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Dr. Roberto Fumagalli

Junior Professor, University of Bayreuth

Research Visitor, London School of Economics

Visiting Scholar, University of Pennsylvania

Who is Afraid of Scientific Imperialism?

Abstract

In recent years, several authors have debated about the justifiability of so-called scientific imperialism. To date, however, widespread disagreements remain regarding both the identification and the normative evaluation of scientific imperialism. In this paper, I aim to remedy this situation by making some conceptual distinctions concerning scientific imperialism and by providing a detailed assessment of the most prominent objections to it. I shall argue that these objections provide a valuable basis for opposing some instances of scientific imperialism, but do not yield cogent reasons to think that scientific imperialism in general is objectionable or unjustified. I then highlight three wide-ranging implications of this result for the ongoing philosophical debate about the justifiability of scientific imperialism.

Keywords:

Scientific Imperialism; Disciplinary Autonomy; Disunity of Science; Scientific Progress; Epistemic Justification; Pragmatic Justification.

1. Introduction

Over the last few decades, there have been intense philosophical discussions of so-called scientific imperialism (henceforth, SI). The involved authors provided increasingly sophisticated conceptualizations of this notion (see e.g. Cartwright, 1999, Clarke and Walsh, 2009 and 2013, and Dupré, 1995) and debated at length about the justifiability of specific instances of SI (see e.g. Downes, 2017, Lazear, 2000, and Mäki, 2009 and 2013). There are at least three reasons why SI deserves detailed philosophical scrutiny. First, SI contributions target a vast range of natural and social disciplines, and have significant epistemic and pragmatic influences on modelling and theorizing in these disciplines (see e.g. Hirshleifer, 1985, and Fine, 2000, on economists' SI contributions to other decision sciences; see also Churchland, 2007, and Piccinini and Craver, 2011, on neuroscientists' SI contributions to other cognitive sciences). Second, discussions of SI bear on a series of foundational issues in epistemology and philosophy of science (e.g. unity of science and methodological pluralism, proper relations between different disciplines, epistemic authority of science). And third, SI contributions raise several concerns of pressing social and political relevance (see e.g. Dupré, 2001, ch.3-4, on some controversial implications of evolutionary psychologists' SI contributions for the politics of sex and gender, and Vincent, 2013, on how neuroscientists' SI contributions affect entrenched conceptions of moral and legal responsibility).

To date, however, widespread disagreements remain regarding both the identification and the normative evaluation of SI (see e.g. Davis, 2017, on economists' SI contributions, and Fumagalli, 2017a, on neuroscientists' SI contributions). In this paper, I aim to remedy this situation by making some conceptual distinctions concerning SI and by providing a detailed assessment of the most prominent objections to SI. The paper is organized as follows. In Section 2, I explicate the notion of SI and distinguish it from various forms of non-imperialistic disciplinary interaction. In Sections 3-6, I draw on a wide range of illustrations from natural and social disciplines to reconstruct and appraise four influential objections to SI. I consider in turn: the objection from *disciplinary autonomy* (see e.g. Aizawa and Gillett, 2011, and Fodor, 1974); the objection from *the disunity of science* (see e.g. Dupré, 1983, 1995 and 2001); the objection from *counterfactual scientific progress* (see e.g. Clarke and Walsh, 2009 and 2013); and the objection from *cumulative constraints* (see e.g. Mäki, 2009 and 2013).¹ I shall argue that these objections provide a valuable basis for opposing some instances of SI, but do not yield cogent reasons to think that SI in general is objectionable or unjustified. If correct, this result supports at least three

¹ This list encompasses the most cited and influential objections to SI. Not all of these objections have been proposed to ground principled opposition to SI. Still, all those objections have been put forward to identify what features allegedly make SI unjustified and to articulate why one should resist SI contributions (see e.g. Clarke and Walsh, 2009, Dupré, 1995, and Mäki, 2013).

wide-ranging implications for the ongoing philosophical debate about the justifiability of SI.

First, the critics of SI should provide more informative criteria for assessing SI and ground their opposition to SI on more plausible empirical and normative presuppositions. Second, what (if anything) makes some SI contributions objectionable does not lie in their imperialistic character, but rather relates to putative flaws in their empirical or normative presuppositions and to the unwarranted social or pragmatic implications some derive from such contributions. And third, the justifiability of SI contributions is best judged on the basis of specific case studies (see e.g. Fumagalli, 2017a, for a critical appraisal of neuroscientists' SI contributions targeting entrenched philosophical conceptions of free agency) rather than general evaluative criteria that abstract away from the modelling and explanatory practices of the examined disciplines. These three implications do not exclude that one may gain informative insights by assessing specific subsets of SI contributions *qua* imperialistic contributions (e.g. some SI contributions may share evidential and methodological features that make it helpful to group them for specific evaluative purposes). Still, they should make scientific theorists and practitioners wary of general calls either in favour of or against SI contributions. In particular, they challenge all those who debate about SI to further refine the criteria proposed for evaluating the justifiability of SI and focus their evaluations on specific subsets of SI contributions.

2. Defining Scientific Imperialism

The notion of SI has been given several characterizations by philosophers. Most of these characterizations relate SI to the systematic application of a discipline's theories and methods to model and explain phenomena investigated by other disciplines. For instance, some define SI in terms of specific disciplines' (e.g. physics and economics) aim to "account for almost everything" in the natural and the social world (Cartwright, 1999, 1; see also Fine, 2000, and Lazear, 2000, for similar characterizations). Similarly, others define SI as "the tendency to push a good scientific idea far beyond the domain in which it was originally introduced" for modelling and explanatory purposes (Dupré, 2001, 74; see also Stigler, 1984, and Dupré, 1995, for similar characterizations). The specialized literature has recently witnessed the proliferation of fine-grained distinctions regarding both the notion of SI and putatively different types of imperialistic contributions (see e.g. Mäki, 2009, for a distinction between imperialism of standing, imperialism of style, and imperialism of scope; see also Peels, 2016, and Stenmark, 2001, ch.1, on various forms of scientism). Below I gloss over these distinctions unless the cogency of my evaluation rests on such distinctions. For present purposes, the following remarks about the notion of SI are worth making.

