try to evoke their colloquial meaning and I do claim that the defined meaning reflects the colloquial meaning (or at least, a reasonable understanding of the colloquial meaning).

2.1 Basic notions: achievements, importance, struggle, and competitions

I will start with an attempt to clarify some basic notions. The first step is the identification of the products of a scientific field and their clients: The field's products are scientific achievements, and their clients are the scientists working in the field. The tight connection between this type of products and their clients is embedded in the definition of scientific achievements.

Achievements: A (scientific) achievement is a *scientific product that modifies and/or affects the views and/or activities of other people* (typically, the views and/or activities of other scientists). This achievement (typically presented in a scientific work) may be a conceptual framework, a technique, or a result; and its effect on other scientists may be direct (i.e., by explicit use) or indirect (i.e., by an implicit use or influence).⁷

Indeed, the above definition (of a scientific achievement) does not refer to “personal accomplishments” (in the colloquial sense). The latter term may refer to two different situations (or cases). The first case is a scientist's success in advancing his/her own state of knowledge, while not advancing the scientific community's knowledge (e.g., understanding something that others already know). This is certainly a personal accomplishment, but it does not constitute a scientific achievement. The second case refers to the personal aspect of a scientific achievement; that is, the fact that this achievement is attributed to a particular person. In this case, a scientific achievement has a personal impact (i.e., assigning a specific person the credit for this achievement).

Importance: The (scientific) importance of a (scientific) achievement *is proportional to its influence on subsequent research in the field*.⁸ Recall that the influence of an achievement may be direct

⁷By **implicit use** I mean mainly the transitive closure of explicit use, whereas by **implicit influence** I wish to account for a wide range of possible ways in which a scientific achievement may influence subsequent research. These include the modification of the set (or the hierarchy) of problems of the field or the modification of the way these problems are formulated, understood and/or addressed.

⁸The achievement's influence on the corresponding technology is also “important” but this importance refers to a different field (i.e., the field of the corresponding technology). Thus, when referring to a scientific field and to an achievement that has an influence on a corresponding technology, I count towards *importance in the scientific field* only the achievement's influence on subsequent research in the scientific field (e.g., follow-up scientific works that either are aimed at improving some aspects of the technological application or are merely informed by the

and/or indirect, where indirect influence may be even more significant than direct one, although indirect influence is typically harder to trace. Hence, *both the direct and indirect influences are considered towards evaluating the importance of the achievement.*

The importance of an achievement refers to a specific time, and it never decreases with time (even when the related research direction is later proved to be non-fruitful).⁹ Although the notion of importance is well-defined (theoretically), evaluating importance (in practice) is highly non-trivial.¹⁰ Jumping ahead, let me mention that citation counts are poor measures of importance, not only due to the fundamental problems associated with any “statistical filter” (to be discussed below),¹¹ but also because (1) they do not account for indirect influence, and (2) they do not distinguish essential influences from inessential ones.

Struggle in autonomous fields: A scientific field is defined by specific interests and activities, which include an interest in objective truth regarding a specific type of problems and a specific form of rational practices that may be used towards addressing these problems. As any other social field, each scientific field has its own structure, which includes values, norms, hierarchy, and patterns of interactions and relations among its participants. A key aspect of these interactions and relations is the *struggle over positions* in the field, which in (autonomous) scientific fields is determined by the *struggle over scientific achievements*, where this determination is explained below.

The aforementioned positions may be abstract (e.g., prestige) or concrete (e.g., a faculty position), often both types are linked, and may be conceptualized as **symbolic capital** (specific to the field). By **struggle** I mean both *the great investment of effort towards achieving some goals* and the (possibly implicit) *conflict that arises between different people who try to achieve the same goals* (or related goals). In the context of scientific fields, typically, this struggle does not involve a direct interaction between the participants of the field, let alone an interaction of a confrontational nature.¹²

A scientific field is called **autonomous** if its internal dynamics are determined by itself and are not affected by any external force. Of course, the autonomous field is an ideal, which never exists in reality, but relatively autonomous fields do exist. Indeed, the level of autonomy enjoyed by a specific field is a key parameter in the analysis of the field.

In an autonomous scientific field, *the struggle over symbolic capital is determined by and actually manifested in the struggle over scientific achievements.* In fact, in this case, these two struggles are

demonstration of the technological relevance of this line of research). This does not mean that scientific importance (i.e., importance w.r.t the scientific field) is more “important” than technological importance; they are incomparable since they refer to different fields (whereas I only defined importance w.r.t a fixed field). Likewise, there is no way to compare importance to TOC with importance to, say, Mathematics or Biology. (Of course, one may suggest to define importance w.r.t the society at large, which will allow to make comparisons across different fields. But such a definition would be irrelevant to the internal functioning of an autonomous field.)

⁹The fact that an achievement was useful in the past is not eliminated by the fact that the directions of research that used it were proven to lead to a dead-end (i.e., are not useful). In other words, the history of the field is always present in it. On the other hand, an achievement that is useful to directions of research that continue to flourish (i.e., are useful) grows in importance with time (because it gets used, at least, indirectly by more and more works).

¹⁰Needless to say, evaluating the future importance of an achievement (i.e., evaluating the potential importance of an achievement) is even harder.

¹¹Specifically, see the subsequent discussions regarding the distorting effects of competitions (and, especially, of secondary competitions).

¹²Typically, each group of participants is struggling for (i.e., investing effort towards) some achievements, and the existence of competing attempts is of secondary importance. However, once an achievement is obtained it may confront other achievements, which may be conceptualized as a struggle between these achievements (or among groups making the same achievement), where most typically this is a struggle over relative importance (or over credit for the said achievement).

actually two faces of a single struggle, because the symbolic capital is merely the objectivization (or materialization) of the achievements. These assertions are justified by referring to the specific nature of a scientific field – the fact that the clients for the scientific products (i.e., achievements) are the other (competing) producers.¹³ The point is that the latter are going to endow symbolic capital only for products that are useful to them, and they cannot avoid endowing symbolic capital (at least implicitly) to products that they use. Thus, the evaluation of a scientific product (which may be viewed as its “exchange value” as determined in the field) is intrinsically linked to its actual use (i.e., its “use value”).¹⁴ That is, what the research community “thinks” of a scientific work (i.e., the amount of symbolic capital assigned to this work) perfectly reflects the work’s actual importance (i.e., its influence).

This analysis assumes that the field is autonomous; that is, that it is not affected by the society at large. In such a case, the evaluation is non-separated from the use (i.e., the credit is non-separated from the benefit to others). The same practically applies for fields that are practically autonomous.

The aforementioned coupling of the evaluation of a scientific product (i.e., the symbolic capital assigned to it) with its actual importance is less tight when the field is less autonomous. Typically, external influences will cause a separation between the process of evaluation and the usefulness of the product; this is best reflected in competitions (to be defined below).