2.1 SI and Disciplinary Boundaries. Discussions of SI commonly presuppose that scientific theorists and practitioners can reliably demarcate disciplinary boundaries (see e.g. Lazear, 2000). Nonetheless, speaking of SI does not commit one to associate sharp and immutable boundaries to disciplines (see e.g. Dupré, 1983, and Kidd, 2013). In fact, various authors in the SI literature emphasize that disciplinary boundaries are often blurry and can vary remarkably depending on several factors, ranging from researchers' epistemic interests to specific technological developments (see e.g. Roskies, 2010, on how progress in brain-imaging technology expanded the set of phenomena amenable to neuroscientific investigation). This variability can make it difficult to demarcate the domain of specific disciplines (see e.g. Brigandt, 2010, on biology) and establish whether particular cross-disciplinary interactions qualify as instances of SI (see e.g. Mäki and Marchionni, 2010). However, as I illustrate in point 2.2, it does not preclude one from identifying clear paradigmatic cases of SI (see also Sections 3-6 for other illustrations).²

2.2 Imperialistic versus non-Imperialistic Contributions. In the literature on SI, various criteria have been put forward to demarcate the set of SI contributions. In this paper, I speak of SI contributions broadly to cover systematic cross-disciplinary applications of theories and methods that *directly* intrude in the modelling and explanatory practices of the targeted disciplines (see e.g. Mäki, 2013, on so-called disciplinary imperialism). In doing so, I use the expression 'modelling and explanatory practices' to include both theoretical (e.g. classificatory) and pragmatic (e.g. problem solving) activities, with the terms 'models' and 'explanations' covering different types of models (e.g. causal and mathematical models) and distinct accounts of explanation (e.g. unificationist and mechanistic accounts). This characterization of SI contributions is sufficiently broad to encompass most alleged instances of SI (see e.g. Sections 3-6) and sufficiently precise to distinguish SI contributions from various forms of non-imperialistic disciplinary interaction (e.g. think of cases where a discipline's methods are applied to model previously unexplored phenomena and of cases where different disciplines exchange specific findings while continuing to pursue specialized research agenda). In particular, such characterization excludes from the set of SI contributions cross-disciplinary applications of theories and methods that do not directly intrude in the modelling and explanatory practices of the targeted disciplines (see e.g. Mäki, 2013, on so-called domain-only imperialism).³

² I focus on disciplinary boundaries as opposed to boundaries between units of analysis other than disciplines (see e.g. Darden and Maull, 1977, on fields, and Lakatos, 1970, on research programs) because in this paper I prevalently discuss interactions between different disciplines. My remarks concerning prominent objections to SI may be reformulated so as to target interactions between units of analysis other than disciplines.

³ The set of systematic cross-disciplinary applications of theories and methods that directly intrude in the modelling and explanatory practices of the targeted disciplines may be regarded as more or less broad depending on how one interprets the terms 'systematic' and 'applications'. However, this interpretative concern does not make my characterization of SI overly broad or uninformative. For on

2.3 *Descriptive versus Normative Definitions of SI.* Two types of definitions of SI are frequently contrasted in the SI literature. On the one hand, some employ ‘SI’ to designate an inherently objectionable (see e.g. Dupré, 1995) or commendable (see e.g. Hirshleifer, 1985) form of cross-disciplinary interaction (*normative definitions*). On the other hand, others (see e.g. Mäki, 2009, and Mäki and Marchionni, 2010) use ‘SI’ as a normatively neutral term, and distinguish justified and unjustified SI contributions depending on how these contributions fare in terms of further evaluative criteria (*descriptive definitions*). If correct, my thesis that the justifiability of SI contributions is best judged on the basis of specific case studies challenges the proponents of normative definitions of SI to provide reasons other than mere stipulation to adopt normative - as opposed to descriptive - definitions of SI. To be sure, one could always advocate the adoption of normative definitions of SI by stipulating at the outset that SI constitutes an inherently objectionable (or commendable) form of cross-disciplinary interaction. However, this stipulation threatens to trivialize the debate about the justifiability of SI by presupposing (rather than showing) that there are no justified instances of SI (or that all instances of SI are justified). Moreover, the reasons proposed to adopt normative (as opposed to descriptive) definitions of SI fall short of demonstrating that normative definitions are generally superior or otherwise preferable to descriptive ones. Let me expand on this point.

In recent years, various authors have advocated the adoption of normative definitions over descriptive ones on the alleged ground that the term ‘imperialism’ was originally imported from political contexts, where such term has inherent negative connotations (see e.g. Dupré, 2001). The idea is that just as political imperialists aim to dominate other political communities (e.g. population groups) scientific imperialists aim to dominate other scientific disciplines (see e.g. Clarke and Walsh, 2009). These calls to adopt normative (as opposed to descriptive) definitions of SI invite two interrelated rejoinders. First, the term ‘imperialism’ does not invariably have inherent negative connotations in political contexts (see e.g. Walsh and Boucher, 2017, on normatively neutral uses of such term). And second, even assuming that the term ‘imperialism’ invariably has inherent negative connotations in political contexts, the hypothesized fact that this term invariably has inherent negative connotations in such contexts falls short of implying that ‘SI’ should be used in a normative (as opposed to descriptive) sense. In particular, vindicating the adoption of normative definitions of SI would require the proponents of such definitions to show that cross-disciplinary relations are analogous to relations between political communities. However, the proponents of normative definitions of SI have hitherto failed to substantiate this analogy (see e.g. Mäki, 2016), and such analogy seems to break down in several respects (see e.g. Olson and

most interpretations of ‘systematic’ and ‘applications’, a few occasional or isolated instances of cross-disciplinary interaction fall short of constituting instances of SI.

Kähkönen, 2000, for a critical comparison of relations between political communities and relations between academic disciplines).

2.4 Evaluating SI. Several criteria have been proposed to assess the justifiability of SI contributions (see e.g. Fallis, 2006, on trade-offs between epistemic costs and benefits, and Clarke and Walsh, 2013, on the availability of non-imperialistic forms of cross-disciplinary interaction). What relevance one ascribes to distinct criteria may vary remarkably depending on several factors, ranging from the epistemic interests of the involved disciplines' practitioners (see e.g. McMullin, 1983) to the modelling standards and methods entrenched in such disciplines (see e.g. Longino, 2002, ch.8). Both epistemic and non-epistemic values inform the criteria proposed to assess the justifiability of SI contributions (see e.g. Ylikoski and Kuorikoski, 2010, on epistemic values such as explanatory power, Parker, 2013, on pragmatic values such as predictive robustness, and Rolin, 2017, on values that combine epistemic and non-epistemic dimensions such as epistemic justice). In fact, the debate concerning the justifiability of SI contributions frequently targets not just the issue whether these contributions promote (rather than hinder) specific values, but also the issue how these values are most aptly conceptualized (see e.g. Ylikoski, 2013, on distinct conceptualizations of explanatory power). These evaluative issues are conceptually distinct, but many interrelations can be found between them (see e.g. Kuorikoski and Marchionni, 2014, on how discipline-specific conceptions of epistemic values affect model construction and model evaluation in economics and sociology). I expand in Sections 3-6 on these interrelations and their implications for the evaluation of the justifiability of SI contributions.⁴

3. Objection from Disciplinary Autonomy

The *objection from disciplinary autonomy* opposes SI contributions on the alleged ground that these contributions reduce or threaten the autonomy of the targeted disciplines from the imperializing disciplines (see e.g. Aizawa and Gillett, 2011, and Fodor, 1974). The critics of SI have provided different characterizations of disciplinary autonomy, which refer to criteria such as the availability of specialized methods and modelling frameworks, the existence of independent research groups and journals, and the irreducibility of a discipline's theoretical principles or empirical generalizations to the principles or generalizations of other disciplines (see e.g. Fine, 2000, Mäki, 2016, and Mayr, 2004, ch.2). Suppose, for the sake of argument, that the critics of SI provide a precise and uncontroversial

⁴ In recent years, some authors attempted to provide more fine-grained partitions of the set of epistemic and non-epistemic values (see e.g. Douglas, 2013). I mention these attempts in passing since the cogency of my evaluation does not rest on what position one takes concerning such attempts. For a critical discussion of the distinction between epistemic and non-epistemic values, see e.g. Longino, 1996. For a proposal to regard such distinction as a continuum in some scientific contexts, see e.g. Rooney, 1992.

characterization of disciplinary autonomy. Assume further that these authors succeed in showing that disciplinary autonomy is an intrinsically valuable desideratum that is worth promoting and defending across disciplinary boundaries (see e.g. Chang, 2015, and Kusch, 2015, for a recent debate). Even so, the proposed attempts to ground opposition to SI on considerations of disciplinary autonomy face at least three unaddressed justificatory challenges.

First, the alleged fact that disciplinary autonomy is *intrinsically* valuable does not imply that it is also *unconditionally* valuable, i.e. that it is valuable irrespective of the influence that considerations of disciplinary autonomy do or may have on the involved disciplines' modelling and explanatory practices. Indeed, one can think of several cases where disciplinary autonomy does not seem unconditionally valuable in this sense. To give one example, considerations of disciplinary autonomy frequently hamper the development of epistemically productive forms of cross-disciplinary collaboration, preventing scientific theorists and practitioners from achieving epistemic and pragmatic goals they would be able to achieve through cross-disciplinary exchanges and integrations (see e.g. Ylikoski, 2014, for various examples of research projects that require cross-disciplinary expertise in the social sciences, and Wray, 2002, for analogous illustrations from the natural sciences).

Second, SI contributions often yield major modelling and explanatory *benefits* to the imperializing and/or the targeted disciplines. These benefits, in turn, may *compensate* for the loss of disciplinary autonomy involved in SI contributions even under the assumption that disciplinary autonomy has high intrinsic value. To illustrate this, consider the systematic import of analytical techniques from mathematics and physics into population ecology (see e.g. Vandermeer and Goldberg, 2003, on equilibrium-based analyses and non-equilibrium dynamics). The import of these analytical techniques reduced the autonomy of population ecology from mathematics and physics, making several results subsequently obtained by population ecologists conditional on the reliability and the accuracy of the imported techniques. Yet, population ecologists have significantly increased their models' explanatory power by importing these analytical techniques, and few population ecologists would relinquish this increase in explanatory power for the sole sake of preserving disciplinary autonomy (see e.g. Clarke and Walsh, 2009).⁵

And third, on most of the criteria for assessing disciplinary autonomy, not all SI contributions reduce or threaten the autonomy of the disciplines they target. In fact, several SI contributions *enhance* (rather than *reduce*) the

⁵ Whether the modelling and explanatory benefits yielded by SI contributions are plausibly taken to justify these contributions may depend on a number of evaluative issues (e.g. how such benefits are distributed across the imperializing and the targeted disciplines; see also point 2.4 above). As a result, many judgments about the justifiability of SI contributions are contestable. This, however, implies neither that all judgments about the justifiability of SI contributions are equally plausible nor that disagreements about such judgments are irresolvable.