Competitions: Loosely speaking, competitions are artificial evaluation processes, which are typically introduced in order to address considerations that are external to the field (i.e., to the interests and activities that define the field).¹⁵ Competitions are *unavoidable* to some extent (i.e., when used for the allocation of limited resources), but exist beyond this necessity in a form that may be called *superfluous*.¹⁶ When a field is successful (i.e., is perceived so from the outside) both types of competition are intensified: The *unavoidable* competition intensifies because there is a greater interest in joining the field, whereas typically the growth in the number of people who wish to join the field exceeds the growth in the field’s resources. The *superfluous* competition intensifies because external parties, who have no real interest in the field and/or have a much greater interest in aspects external to the field, become interested in the internal hierarchy of the field and in particular in the outcome of its internal struggles (i.e., the distribution of its symbolic capital).

The simplest form of competitions are *single-winner* competitions, where a *single-winner competition* is a *mapping from a sequence of collections of scientific achievements* (and/or perceived potentials of obtaining such achievements) *to the identity of a single collection*. More general *multiple-winner competitions* map the sequence of collections *to a set of such identities* (which may be of either a predetermined or a varying size). Even more general *ranking competitions* map the

¹³In contrast, in other fields the clients are mostly outsiders who do not participate (directly) in the struggle that takes place in the field (over its symbolic capital).

¹⁴Indeed, the same coupling of exchange value and use value is a cornerstone of classical economy, which refers to values of commodities in the society at large. Here, however, I refer to the symbolic capital of a field, which is a focus of struggle in the field. When the field’s products are used mostly by outsiders, their use of these products need not determine the struggle that takes place in the field (since they do not participate in it). A good example is provided by fields of arts, where the internal hierarchies (and symbolic capital) often do not reflect the use (and perception) of the products by outsiders. But in case of scientific fields the users are typically participants in the field, and thus this coupling of exchange value and use value holds.

¹⁵I use the term *artificial* to indicate that competitions are not inherent to the field. The evaluation of the importance of scientific achievements is implicit in the operation of the field, and there is no inherent (rather than circumstantial) need for the field to make this process explicit, let alone establish auxiliary evaluation processes that generate outcomes that do not match the outcomes of the primary and implicit evaluation process.

¹⁶I refrain from calling this form *parasitic*, although this term strikes me as most adequate.

sequence of collections *to a ranked list of a subset of the identities*. Most generally, there are **scoring competitions** that map the sequence of collections *to a sequence of numbers, where the domain of these numbers is significantly smaller than the domain of possible collections*. Indeed, all the foregoing forms of competition are special cases of the scoring competition. Five typical cases of competitions are listed next.

1. Competition for a position, where the individual collections correspond to the research records and perceived potentials of individual candidates, and the result is either a single winner or an ordered list of offers;¹⁷
2. Competition for grants, where the individual collections correspond to research proposals (which in turn include elements as in (1)), and the result may be either a set of proposals to be funded or the amount of funding assigned to each proposal;¹⁸
3. Competition for conference presentation, where the individual collections correspond to submitted papers and the result is a set of accepted papers;
4. Competition for workshop invitation, where the individual collections are as in (1) except that the candidates may not apply explicitly;
5. Competition for awards may take forms similar to the above. Unlike all other competitions listed above, competition for awards are clearly superfluous; they are not necessary under any reasonable meaning of this term. See further discussion of awards at the end of Section 2.2.

A key feature of *competitions* is that they *are highly lossy filters*; that is, *they lose information by mapping collections of achievements to numbers* (which are taken from a significantly smaller domain). This effect is most drastic in the case of single-winner and multiple-winner competitions, which take as input collections of scientific achievements that may be quite close to one another in their importance, and separate them into winners and losers. The point is that *the separation line may not reflect any clear separation in the space of collections*; it is merely an artifact of the rules of the competition. Thus, in these cases we get a significant *distortion* effect.¹⁹ I wish to stress that the foregoing holds even if the filter (competition) is monotone (i.e., more important collections obtain higher values). *The only possible justification for the application of such a filter is the principle of necessity*. For example, whenever the number of indivisible resources (e.g., positions or conference presentations) exceeds the number of indivisible entities (e.g., candidates or works), the allocation need to be determined by such a filter.

A competition is called **direct** if the mapping is applied simultaneously (or effectively so) to all collections. A journal review of an individual paper may be viewed as a direct competition consisting of a single collection (i.e., the submitted paper), and having either a single winner or no winners. Alternatively, journal review may be viewed as an indirect (multiple-winner) competition when referring to the set of all papers submitted in a given period.

¹⁷The former case (which constitutes a single-winner competition) refers to the practice in which the institute makes a single offer and does not fill the position if the offer is declined. The latter case, which constitutes a ranking competition, corresponds to the more common practice of turning to the second ranked candidate if the first one has declined the offer (and so on).

¹⁸The first case constitutes a multiple-winner competition, whereas the second case constitutes a scoring competition.

¹⁹Indeed, I define the **distortion** created by a competition as the deviation of its outcome from the closest proportional ranking of the importance of the corresponding (collections of) achievements, where a proportional ranking is an affine transformation.

Competitions can be explicit (as in all the aforementioned cases) or implicit, where a competition is called implicit if no visible action is directly taken based on the outcome of the competition (and the related scientists may not even be aware of the competition). An archetypical example of an implicit competition is the scoring competition that emerges when somebody aggregates the outcomes of several other competitions. The notorious practice of counting journal publications (or conference presentations) is such a case. Typically, implicit competitions are used as input to secondary competitions, discussed next.

Competitions can be **primary** or **secondary**, where a competition is called **secondary** if it is determined to a large extent by the outcomes of prior (primary and/or secondary) competitions.²⁰ Indeed, some of the foregoing competitions (e.g., hiring decisions) become secondary *if* their outcome is mostly determined by the outcome of prior competitions (e.g., counting journal and/or conference publications), while the corresponding collections of achievements are not considered directly.

Competitions distort the struggle in the field; that is, in the presence of competitions, the evaluation of scientific products is decoupled from their actual use, and consequently the distribution of symbolic capital is decoupled from the importance of the scientific achievements. This decoupling is increasingly more drastic when the competitions are more dominant in the field. Furthermore, secondary competitions tend to have a worse (decoupling) effect than primary ones, since they are distortions of distortions. In particular, a secondary competition that is determined by the outcomes of monotone competitions may be non-monotone (with respect to the importance of the collections of achievements even if it is monotone with respect to the outcomes of the competitions that it uses).²¹ Let me stress that all these effects are valid even when assuming that all primary competitions are monotone (i.e., more important collections obtain higher values), which – in my opinion – is rarely the case nowadays in TOC.²²

Warning: The rest of this essay is even more opinionated than the text so far. With this warning in place, let me reiterate the main opinion expressed so far.

Main normative thesis (recap.): *The only possible justification for the use of significantly distorting competitions is the principle of necessity.*²³ Here I refer to the legal principle that allows

²⁰I wish to stress that the distinction between primary and secondary competitions refers to the question of what determines the outcome, not to the internal procedures of the competition. In particular, I do not consider as secondary competitions that proceed in two rounds such that in the first round the set of candidates are trimmed according to some implicit secondary multi-winner competition (which may be non-monotone) and in the second round only these candidates are considered, provided that the final outcome (of the two-round competition) is identical to the outcome that would have been determined by a monotone primary competition.