autonomy of the targeted disciplines. By way of illustration, consider the wide range of evolutionary branches generated by evolutionary SI contributions across distinct social and behavioral sciences (see e.g. Alexander, 2007, and Downes, 2017, on evolutionary biology, evolutionary linguistics and evolutionary game theory). These evolutionary branches count as autonomous from the imperializing disciplines in terms of several criteria for disciplinary autonomy (see e.g. Alexander, 2007, on the irreducibility of the notions of cultural evolution targeted by evolutionary game theorists to mere biological evolution; see also Downes, 2017, on the independent journals and departments dedicated to evolutionary approaches in various behavioral sciences). The emergence of these autonomous evolutionary branches does not license the claim that SI contributions generally enhance (rather than reduce) the autonomy of the targeted disciplines. However, it indicates that considerations of disciplinary autonomy can ground opposition to a relatively narrow subset of SI contributions, and may often be taken to bear in favour of SI contributions.

Faced with these observations, a critic of SI may acknowledge that considerations of disciplinary autonomy do not ground wide-ranging opposition to SI. Still, she may object that SI contributions should be resisted on the alleged ground that they unjustifiably reduce *cross-disciplinary diversity*, i.e. the heterogeneity of the modelling practices and the theoretical perspectives entrenched across the involved disciplines (see e.g. Dupré, 1995). The idea is that SI contributions prompt unjustified restrictions in disciplines' modelling practices and tend to impose a "unique and homogeneous [theoretical] perspective" on the study of heterogeneous phenomena (Dupré, 2001, 131; see also Rosenberg, 1979, for analogous remarks against Becker's, 1976, rational choice models of family relations and racial discrimination). Suppose, for the sake of argument, that cross-disciplinary diversity is an intrinsically valuable desideratum that is worth promoting and defending across disciplinary boundaries. Even so, the three justificatory challenges facing the objection from disciplinary autonomy also plague the proffered appeals to cross-disciplinary diversity.

First, the alleged fact that cross-disciplinary diversity is *intrinsically* valuable does not imply that it is also *unconditionally* valuable, and one can think of several cases where cross-disciplinary diversity does not seem unconditionally valuable. To give one example, profound differences can be identified between the modelling practices that are respectively entrenched in economics, psychology and neuroscience (see e.g. Fumagalli, 2016a, on the modelling constructs used in these disciplines and the explanatory aims pursued by the practitioners of those disciplines). These differences, in turn, have severely hindered the development of explanatory neuroeconomic models of choice (see e.g. Kuorikoski and Ylikoski, 2010). This does not cast doubt on the alleged intrinsic value of cross-disciplinary diversity, but nicely illustrates that relying on heterogeneous modelling practices does not *per se* enable one to build more explanatory models than the models one can build by relying on discipline-specific modelling practices (see e.g. Fumagalli, 2017b, for a critical comparison of leading

economic and neuroeconomic models of choice).

Second, SI contributions often yield to the imperializing and/or the targeted disciplines modelling and explanatory *benefits* which may *compensate* for the loss of cross-disciplinary diversity putatively involved in such contributions even under the assumption that cross-disciplinary diversity has high intrinsic value. By way of illustration, many authors complain that economists' SI contributions invade the 'traditional territory' of other disciplines (see e.g. Fine, 2000, on sociology, and Lewin, 1996, on psychology) and export a "methodology that is in many cases [...] inappropriate" (Dupré, 2001, 128). However, those contributions have yielded valuable modelling and explanatory benefits not just to economists (see e.g. Lazear, 2000), but also to the practitioners of the targeted disciplines. To see this, consider economists' recent applications of constrained optimization techniques to model the activation patterns of specific neural populations across several choice settings (see e.g. Ross, 2008, on the neural substrates of intertemporal choices). These applications yielded significant modelling benefits to neuro-psychological modellers, enabling them to develop algorithmic models that predict choices even when standard economic models (e.g. expected utility theory) fail to do so (see e.g. Glimcher, 2011, ch.12-15).

And third, on many indicators of cross-disciplinary diversity, not all SI contributions reduce or threaten cross-disciplinary diversity. In fact, several SI contributions seem to *enhance* (rather than *reduce*) such diversity both by opening novel research avenues and by generating new cross-disciplinary fields. To illustrate this, consider the wide range of studies that from the 1960s onwards have attempted to systematically integrate evolutionary theory and game theoretic models to explain aspects of human behavior formerly investigated by other disciplines (see e.g. Rice and Smart, 2011, for a review). These SI contributions have enhanced cross-disciplinary diversity not just by opening novel research avenues (see e.g. Alexander, 2014, on evolutionary game theoretic studies of the emergence of signaling systems and linguistic drift, and Bicchieri, 2006, on evolutionary game theoretic studies of the emergence of social norms of cooperation), but also by generating new cross-disciplinary fields (e.g. think of evolutionary game theory).

4. Objection from the Disunity of Science

The *objection from the disunity of science* (see e.g. Dupré, 1983, 1995 and 2001) proceeds as follows. Scientific theorists and practitioners commonly aim to increase the variety of phenomena they can model and explain within their disciplines. Modelling and explanatory unifications are often regarded as major accomplishments both in general philosophy of science (see e.g. Kitcher, 1981, and Thagard, 1997) and in specific disciplines (see e.g. Bechtel and Hamilton, 2007, on biology, and Ferejohn and Satz, 1995, on economics). However, SI contributions frequently target disciplines that

sharply differ from the imperializing disciplines in terms of methods, evidential standards, and categorizations (see e.g. Roskies, 2010, on neuroscientists' SI contributions targeting philosophical conceptions of free agency). These cross-disciplinary differences, in turn, significantly constrain the modelling and explanatory relevance of SI contributions for the targeted disciplines (see e.g. Dupré, 1995). Indeed - the objection goes - the history of science abounds with cases where the lack of unification between the imperializing and the targeted disciplines hampers the modelling and explanatory relevance of SI contributions between such disciplines (see e.g. Dupré, 2001, ch.6, for a critique of economists' SI contributions targeting other decision sciences).⁶

The objection from the disunity of science correctly notes that cross-disciplinary differences may hamper the modelling and explanatory relevance of SI contributions between the involved disciplines. Even so, there are at least three reasons to doubt that this objection provides a cogent basis to oppose many instances of SI. First, it is often hard to integrate judgments of the extent to which specific disciplines differ in terms of methods, evidential standards, and categorizations into overall judgments of cross-disciplinary unification. To illustrate this, consider the ongoing debate as to whether or not economics and neuro-psychology have become more unified over the last few decades (see e.g. Bruni and Sugden, 2007, Hands, 2010, and Hausman, 1992). Disagreements about this issue stem not just from the fact that different authors use dissimilar criteria to demarcate the domain of economics and neuro-psychology, but also from profound divergences as to how judgments of the extent to which these disciplines differ in terms of methods, evidential standards, and categorizations should be integrated into overall judgments of cross-disciplinary unification (see e.g. Fumagalli, 2016b). Hence, such disagreements cannot be resolved simply by agreeing on particular criteria to demarcate the domain of the examined disciplines.

Second, even assuming that judgments of the extent to which specific disciplines differ in terms of methods, evidential standards, and categorizations can be feasibly integrated into overall judgments of cross-disciplinary unification, several SI contributions target disciplines whose methods, evidential standards, and categorizations *resemble* (rather than *sharply differ* from) those entrenched in the imperializing disciplines (see e.g. Churchland, 2007, on cognitive neuroscientists' SI contributions that aim to replace cognitive psychologists' functional models of specific cognitive capacities with neuroscience mechanistic models of the same capacities; see also Ross, 2008, on reductionist neuroeconomic models of choice which replicate the experimental protocols of the behavioral economics models they declaredly aim to supersede). Whenever this is the

⁶ Philosophers have proposed various indicators of cross-disciplinary unification, which encompass a range of ontological, axiological, and methodological elements of the involved disciplines (see e.g. Grantham, 2004, and Wylie, 1999). Here I focus on the unity of methods, evidential standards, and categorizations since most proponents of the objection from the disunity of science focus on these elements.

case, appealing to putative differences between the imperializing disciplines and the targeted disciplines does not provide a cogent basis to oppose SI contributions between such disciplines.

And third, SI contributions frequently yield valuable modelling and explanatory *benefits* even in cases where the targeted disciplines *sharply differ* from the imperializing disciplines in terms of methods, evidential standards, and categorizations. To illustrate this, consider economists' SI contributions that systematically apply constrained optimization techniques to model and explain phenomena investigated by several different disciplines (see e.g. Boudon, 2003, on sociology, Glimcher, 2011, on neuroscience, and Green and Shapiro, 1994, on political science). Some of these SI contributions merely represent (as opposed to explain) their target phenomena. Others, instead, yield insights that are plausibly regarded as explanatory under various entrenched accounts of explanation (see e.g. Ferejohn and Satz, 1995, and Fumagalli, 2017c, on the unificationist account; see also Kuorikoski and Ylikoski, 2015, and Ylikoski and Aydinonat, 2014, on the counterfactual account). That is to say, the mere fact that some SI contributions target disciplines whose methods, evidential standards, and categorizations sharply differ from those entrenched in the imperializing disciplines does not *per se* cast doubt on the modelling and explanatory relevance of such contributions. In particular, grounding cogent opposition to SI would require one to explicate how exactly the differences between the imperializing and the targeted disciplines hamper the modelling and explanatory relevance of SI contributions between such disciplines. Unfortunately, the critics of SI have hitherto failed to meet this justificatory requirement.

To be sure, some proponents of the objection from the disunity of science attempt to identify correlations between variations in the distance between specific disciplines' domains and variations in the modelling and explanatory relevance of SI contributions between such disciplines. More specifically, some defend the objection from the disunity of science by alleging that the modelling and explanatory relevance of SI contributions *reliably decreases the further away* these contributions are applied beyond the original domain of the imperializing disciplines (see e.g. Dupré, 2001, ch.1). The idea is that a discipline's theories and methods become "increasingly partial in their relevance [the] further away [they are applied] from their central areas of application" (Dupré, 1995, 380). This defense points to a *prima facie* promising way to strengthen the objection from the disunity of science, but faces at least three unaddressed justificatory challenges.

First, its proponents have yet to provide precise and defensible *metrics* to measure the distance between different disciplines' domains (see e.g. Mäki, 2013 and 2016). This lack of precise and defensible metrics, in turn, hampers the proffered attempts to ground opposition to SI on appeals to the putative distance between different disciplines' domains. Second, even assuming that some precise and defensible metric to measure the distance between different disciplines' domains was provided, it is dubious that the

modelling and explanatory relevance of SI contributions *reliably decreases* with putative increases in the distance between the domains of the disciplines involved in such contributions (see e.g. Kitcher, 1981, and Klein, 2010, on various cases of explanatory unification that encompass several different disciplines).⁷ And third, even if the modelling and explanatory relevance of SI contributions reliably decreased with putative increases in the distance between the domains of the disciplines involved in such contributions, the application of a discipline's theories and methods may still prompt (or be more likely to prompt than to hinder) *significant* modelling and explanatory *advances* across several different disciplines. In fact, as noted in this and the previous sections, the history of science abounds with cases where specific disciplines' theories and methods have been successfully applied to model and explain phenomena investigated by several different disciplines. To put it differently, neither the objection from the disunity of science nor the proffered defenses of this objection provide a cogent basis to oppose many instances of SI.