²¹Consider, for example, the secondary scoring competition that consists of counting the number of publications in a specific venue, which in turn is a multiple-winner competition (assumed to be monotone). This secondary competition ranks a collection of two works that are each marginally above the acceptance threshold of the venue *higher than* an alternative pair of works consisting of one work that is significantly above that threshold and a second work that is only marginally below that threshold. Unfortunately, this example is far from being hypothetical.

²²Unfortunately, the most dominant primary competitions in TOC nowadays are multiple-winner competitions that correspond to conference presentations. As discussed in the sequel, due to several inherent reasons, these competitions are rarely monotone. These inherent reasons have nothing to do with the quality of the PC; that is, these competitions are highly unlikely to be monotone even assuming the best possible PC, but of course they may be even more distorting as the quality of the PC deteriorates. (Let me stress that by the “quality of the PC” I mean the quality of its collective operation (i.e., the way it solicits reviews, its deliberations, and its principled and specific decisions).)

²³Recall that multiple-winner (and ranking) competitions are highly distorting. The same hold for any secondary competition that is determined by their outcome. The only competitions that may not be significantly distorting are

a (proportional) violation of the law out of *clear necessity*. Of course, in the current context, law should be replaced by the principle of assigning credit proportionally to the importance of achievements. The role of the latter principle in the dynamics of scientific fields is discussed in Section 2.3.

2.2 On the effect of competitions (as seen in TOC)

Recall that competitions distort the struggle in the field; they decouple the evaluation of scientific achievement from their actual use, and consequently distort the distribution of symbolic capital (w.r.t the importance of the scientific achievements). I will consider these effects as seen in TOC, starting with an analysis of the FOCS and STOC conferences, hereafter referred to as FOCSTOC.

The case of FOCSTOC: The original purpose of these conferences was to allow for the fast dissemination of ideas of *potential* importance and wide interest through the TOC community. Thus, these conferences were established as (multiple-winner) competitions with the justification of serving the community (by providing it with a selected program of feasible size). The selection of a program for FOCSTOC already creates distortion, since a work included in the program has a significantly higher chance of reaching potential users than a work that is not included in the program. Thus, this selection effectively increases the potential importance of some work in a way that is likely to be disproportional to their intrinsic potential (e.g., if presentation in FOCSTOC increases the chances of potential use by a factor of 1.1, then the gap between an accepted work having grade 6 and a non-accepted work of grade 5.7 increased from 0.3 to 0.9).

But things have become (and are becoming) even worse: Once a “FOCSTOC publication” began to be counted as evidence to the importance of a work, the foregoing gap increased further; that is, the more this count matters (e.g., for other competitions), the greater the distortion affected by this count becomes.²⁴ In addition, given the increased impact of the outcome of this competition, authors have modified their strategy accordingly. Rather than trying to address the community at large, in order to increase the potential use of their work (once accepted), they aim at the experts that are expected to be most influential in determining whether or not their paper is accepted. That is, *the focus of the authors has moved from the actual presentation in FOCSTOC* (i.e., trying to communicate the achievement to the community at large) *to just getting the paper accepted*.

Needless to say, the audience (which consists of authors of similar works) as well as the program committee (PC) have modified their expectations regarding the authors’ behavior, and are behaving accordingly. It is increasingly the case that scientists do not attend FOCSTOC to learn of new achievements *outside their expertise*, and the PC tends to subcontract the selection process to expert reviewers (rather than assuming full responsibility for the selection, which is informed by the experts’ articulation of the potential benefit of the presentation to the community at large). The PC’s behavior only encourages further change in the authors’ focus, creating a vicious cycle. The end result is a conference that serves no goal but the mere holding of a competition (which

scoring competitions that use a sufficiently large range.

²⁴Here I refer to the effect of FOCSTOC on TOC at large. My starting point is that FOCSTOC (as any multiple-winner competition) is inherently distorting (to some significant extent), and the question at hand is what is the impact of the distortion (in this competition) on the field at large. My (self-evident) claim is that the growing influence of the outcomes of FOCSTOC on the outcomes of other competitions means that the distortion introduced by FOCSTOC is becoming increasingly more influential. That is, even when the level of distortion in the outcomes of FOCSTOC remains fixed, the growing impact of these outcomes mean that their distortion effect on the field at large increases.

indeed creates outcomes used by other competitions).²⁵

Thus, eventually FOCSTOC may become a *pure competition*, defined as a *competition having no aim but its own existence* (i.e., the existence of a competition). That is, pure competitions serve no *scientific* purpose. Did FOCSTOC reach this point or is close to it? Let me leave this question open, and note that my impression is that things are definitely evolving towards this direction.²⁶ In any case, I think we should all be worried about the potential of such an evolution.

Other TOC conferences seem to suffer less from the aforementioned phenomena. This is mainly because they “count” less as evidence of importance (i.e., publications in them are either not counted by other competitions or their effect on these competitions is less significant). Thus, the vicious cycle described above is less powerful, and consequently these conferences may still serve the intended scientific purposes.

The case of awards: Competitions for awards are perfect examples of pure competitions, lacking any objective scientific justification and/or purpose. The awards are established with the sole purpose of *highlighting* certain achievements and/or individuals. In my opinion, this highlighting is nothing other than an intentional distortion of the distribution of symbolic capital (i.e., artificially increasing the symbolic capital attributed to some achievements and/or individuals), and a competition is established for the purpose of effecting this distortion. That is, regardless of whether the competition is monotone or not (i.e., if the award is given to the most deserving candidate), its objective effect is to increase the prestige of the winner beyond whatever is justified by the importance of the actual achievement(s).^{27 28}

²⁵Let us take a closer look at what happens when competitions such as FOCSTOC become more dominant. Recall that initially FOCSTOC has had the effect of increasing the potential use of accepted works, say by a constant factor of 1.1. Assuming that all potentials are materialized, this means that a work of potential x is eventually assigned (symbolic) capital $1.1 \cdot x$ if it were accepted to FOCSTOC and is credited with x otherwise. When the mere acceptance to FOCSTOC starts to be counted as evidence of importance, we may conceptualize the situation as assigning c additional credit points to accepted papers. Thus, a paper with potential x gets $1.1 \cdot x + c$ credit if the paper is accepted, and x credit otherwise. If FOCSTOC becomes a pure competition and its outcome dominates the credit distribution, then this means that a paper is assigned credit C if accepted and zero otherwise. Assuming that FOCSTOC is a monotone competition with acceptance threshold t , this means that this competition maps a potential of x to credit of $\text{Th}_t(x) \cdot C$, where $\text{Th}_t(x) = 1$ if $x \geq t$ and $\text{Th}_t(x) = 0$ otherwise.

²⁶I cannot refrain from commenting, already at this point, that if these conferences cannot be restored to a form that does serve the intended scientific purposes, then they better be abolished. This statement is not made lightly: For many years FOCSTOC has served our community well, providing a unique TOC-wide forum for exchange of ideas as well as a focal point of the TOC community (shaping its scientific agenda and fostering solidarity among its members).