5. Objection from Counterfactual Scientific Progress

The *objection from counterfactual scientific progress* opposes SI contributions insofar as they preclude the targeted disciplines from progressing in ways these disciplines would have progressed in the absence of such contributions (see e.g. Clarke and Walsh, 2009, 195 and 201, and 2013, 342-4). Considerations of counterfactual scientific progress are frequently taken to bear on the justifiability of cross-disciplinary interactions (see e.g. Wray, 2002), and figure prominently in recent attempts to evaluate the justifiability of SI contributions (see e.g. Walsh and Boucher, 2017; see also Bowler, 2013, and Soler et al., 2015, for recent attempts to build on counterfactual history of science to elucidate how specific disciplines could have developed in the absence of particular events). Nonetheless, there are three major reasons to doubt that the objection from counterfactual scientific progress provides a cogent basis to oppose many instances of SI.

First, whether or not SI contributions are plausibly taken to promote progress in the targeted disciplines may crucially depend on *what account* of scientific progress one adopts (see e.g. Hands, 1985a and 1985b, on different accounts of progress in economics). However, dissimilar accounts of scientific progress have been proposed (see e.g. Kuhn, 1970, Lakatos, 1970, Laudan, 1977, and Popper, 1963, ch.10-11), and determining what account of scientific progress should be adopted to assess cross-disciplinary interactions is notoriously controversial (see e.g. Bird, 2007, and Cevolani and Tambolo, 2013, for recent discussions). This does not prevent the

⁷ Kitcher has more recently endorsed a more 'modest' unificationist view, according to which scientific theorists and practitioners should aim at "finding as much unity as [they] can" (1999, 339) while acknowledging that there are limits to the extent science can be unified. My reference to Kitcher's earlier works (e.g. 1981) does not commit me to endorse his later 'modest' unificationist view.

proponents of the objection from counterfactual scientific progress from identifying accounts of scientific progress that are sufficiently precise and plausible to ground evaluations of some SI contributions. Still, it severely constrains the informativeness of these evaluations whenever the accounts of scientific progress on which those evaluations rest involve controversial empirical or normative assumptions. This point holds not just for accounts of scientific progress that disagree on the possibility of providing rationally defensible criteria for assessing the progressiveness of disciplines' theoretical developments (see e.g. Laudan, 1977, versus Kuhn, 1970), but also for accounts of scientific progress that aim to provide such criteria.

To give one example, compare Popper's and Lakatos' accounts of scientific progress. Both of these accounts aim to provide rationally defensible criteria for assessing the progressiveness of disciplines' theoretical developments (see e.g. Worrall, 2002), but can ground rather dissimilar evaluations regarding such developments. For instance, according to Popper a novel theory fosters scientific progress when it has "new and testable consequences" and successfully predicts previously unobserved phenomena (1963, 241-3). For his part, Lakatos defines a series of theories as progressive if it is "consistently theoretically progressive" - i.e. each successive theory predicts some previously unexpected fact - and at least "intermittently empirically progressive" - i.e. "every now and then [the theories' predictions are] corroborated" (1970, 134). There are several respects in which Lakatos' criteria for scientific progress are less demanding than the ones proposed by Popper. For example, Lakatos requires intermittent (rather than continuous) empirical progress. Moreover, while Popper holds that only previously unknown facts count when assessing theories' progressiveness, Lakatos allows that a theory can be supported by previously known facts, provided that those facts were not employed in constructing the theory. These differences, in turn, can ground rather dissimilar evaluations regarding the alleged progressiveness of the theoretical developments prompted by SI contributions (see e.g. Hands, 1985a and 1985b, on various theoretical developments prompted by economists' SI contributions).

Second, scientific theorists and practitioners are *rarely* in the epistemic position to *establish* that SI contributions preclude the targeted disciplines from progressing in ways these disciplines would have progressed in the absence of such contributions (see e.g. Chang, 2015, and Kusch, 2015, for contrasting evaluations of what impact counterfactual developments in 18-19th century chemistry would have had on the subsequent study of energy). This problem stems not only from the difficulties inherent in providing a precise and uncontroversial account of scientific progress, but also from the limitations affecting scientists' epistemic access to counterfactual disciplinary developments (see e.g. Kidd, 2013 and 2016a). By way of illustration, ascertaining whether a discipline's SI contributions have detrimental (as opposed to beneficial) effects on the progress of another discipline requires one to estimate and assess the developmental trajectories of both disciplines in a number of actual and counterfactual scenarios. Unfortunately, several factors (e.g. the vast range of involved

counterfactual scenarios, the difficulty of reliably estimating these scenarios' probability of occurrence) constrain scientists' ability to make reliable estimates and assessments. To see this, consider recent attempts to evaluate what impact importing currently available neuro-psychological findings would have had on the development of 20th century economic theory (see e.g. Camerer et al., 2005). The limitations affecting scientists' epistemic access to counterfactual developments in economic theory significantly constrain the informativeness of these evaluations. In particular, it is difficult to see on what basis one is to ascertain how economic theory would have evolved, had it been informed by currently available neuro-psychological findings. In fact, the range of possibilities is so wide that favouring one particular counterfactual scenario would be quite arbitrary (see e.g. Fumagalli, 2016a, for a discussion of some such scenarios).

And third, on most accounts of scientific progress, imperializing disciplines often make *considerable progress* thanks to their imperialistic contributions to other disciplines. This progress, in turn, may vindicate the associated SI contributions even if they preclude the targeted disciplines from progressing in ways these disciplines would have progressed in the absence of such contributions. To illustrate this, consider recent attempts to replace cognitive psychologists' functional models of specific cognitive capacities with neuroscience mechanistic models of the same capacities (see e.g. Churchland, 2007, on individuals' processing of sensory experiences). According to some authors, these SI contributions have precluded cognitive psychology from progressing in ways it would have progressed in the absence of those contributions (see e.g. McCauley, 2007). Yet, on most accounts of scientific progress, such contributions have prompted considerable progress in cognitive neuroscientists' modelling of cognitive capacities (see e.g. Piccinini and Craver, 2011, on the predictive and explanatory benefits yielded by neuro-psychological models that systematically integrate functional and mechanistic insights about such capacities). More generally, there seems to be no general reason to think that SI contributions prevalently (or more likely) hamper - as opposed to promote - progress in the disciplines they target. Indeed, as noted in Section 3, SI contributions often prompt significant modelling and explanatory advances in the targeted disciplines. Whenever this is the case, considerations of counterfactual scientific progress do not ground cogent opposition to SI, and may actually support SI contributions unless one is able to demonstrate that the targeted disciplines would have made even more progress in the absence of such contributions.

Faced with these observations, a critic of SI may acknowledge that some SI contributions prompt significant progress in the disciplines they target. At the same time, she may draw on progress-related axiological considerations to object that SI should be resisted when it hampers or prevents the expression of important *moral and social values* in the targeted disciplines (see e.g. Clarke and Walsh, 2009, 195 and 204, and 2013, 342 and 348). Considerations of moral and social values frequently figure in assessments of cross-disciplinary interactions (see e.g. Elliott and McKaughan, 2014,

Kitcher, 2011, and Parker, 2014). Even so, appealing to moral and social values does not ground wide-ranging opposition to SI, and may actually support SI contributions. To see this, consider a recent proposal to regard SI as morally wrong “when it gives rise to [unfair] distribution of credibility”, i.e. when it leads to a specific kind of epistemic injustice consisting in “a mismatch between credibility and expertise in a particular domain” (Rolin, 2017, 1; see also Fricker, 2007, and Kidd et al., 2017, on distinct kinds of epistemic injustice and their interrelations). This proposal aptly emphasizes the implications that considerations of epistemic justice may have for the justifiability of some SI contributions. Still, the proffered attempts to ground opposition to SI on considerations of epistemic justice invite three interrelated rejoinders.

First, *many criteria* have been proposed to assess whether cross-disciplinary interactions lead to unfair distribution of credibility (see e.g. Kusch, 2015, on social indicators of reliability and institutional status), and there may be widespread reasonable disagreement as to how SI contributions fare in terms of distinct criteria (see e.g. Davis, 2017, on various debates about the justifiability of economists’ SI contributions). Second, it remains unclear *which groups* of scientists (e.g. practitioners of the imperializing disciplines, practitioners of the targeted disciplines, putatively independent experts, etc.) are best equipped to judge whether SI contributions lead to unfair distribution of credibility, and by means of what evaluative criteria one is supposed to adjudicate conflicting judgments about this issue. And third, there seems to be no general reason to think that SI contributions prevalently (or more likely) *exacerbate* - as opposed to *remedy* - previous epistemic injustice. In fact, on many criteria for assessing epistemic justice, SI contributions frequently appear to remedy (rather than exacerbate) previous epistemic injustice. By way of illustration, consider again the evolutionary branches generated by evolutionary SI contributions across distinct social and behavioral sciences (see Section 3). These SI contributions challenged a wide range of less predictive and explanatory theories that were formerly entrenched in the targeted disciplines, thereby contributing to realign those theories’ credibility with their actual epistemic performance (see e.g. Downes, 2017, on evolutionary biology’s influence on various theories in biology; see also Downes, 2015, on evolutionary biologists’ criticisms of prominent works in evolutionary psychology). That is to say, considerations of epistemic justice may bear on the justifiability of some SI contributions, but do not ground wide-ranging opposition to SI, and may often be taken to support SI contributions.

6. Objection from Cumulative Constraints

The *objection from cumulative constraints* subordinates the justifiability of SI contributions to the satisfaction of a series of cumulative evaluative constraints, namely the ontological, axiological, institutional and epistemological constraints (see e.g. Mäki, 2009 and 2013). The idea is that SI contributions are justifiable if they satisfy all these four constraints, and

are instead to be resisted or opposed if they fail to satisfy one or more of those constraints (see e.g. Mäki, 2009, 373, and 2013, 336). In this section, I assess each of the aforementioned four constraints in turn. I shall argue that those constraints improve on former attempts to evaluate various instances of SI, but are articulated at an exceedingly high level of abstraction to ground cogent opposition to SI and identify what features allegedly make SI unjustified. This does not preclude the proponents of these constraints from refining the formulation of those constraints so as to license informative verdicts regarding specific instances of SI. Still, as I argue below, the proposed refinements of such constraints face non-trivial challenges. As a result, the proponents of the objection from cumulative constraints should either develop further refinements of the proposed constraints or modify the justificatory requirements associated with such constraints (e.g. allow that some SI contributions may be justified even if they fail to satisfy some constraints, limit the intended scope of the proposed constraints to specific sets of SI contributions, etc.).⁸