²⁷The establishment of awards is sometimes justified by the “need” to address external pressures, but I reject this justification for several reasons. First, let me stress that such a justification does not relate to the field’s internal dynamics, but rather to its interaction with the external world. Thus, the “need” claimed here is not an inherent necessity of the field, but rather a matter of convenience for its advocates towards the outside. Second, as hinted in the last phrase, I claim that the use of awards in such advocacy is a matter of convenience, not necessity (even when considered within the realm of advocacy). Furthermore, my opinion is that even when used in external circumstances, awards cause more harm than benefit. Specifically, easy to establish assertions of merit are indeed supported by awards (although they could easily pass without them); but the typical cases, which typically lack awards, are discredited by the lack of awards. Lastly, I claim that these awards were established without careful consideration of their *effect on the dynamics of the field*.

²⁸Personal awards have another bad effect: They enforce the illusion that “great people” make great contributions, whereas the truth is that great contributions make (or define) great people. The myth of (inborn) “great people” is often an impediment to the development of aspiring scientists, and I think that any field will benefit by disposing of this false myth.

Another issue: potential importance versus actual importance. Competitions for the “best paper in the conference” awards are particularly distorting, since they are typically decided before the importance of the candidates can be soundly evaluated.²⁹ The problem is that these awards (which may have no direct effect), influence secondary competitions that take them into account, while neglecting to note that only the potential importance of the paper (rather than its actual importance) was evaluated by the PC.

The latter comment applies also to the mere acceptance to the conference; that is, here too the secondary competitions that rely on this outcome refer to potential importance as judged at a very early stage and under highly unfavorable circumstances (e.g., an often sub-optimal choice of polled opinions, time pressure, and strong effects of group dynamics).³⁰ This should be contrasted with a proper evaluation of the importance of various achievements, which may be obtained by sampling a larger number of experts a few years after the achievement was obtained.

2.3 What do competitions serve and why are they bad

As stated up-front, some competitions are justified by the principle of necessity, but many competitions are just superfluous. In this section, I first address the question of why do the latter exist (or how did they come into being). I then turn to the main issue, which is the harmful effect of competitions.

On the function of unnecessary competitions. Considering awards and other unnecessary (i.e., superfluous) competitions, one may ask why do these competitions exist at all. I reject the popular answer that asserts that these competitions contribute to the promotion of science by providing incentives for good research. As described up-front, the incentive for obtaining important achievements is already built in the struggle over symbolic capital (i.e., important achievements mean higher prestige), let alone that there are more than enough necessary competitions. My answer to the foregoing question is that the pure and the superfluous competitions provide vulgar entertainment, which is particularly appealing to external people (e.g., the general public, the educated public, deans, etc) and/or to scientists whose main interests lie outside the field (e.g., in collaboration with external forces and enterprises).³¹ In addition, when a secondary competition relies on a prior competition, the “judges” in the secondary competitions can neglect their duty to

²⁹Indeed, it would have been more adequate to refer to papers that win these awards as the “most promising papers in the conference” (as judged by the PC).

³⁰Let me spell out what I mean in these terms. Firstly, let me stress that my concern regarding the effect of these circumstances refers to the PC’s decisions in typical cases, whereas in some (relatively rare) cases one can obtain a good estimate of the achievement’s (potential) importance already at an early stage (and even under the unfavorable circumstances discussed next). By a *sub-optimal choice of polled opinions*, I refer to the fact that the evaluation of each submission is typically determined by a couple of experts that are on the PC and/or by a few external reviews. Both these PC members and the reviewers are not necessarily most qualified to offer such evaluations; they are typically selected based on various considerations (rather than based on qualifications that are most relevant to the evaluation of the specific submission). Furthermore, both the PC and the reviewers operate under *time pressure*, which is most problematic in the typical case in which they are asked to evaluate a work they did not encounter before. Lastly, the PC’s decisions regarding the bulk of the submissions are strongly affected by various aspects of *group dynamics*. Although I think that program committees can do a better job than what is currently the norm in TOC conferences, it is important to realize that the selection of a program for a conference has inherent limitation w.r.t evaluating the potential importance of the various submissions. This may be tolerated provided that everybody understands that the PC is merely doing its best in attempt to compose the best possible program, but things get out of hand if scientists start using the PC’s decisions (i.e., whether or not a paper was included in the program) as a basis for evaluating the paper’s importance.

³¹In addition, there is a vulgar element in each person, and so, to some extent, vulgar entertainment appeals to all.

evaluate the material in depth, and escape feeling responsible for their decisions.³²

Why are competitions bad? Assuming that one agrees that competitions are distorting filters, one may still ask why is this distortion bad. Evidently, this distortion is *unfair* (i.e., it distorts the proportionate distribution of credit relative to the importance of the achievements) and it *affects the strategies of the scientists* (which may become geared towards winning these competitions rather than towards obtaining important achievements).³³ But is this necessarily bad?

While one may be willing to sacrifice fairness for the progress of science, the acute problem is the effect on the scientists' strategies.³⁴ That is, the question at hand is what will happen if all scientists use strategies directed towards winning competitions that introduce a significant amount of distortion.³⁵ This question has a principled aspect and a practical aspect. Let me start with the latter.

The main practical issue is that once the coupling of credit with achievement is broken, the question arises as to *who will determine the new crediting mechanism and how will it be determined so as to guarantee the progress of the scientific field* (let alone under changing circumstances). The obvious danger is that the mechanism will be determined by a certain "class" of scientists (i.e., those holding administrative positions) and under heavy influence of external forces. It is highly unlikely, to say the least, that this will serve the long-term benefit of the field.

A second practical issue is that the perception of the progress (or the state) of a scientific field as being reflected by the mere sum of all achievements (at a given time) is highly inadequate. This perception does not account for the (commonly valued) diversity of research directions, which

³²Of course, I do not expect the members of a departmental hiring committee to read the candidates' papers (unless they are experts in the area), but I do expect them to obtain the evaluation of reliable experts, and I do expect these experts to offer a professional evaluation of scientific works (rather than count publications). Indeed, even identifying reliable experts and understanding what they actually say in their reports is a highly non-trivial task, but this is a task that a departmental hiring committee can be expected to do. Ditto w.r.t committees that judge in any other necessary competition. But, indeed, acting as good "judges" is work (i.e., more work than counting), and it calls for assuming responsibility (rather than just saying "the count was too low").

³³Referring to the example presented in Note 21, a strategy geared towards winning the secondary competition (which only counts publications) would lead the scientists to prefer two marginally accepted papers over a pair of papers consisting of one clearly accepted paper and one marginally rejected paper. Such strategies will affect the way research is conducted, encouraging scientists to invest resources in a work only to the point that guarantees acceptance for publication. The fact that this behavior sounds absurd to the reader is actually a good sign. It means that competitions (of the foregoing type) do not totally dominate our field, or at least that the reader does not fully yield to their logic. On the other hand, behaviors of similar flavor (although not as extreme) can be seen in TOC nowadays (and also existed to a lesser extent in the past). I believe that the foregoing conceptualization provides an explanation for these behaviors.

³⁴In contrast to the naive assumption, Kleinberg and Oren have suggested that, under some circumstances, an unfair distribution of credit *may* serve the progress of science (i.e., social welfare) better than a fair distribution of credit. In their paper "Mechanisms for (Mis)Allocating Scientific Credit" [STOC'11], they demonstrate this thesis by rigorous results that refer to an oversimplified formal model, which relies on the standard game theoretic assumptions (i.e., rationality, independence, and full information, which give rise to the relevance of the notion of equilibria). Even those skeptic of these assumptions (e.g., myself) must view their results as a demonstration of the aforementioned thesis in a "toy model" (which does not seem rigged to support the thesis). Needless to say, not every unfair allocation of credit outperforms the fair allocation, let alone yield socially optimal welfare. Indeed, some unfair credit-allocation schemes may yield very bad results (from the perspective of social welfare). Likewise, fair allocation of credit may be socially optimal under some circumstances; it is just not socially optimal under all circumstances. The question is what may happen in our context (i.e., under the circumstances of a scientific field like TOC). While the first practical issue (discussed below) refers to this question, the other issues go beyond it.

³⁵Indeed, scoring competitions that use a large range of values may have a very small distortion effect. But these are not the typical competitions that spread as fire in the various fields. The typical (single-winner and multiple-winner) competitions induce large distortion, and this effect is amplified by secondary competitions that rely on them.

is so crucial for scientific progress. Thus, over-crediting some achievements while under-crediting others may yield a higher temporary return, but it may also distort the evolution of the field by encouraging research in over-credited directions (and discouraging research in under-credited directions). Such a distortion may cause much damage in the long-term. (Indeed, in many social settings, short-term optimization fails to produce a long-term optimum.)³⁶ In contrast, crediting each research direction proportionally to (the best current estimate of) its importance seems the best thing to do.

The principled issue is that the situation described above (i.e., the universal use of strategies that are geared towards winning competitions that significantly distort credit) cannot really persist (in the following theoretical sense). The first point is that the *strategies employed by the different scientists effect the structure of the field*.³⁷ In particular, strategies geared at winning certain competitions create activities geared towards this goal, and in the context of such activities the notion of what is important gets modified (since the usefulness of achievements is modified by the activity). Specifically, under normal circumstances, the modified notion of importance (i.e., usefulness) will fit the actual activity (determined by the competitions). Thus, the actual structure of the field gets modified such that the (previously distorting) competitions are no longer distorting.³⁸ It follows that the issue is neither confined to the problem of maximizing the aggregate “production of the field” (as discussed in Note 34) nor is it confined to any aspect of a fixed field (e.g., the effect of various strategies on a fixed field); the field itself is modified – it is no longer the same field. Hence, *the issue at hand is not the assignment of credits in a way that is not aligned with the field’s notion of importance, but rather modifying the field so that the resulting field’s notion of importance is aligned with this assignment of credit*.

The last assertion does not kill the debate but rather uncovers its true nature: *This is not a debate on the assignment of credits, but rather a debate on the structure of the field*. That is, those who want to assign credits in a way that does not reflect the current notion of importance in the field, actually *want to modify the field so that its notion of importance will reflect the credit assignment that they propose*. This by itself does not say that they are wrong; *disagreement on the field’s notion of importance is a central part of the struggle in any field*. But an honest argument will admit that the question in dispute is the very structure of the field, rather than pretend that the dispute is about enhancing productivity of a fixed field.

This brings me to my promise (stated in the Introduction) to articulate in what sense is the original field better than the field modified by competitions. In general, the question is in what sense is one interpretation of importance better than another. My answer is that an interpretation that abides the interests of the field (which in the case of a scientific field includes an interest

³⁶This is indeed reminiscent of the failure of greedy algorithms to yield optimal solutions.

³⁷Indeed, the field evolves according to its intrinsic logic if the strategies of its participants are determined by the field’s intrinsic notions of importance. But when the participants employ strategies that are determined by a different notion of importance (e.g., one derived from the credits assigned by competitions), the field is reconfigured according to this different logic.

³⁸The typical case is when competitions are introduced gradually, causing a corresponding gradual modification of the strategies. In other words, the credit assigned by the scientists (according to their evaluation of importance of the achievements) is gradually modified to fit the gradually modified allocation of credit performed by the competitions (since their activities are aimed at winning these competitions). In this case, no significant gap is ever created between the two patterns of credit distribution; they both change almost simultaneously, but the competitions are the ones that determine the change. A different situation arises when highly distorting competitions are introduced abruptly. Note that such a distortion can only be enforced by external forces (since the active scientists will continue, at least for some time, to use the prior credit distribution scheme). But also in this case, if the credit distribution pattern of the competitions is maintained (by an external force), then this will cause the field to change so that its new notions of importance adapt to these competitions.

in objectivity regarding a specific type of problems) is superior to an interpretation imposed by external circumstances. In other words, I distinguish between a *struggle over the interpretation of the field's interests* and an *attempt to modify the field based an agenda that is indifferent (or even antagonistic) to the field's interests*. In the first case there is no objective position (or judge) in the field that can rule which interpretation is better, but in the second case the intrinsic logic of the field rules against such a modification.³⁹ (Of course, the latter ruling is not universal; it is only a ruling of the field's intrinsic logic, which remains unchanged under different interpretations of its interests. In other words, this ruling is categorically correct within the field, and only within the field, as long as the very basic definition of the field remains intact.)⁴⁰

More on struggle versus competition. Another difference between struggles and competitions in scientific fields, which is discussed by Bourdieu (see Note 6), is that struggles tend to emphasize (fundamental) differences whereas competitions tend to emphasize (fundamental) similarities. Specifically, as noted above, a fair amount of the struggle in a field evolves around determining what is important, and in this case the struggling participants hold fundamentally different views and/or approaches. In contrast, competition favor a situation in which the competitors are comparable, whereas fundamentally different approaches are hard (or even impossible) to compare.⁴¹ Thus, struggles reflect inherent questions of the field (e.g., which approach is more adequate), which makes them stimulating and good for the field, whereas competitions reflect nothing of that sort. It seems to me that this analysis is more relevant to fields of art and social sciences, but still it is not irrelevant to scientific fields.

2.4 Additional comments

I conclude the theoretical analysis with a few comments regarding the issues discussed so far. The first couple of comments are actually recapitulations (and elaborations) of comments that I previously made in passing.

On the monotonicity assumption (recap). My entire analysis of the distortion affected by competitions was based on the assumption that these competitions are monotone; that is, more important (collections of) achievements are assigned higher values (in case of a scoring competition). This assumption rarely holds and is often severely violated (cf, e.g., Notes 21, 22, and 30), which creates an even larger distortion.

Potential importance and potential achievements (recap). As pointed out in Sections 2.1 and 2.2, the need to evaluate the *potential* importance of new achievements as well as a scientist's *potential* for future achievements is an artifact of the existence of competitions. Needless to say, such evaluations are speculative in nature; still, they can be justified by the principle of necessity when

³⁹As evidence to the latter assertion, I note that external (i.e., external to the field) influences and/or circumstances (including considerations) *must be used* in order to promote (let alone enforce) an interpretation that is indifferent to the field's interests.

⁴⁰Indeed, this discussion assumes the existence of a basic definition of the field, which is agreed upon and considered self-evident by all participants in the field. This agreement determines who are the participants, what are the shared interests, and what are the legitimate activities. All disagreements refer to this shared agreement; there could be no disagreement between people who share nothing.

⁴¹One possible consequence of this tendency is that competitions are held only among competitors that share the same approach, and another is that one approach dominates the competition. In either case, holding a competition requires reducing all competitors to the same scale.

applied to some types of necessary competitions (e.g., competitions for positions and conference presentations). However, as I argued in Section 2.2, using the outcomes of such competitions to determine the outcome of a secondary competition is (in many cases) twice wrong. The first fault, which is common to all secondary competitions, is that replacing the direct evaluation of scientific work by the outcome of a (multiple-winner) competition introduces a significant distortion. The second fault occurs whenever an updated evaluation of the importance of the achievements is available but one still refers to a competition in which only speculations regarding the potential importance of these achievements were taken into account.

On the evolution of importance in time. The importance of an achievement was defined as the accumulative influence of the achievement on subsequent research. Thus, as stated in Note 9, the importance of a scientific work can only increase in time. Each achievement starts with zero importance, although even at this starting point its *potential importance* may be soundly evaluated (or just speculated) to be significant. With time, the potential importance of an achievement may (or may not) materialize; and, typically, at some point, one stops talking of its potential importance and refers only to its (proven) importance, since at that time the actual importance dominates the potential importance. However, fundamentally important work escape the latter fate; they always have a potential importance that significantly exceeds their current importance, since they are believed to influence also much future work (which is clearly a pure potential).

In this context, one may talk of the *trajectory of importance* (at a certain time) defined to be the rate of growth of importance (at that time).⁴² Indeed, fundamentally important work maintain a steep trajectory of importance over time. In contrast, the trajectory of importance of an achievement may become zero, for example, when the community perceives the related research directions as reaching a dead-end.

A basic augmentation. For simplicity, my presentation presumes that symbolic capital is allocated only for scientific achievements. However, in reality, symbolic capital is allocated also for service to the scientific community (i.e., educational and administrative contributions), because this service is useful to this community. I wish to stress that also this allocation is governed by the same principle; that is, the allocation of symbolic capital to (educational or administrative) service is coupled with the actual importance of this service (i.e., its usefulness).

A parenthetical comment on the relation to the intellectual versus instrumental dichotomy. In a recent essay entitled “On the status of intellectual values in TOC”⁴³, I referred to intellectual versus instrumental dichotomy (i.e., the intellectual and instrumental faces of science). I defined the intellectual face of science as the *desire to understand a specific type of phenomena* (which in turn is captured by a specific type of questions), and its *instrumental face* as the *desire to use such understanding in order to predict and/or effect (or manipulate) the environment*. As is stated in that essay, instrumental achievements are typically easier to identify and evaluate than intellectual ones, and thus competitions tend to favor the former, especially when the competitions are observed and/or monitored by external people. I wish to stress that this does not mean

⁴²Often, when researchers talk of “importance” they actually mean what I have defined as the trajectory of importance. This may be reflected in phrases such as “the importance of X for current research”, which seems to correspond to the current (or recent past) value of the trajectory of the importance of X. Regardless of its name, the notion of the trajectory of importance is related to the yet unmaterialized potential importance (i.e., to the future influence of the achievement).

⁴³Available from the web-page <http://www.wisdom.weizmann.ac.il/~oded/on-toc-val.html>

that intellectual achievements are generically more important than instrumental achievements (or the other way around): By definition, the importance of each achievement is determined by its influence. My only point here is that vulgar competitions tend to introduce a bias in favor of instrumental achievement, not necessarily because they value “instrumental influence” more but rather because such influence is relatively easier to identify. Thus, the preoccupation with vulgar competitions tends to bias the field against its intellectual face.

3 Practice: Against the Dominance of Competitions

Assuming the validity of my analysis, it is quite clear what should be done in order to improve the situation: (1) all unnecessary competitions should be abolished, and (2) all (necessary) secondary competitions should be converted to primary ones (as much as possible). This is theoretically obvious, but practically non-trivial.

The non-triviality of implementing the aforementioned actions arises from the circumstances that led to the creation of these unnecessary and/or secondary competitions. Recall that I attributed the creation of these competitions to the desires of external forces (which do not care for and cannot understand the actual contents of the field), and to the intellectual laziness and fear of responsibility of *some* senior scientists (i.e., mainly those sitting in committees that run necessary competitions).⁴⁴ That is, I claim that the unnecessary and/or secondary competitions came under existence due to conscious and/or unconscious actions of a small number of scientists; specifically, some senior scientists (especially those holding administrative positions in the field). In most cases, these actions were taken without careful considerations of their consequences on the field, let alone a public and open discussion.⁴⁵ Furthermore, I claim that these actions were motivated by conscious and/or unconscious considerations that are external to the field; for example, the personal comfort of scientists as human beings (rather than their interests as participants in the field).

The latter claim is of key importance. To balance out motivations that are external to the field, we should put into action opposing motivations of a similar nature. Such motivations do exist, since almost all active scientists suffer from the increase in the dominance of competitions, which increase and concretize the stress and frustration that is an inherent part of the scientists’ life of struggle.⁴⁶ Furthermore, there is a fundamental difference between a meaningful struggle evolving around interests that the scientists chose (when they entered the field) and a meaningless rat race ruled by competitions that do not reflect their own interests.⁴⁷ (Indeed, things did not get to that extreme in TOC yet, but they may get there if the logic of competitions is allowed to be played out to its bitter end.)

The next question is how can these personal interests of almost all scientists be put into action (so that the dominance of competitions is reduced). My answer is that there should be a collab-

⁴⁴Needless to say, I do not attribute these faults to all senior scientists who sit on these committees. But I do claim that what underlies the growing use of various publication counts (i.e., implicit “publication competitions” of the scoring type) in hiring and promotion committees (which then become secondary competitions) is the intellectual laziness and fear of responsibility of *some* senior scientists. Indeed, counting is easier than thinking, and relying on “objective” measures (while neglecting to question what they actually measure) allows people to escape responsibility. Ditto w.r.t the reference to various awards.

⁴⁵By now, many of these senior scientists have convinced themselves (as well as others) that there is no alternative (to this mode of operation). Needless to say, I reject this claim.

⁴⁶In addition, on the personal (or human) level, fairness has a value (or is a value) that transcends its functionality, and its violation is offensive to the individuals.

⁴⁷Indeed, competitions will exist also under “my vision”, but they will be reduced to the very minimum that is necessary. Thus, they may be less present in our lives, add less stress and frustrations, and be less offensive to our feeling of fairness.

orative action towards creating a normative atmosphere that is antagonistic to competitions. For example, it may and should be considered bad taste to refer to publication counts, and ditto using the outcome of any competition when discussing the merits of achievements and/or individuals. Indeed, I am talking about a change in values, one that puts more emphasis on the field's inherent values (i.e., both the general scientific values and the specific interest in the field's founding problems).

To summarize, on the theoretical level, the aforementioned actions (1) and (2) should be augmented by (3) strengthening the community's commitment to the field's values. Indeed, action (3) is the key to affecting actions (1) and (2) in reality, but it also holds that the latter actions promote (3). This dialectics yields a feedback cycle in which progress in one front helps the effort on the other front, and this positive feedback cycle can overcome the vicious cycle that increases the dominance of competitions in the field.

On less radical reforms. Some readers found the course of action that I suggest somewhat unrealistic, noting that it requires an unfamiliar collaborative action (towards forming a different atmosphere). My view is that this is unrealistic only in the sense that it does not fit the current reality (and atmosphere), which is the very thing that should be changed. Still, it seems a good idea to suggest a few examples of reforms that may be implemented within the current reality. These reforms do go against the dominance of competitions, but do not confront it directly. Let me start with individual actions, which many of us can take.

- *Try to steer PCs of conferences towards forming a program rather than running a competition:* When either serving on a PC or advising it as an external reviewer, try to steer the PC to focus on selecting a program that will best serve the attendees (rather than “awarding” the “most deserving” papers with a decision to accept them).
- *Try to steer competitions towards being primary:* When serving on either a hiring/promotion committee or a PC, try to steer the committee towards taking decisions on the basis of a real *understanding* of the contents of the work being considered rather than on the basis of the outcome of prior competitions. In particular, object to any argument that is based solely on reference to competitions (i.e., awards, various forms of paper counts, various forms of citation statistics, etc).

As for reforms that require a decision of the community at large (or rather its elected officers), I suggest to revitalize FOCSTOC by taking the following two steps:

- *Changing the format of the conference:* Devote approximately half of the conference to plenary sessions that are directed at the TOC community at large, while devoting the rest of the conference to numerous area-specific parallel sessions (which may be administrative as independent satellite workshops).

Such a change will better serve the original goals of the conference (at the current time), and is likely to reduce the focus on its competitive nature. Specifically, the new format may somewhat blur the gap between “acceptance to FOCSTOC” and “nothing” (by introducing an intermediate level of acceptance to the parallel sessions).

- *Changing the decisional procedures of the PCs:* Give more weight to minority opinions expressing strong support for including a specific work in the program. To some extent, this

policy will prefer a less safe potential for greater importance over a safer potential for smaller importance.

The resulting program is likely to be more inspiring and is less likely to be taken as a measure of “proven importance” (which is likely to reduce the competitive nature of the conference).

Of course, the above are just examples of possible reforms that have a progressive potential (according to my views...).

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Appendix: Fields and Scientific Fields According to Bourdieu

The following summary represents my (limited) understanding of Bourdieu's theories of general and scientific fields. This summary provides a perspective that is far wider than necessary for the current essay. In particular, even when referring to scientific fields, much of Bourdieu's focus is on the social sciences, and on direct political influences that limit their autonomy. Still, I believe that his general conceptual framework can be used also for analyzing TOC, as I did in the current essay.

In my opinion, my analysis (as presented in the main text) is consistent with Bourdieu's theory (as least as far as I understand it). However, I am not too concerned of this issue. This is firstly because I believe that the key question is whether my own analysis is valid, and not to what extent it is consistent with Bourdieu's analysis; my (acknowledged) reliance on Bourdieu is not meant to be an argument towards validity (but is rather a token of proper credit as well as an offer of a wider perspective). Secondly, like Bourdieu himself, I believe that one should take from each theory only parts that one finds useful, and not be too concerned of the eclectic and partial nature of this borrowing. Lastly, and again like Bourdieu, I believe that whatever is discovered in a detailed analysis of a special case has precedence over a general theory that was used in that analysis. In fact, the specific analysis refines the general theory by describing conditions under which the general theory may or may not hold.

On fields in general. As the division of labor within the society evolves and deepens, autonomous fields (e.g., politics, law, education, science, art) are created within it. These fields are defined by their specific activities and/or interests, and have their own internal structure and symbolic capital, which are subject to conflict and struggle. The form of symbolic capital is specific to the field, and it often refers to prestige and positions in the field. The symbolic capital and structure of the field are not reducible to economic capital and/or to the structure of society at large; likewise, the translation (or transformation) of symbolic capital to economical capital is far from immediate.⁴⁸

Subjects active in a field come with their own *habitus*, which consists of (conscious and unconscious) attitudes and dispositions that were socialized in other fields and in the society at large (including in various groups of the society, e.g., the family). For example, various aspects of personal taste (e.g., in food) are central to the *habitus*. Ditto regarding language. The socialization in the field modifies the *habitus*. In both cases, socialization is not confined to the conscious adaptation of explicit rules; the unconscious and non-reflexive adaptation of implicit practical attitudes is even more important.

People (esp., privileged ones) may participate in several fields, but their actions in different fields are separated.

For a field to function, there must be people who are interested in participating in it, and their *habitus* must include the *doxa* of the field (i.e., its basic values and norms (rules of conduct)). They must also command its practice.

The structure of the field is its current power relation; the partition of its specific capital, obtained by past struggles, which determines future possibilities of action. A common struggle that evolves, especially in times of crisis (in the field), is between the conservatives (of the status

⁴⁸The relative autonomous functioning of social institutions in a complex society was already recognized by Durkheim and Weber. Bourdieu goes beyond them by conceptualizing these relatively autonomous activities as fields having attributes (including capital) that are similar to those of the society at large, and suggesting that the structure and struggles in these fields may be analyzed separately from the society at large (but using similar methods).

quo, promoting orthodoxy) and the “rebels” or “deviants” (who are typically new comers). But the revolt rarely touches the doxa itself; the struggle presupposes the basic values and norms, but refers to their interpretation. The rebels too are invested in the field – they “paid” its entrance fees.

In contrast to what intellectual think, typically, *people do not act based on conscious rationale calculations*. Their actions are determined by the combination of the (fuzzy) attitudes embedded in their habitus and the circumstances (i.e., the relevant situation). The fact that the participants in a field acts in a way that objectively preserves and reproduces the field is not a result of a cynical calculation; it is rather due to the fact that their habitus was socialized to fit the interests of the field.

The (full) history of a field leaves its mark in each work and creation (and is the basis of the work’s interpretation and evaluation). The field determines what is a legitimate discourse, and more importantly who may or may not speak. These rules are subject of struggle in the field. There are things that cannot be thought of in the (language of the) field, and there are things that are only oppressed and/or marginalized in it (e.g., deemed irrelevant or stupid).

Science is a social field like any other, with its own distribution of power and its monopolies, its struggles and strategies, interests and profiles, but these take on specific forms. These specific forms yield a potential of absolute objectivity (i.e., truth), which can only be materialized under some circumstances (to be studied). One key point is that scientific fields include an interest in objectivity (i.e., truth),

On the objective relations and struggle in a scientific field. The (scientific) field is a system of objective relations between positions already won, and is the locus of struggle over the monopoly of (scientific) authority (or competence), defined as the state of having a socially recognized capacity to speak and act legitimately (in scientific matters). (These “objective relations” do not reduce to the aggregate of actual interactions, and the “social recognition” is more w.r.t the group of scientists when the field’s autonomy increases.)

The operation of the scientific field produces and presupposes a specific form of interest. This interest (and authority) should not be decoupled to “social” and “pure” aspects; both are faces of the same thing. These two faces are also present in the strategies (of the participants), which are both “social” and “pure” (since their social effectiveness w.r.t the scientific community depends on their “pure” appeal).

Given the pivotal role of “pure appeal” (or intrinsic interest), the struggles in the field are actually related to determining what is of pure scientific appeal and/or interest. The objective (or materialistic) view of these struggles means that the scientists’ strategies refer to the expectations of profit (both “social” and “pure”).

However, at the last account, a theory of science should reject the dichotomy between structure and agent, scientific and social, objective and subjective. The two parts of each dichotomy are merely faces of the same thing. One may talk of each of them, but should not think that it appears in isolation from the other.⁴⁹ Furthermore, none of them determines the other, but under some circumstances they are almost in unison (i.e., there is no antagonism between these faces): See discussion of *the conditions for objective diversion of ends*.

On the specificity of the struggle over scientific authority. In addition to an interest in objectivity (i.e., truth), a scientific field is special in the fact that the only clients for the scientific

⁴⁹For example, one can talk of the scientific face (or social face) of an action in the field, but should not think that the actual action is purely scientific (or purely social).

products are the other (competing) producers (see Note 13). This guarantees that the evaluation of these products (i.e., their socially perceived value) will match their actual value (i.e., their usefulness in the field). These assertions assume autonomy of the field, and are less valid when there is less autonomy. Thus, the producers must not only distinguish themselves, but also incorporate and transcend all predecessors and competitors. This may also involve struggle over the definition of the discipline, and its internal hierarchy (e.g., theoretical concepts vs empirical datum, pure vs applied; in general, what is considered more interesting/important). In particular, the perception as if there exists a common agreement on values and norms in the field is wrong; it is reminiscent of “pure functionalism” in sociology (i.e., an approach that views society as one organ and its institutes as satisfying different functions of this organ).⁵⁰ One should, instead, realize that different “classes” have different values and norms, and different strategies are “functional” for different groups.

Indeed, the question of importance is always under dispute. There are no outside judges and/or ways to resolve these disputes. They are “resolved” according to the relative strength of the participants involved in the dispute (i.e., the relative amount of symbolic capital that these participants command). These resolutions are thus nothing more than a snapshot of the structure of the field.

To summarize, the symbolic capital assigned to individuals depends on the perceived value of their product, which is determined by the current structure of the field. But this assignment changes the distribution of capital. (So there is a dialectic in work here; i.e., structure that determine strategies that determine structure.)

Strategies of conservation and subversion. An important observation is that there is a variety of strategies, each fit for different circumstances (depending on the structure of the field and the player’s position in it). Strategies vary in the amount of resources invested in them (e.g., research time) and in their nature (e.g., degree of risk). The choice of strategies depend on the agent’s current capital (e.g., those with more capital can afford higher risk). The initial capital (i.e., the capital at the entry point, which in turn can be traced back to the social class)⁵¹ is of key importance, not only in its direct benefits (of a good education) but also in that it determines the student’s position (i.e., symbolic capital) and the strategies suitable for him/her.

The choice of strategy mostly depends on the (symbolic) capital available to the specific agent, not on the agent’s “intrinsic” characteristics (e.g., age). Low capital suggests low “trajectory” strategies, focused on conservation and following the “beaten route”, whereas high capital allows (and creates expectations for) subversion and/or breakthroughs. It makes no sense to talk of an *average* career (and strategies for it); the point is the *differences* between careers (and the corresponding different strategies).

The dominant “class” (in the field) tends to conservative strategies, whereas new comers may either employ succession strategies or subversion strategies. The latter choice is determined by their initial capital, their general habitus, and the field’s structure.

⁵⁰In contrast, “conflict theory” views society as an arena of struggles between social groups. Within this theory, one may talk of “partial functionality” (i.e., a phenomenon that is functional for a specific group, although it may be “dysfunctional” for another).

⁵¹Viewing graduate school as the entry point, note that the symbolic capital at this time is determined mainly (but not solely) by the educational record of the students, which includes the prestige of undergraduate school they attended (i.e., the symbolic capital of the students when starting undergraduate school). The latter is largely determined by the educational record at high school, which again includes the prestige of that school. In the end of the digression we get to the symbolic capital accumulated at very early age, which is determined by the social conditions (including social class).

The structure of the field and strategies of struggle. Two key parameters are (1) the aggregate capital of the field, and (2) the inequality of capital distribution in the field. Larger aggregate capital tends to create higher entry costs, and thus lesser inequality (within the field), alas the competition intensifies. Another parameter, discussed next, is the field's autonomy.

The conditions for objective diversion of ends. What are the social conditions for an agreement between the individual strategies employed (in personal interest) and the progress of science? The answer can not be obtained by theories that associate the specific conditions of a specific field (and sometimes even of a specific time) with a universal truth, and conclude that such a diversion is always guaranteed (or alternatively that it never occurs). In contrast, to such theories, it is claimed that the diversion of ends is possible, but only under some conditions.

A first condition is a social *autonomy* of the field; that is, its freedom from any external political influence. This condition is reached when the paradigms of rationality becomes dominant (and all other arguments are disallowed) and the field becomes "richer" and thus requires higher entry fees.

A second condition is blurring the distinction between scientific revolutions and normal science, and having a vision of *continuous progress*. (Indeed, it is argued that Kuhn's description of scientific revolutions is an exception, not the norm, and that typical sciences know only much milder forms of "revolution/progress".) This implies that the scientific product is viewed as more valuable when used more by others, which introduces an in-built incentive for progress (and hence diversion of ends).

On non-autonomy. Science's sole basis is the collective belief in its bases, which is produced and presupposed by its very operation. Since the source of this collective belief cannot be in the field itself, the question arises as to its source. On the one extreme (of religious) the source is a *social imposition* (either external or internal to the field), whereas on the other extreme it is by *imposing rationality*. In general, one should ask what is the social arbitrariness of the field's collective belief (e.g., to what extent does this belief depend on the social circumstances).

This is related to the question of autonomy. An autonomous field develops according to its own dialectics, and its belief system does not rely on a claim of social independence (or any other claim regarding the society outside it).⁵² This is related to *strategies of false separation* in which non-autonomous fields assert their false autonomy: A key example is using evocative (descriptive-normative) terms that *claim a fictitious distance from the common use*, as opposed to using terms that are defined in terms of the field's system. Likewise, an autonomous field is not preoccupied with its neutrality (in social and/or political issues), since its belief system does not rely on the claim of neutrality. (Note that progress in direction of rationality does not require political neutrality, but rather (in social sciences) awareness of the social contents and effects (on the scientists).)

⁵²Indeed, ironically enough, the belief system underlying a non-autonomous scientific relies on the claim that the field's products and objects are independent of society at large. The belief system of an autonomous scientific field makes no such claim.