The *ontological* constraint subordinates the justifiability of SI contributions to their ability to foster so-called ontological - as opposed to mere derivational - unification. The idea is that these contributions should “successfully represent how things are related in the causal structure of the world”, rather than merely derive “large classes of explanandum sentences from a parsimonious set of theoretical sentences” (Mäki, 2009, 363-4). The following two interpretations of the *ontological* constraint have been proposed (Kuorikoski and Lehtinen, 2010, 349-350). On the one hand, the *structural interpretation* holds that the ontological constraint is met whenever the phenomena investigated by the imperializing and the targeted disciplines exhibit similar structural characteristics (e.g. equilibria with equivalent mathematical descriptions). On the other hand, the *causal interpretation* holds that the ontological constraint is met only when the phenomena investigated by the imperializing and the targeted disciplines share a common causal basis (e.g. think of the micro-physical substrates of the behavioral patterns studied by distinct decision sciences). Neither of these interpretations seems to provide an informative basis for assessing the justifiability of SI contributions. More specifically, the structural interpretation targets properties that are instantiated by too many and overly diverse systems to be plausibly regarded as an informative evaluative criterion (see e.g. Kuorikoski and Lehtinen, 2010, on the vast variety of

⁸ I am not concerned here with discussing which of these two argumentative strategies should be pursued by the proponents of the objection from cumulative constraints. For my evaluative purposes, I just note that my appraisal of Mäki’s constraints differs from other appraisals (see e.g. Davis, 2012), which hold that since these constraints are unlikely to be satisfied, endorsing such constraints supports an exceedingly conservative position regarding the justifiability of SI contributions. To be sure, one might agree that a literal interpretation of Mäki’s constraints could yield implausibly restrictive verdicts about the justifiability of SI contributions. This, however, does not exclude that a more nuanced reading of those constraints may avoid this pitfall. In particular, it does not imply that Mäki’s strategy of evaluating SI contributions by specifying constraints on cross-disciplinary interactions is “itself [...] problematic” (Davis, 2012, 216).

systems that exhibit equilibria with equivalent mathematical descriptions). Conversely, the causal interpretation seemingly overlooks that SI contributions may be justified even when the phenomena investigated by the imperializing and the targeted disciplines lack a common causal basis. To see this, consider again the explanatory insights yielded by economists' SI contributions that systematically apply constrained optimization techniques to model and explain phenomena investigated by several different disciplines (see Section 4). The mere fact that constrained optimization techniques can be used to model phenomena investigated by other disciplines falls short of implying that these techniques provide causal explanations of such phenomena (see e.g. Ylikoski, 2013). Even so, those techniques enabled economists to provide insights that are plausibly regarded as explanatory under various entrenched accounts of explanation (see e.g. Ferejohn and Satz, 1995, and Fumagalli, 2017c, on the unificationist account; see also Kuorikoski and Ylikoski, 2015, and Ylikoski and Aydinonat, 2014, on the counterfactual account). These explanatory benefits, in turn, bear in favour of a range of economists' SI contributions (see Section 4), thereby casting doubt on the causal interpretation's implication that SI contributions may be justified only when the phenomena investigated by the imperializing and the targeted disciplines share a common causal basis.⁹

The *axiological* constraint states that due to variations in the epistemic, moral and social significance of the facts explained, a theory that explains a higher number and variety of facts "is not always a better theory" (Mäki, 2009, 369). The thought is that "theories that unify insignificant phenomena [by] ignoring or marginalizing significant ones are much less supportable than those that unify significant phenomena at the expense of less significant ones" (Mäki, 2013, 337). This constraint provides a basis to integrate considerations of epistemic, moral and social values into the assessment of SI contributions. Nonetheless, as it stands, such constraint does not offer detailed guidance on how to weigh and trade-off epistemic, moral and social values in assessing SI contributions. This lack of guidance is problematic, since many such values influence the justifiability of SI contributions (see e.g. Steel, 2010, on cases where non-epistemic values override epistemic considerations in determining whether SI contributions are justifiable) and often have conflicting influences on the justifiability of SI (see e.g. Elliott and McKaughan, 2014, on the trade-offs between epistemic and non-epistemic values involved in choosing between distinct risk assessment procedures in various policy contexts; see also Hertwig and Ortmann, 2008, on how ethical and methodological bans on lying to

⁹ The causal and structural interpretations do not exhaust the set of possible interpretations of the ontological constraint, so my critical evaluation of these two proposed interpretations does not exclude that one may provide precise and plausible interpretations of this constraint. Even so, my critical evaluation challenges the proponents of such constraint to provide more precise and plausible interpretations. In the absence of such interpretations, my critical evaluation can be provisionally taken to cast doubt on the informativeness of the ontological constraint.

experimental subjects constrain what insights SI contributions can yield in experimental studies with human subjects).

The *institutional* constraint proscribes against SI contributions exhibiting “dismissive hegemonic arrogance” towards the targeted disciplines and against “intellectual monopolies protected by non-argumentative means of exclusion” (Mäki, 2013, 337). This constraint nicely fits several authors’ emphasis on the epistemic and pragmatic benefits of cross-disciplinary collaboration (see e.g. Longino, 2002, Rolin, 2015, and Wray, 2002). At the same time, attempts to ground opposition to SI on such constraint invite three main rejoinders. First, the formulation of the constraint crucially relies on generic metaphorical expressions (e.g. ‘dismissive hegemonic arrogance’), which hinders the constraint’s ability to license informative verdicts regarding the justifiability of many SI contributions (see e.g. Kidd, 2016b, for similar remarks about the practice of epistemic vice-charging).¹⁰ Second, on most commonsense interpretations of ‘dismissive hegemonic arrogance’, only a relatively narrow subset of SI contributions appear to exhibit dismissive hegemonic arrogance towards the disciplines they target. By way of illustration, consider again economists’ SI contributions to other behavioral and social sciences. Some proponents of these SI contributions exhibit what many regard as dismissive hegemonic arrogance towards the targeted disciplines (see e.g. Becker, 1976, for the claim that economic theory can be used to explain ‘all human behavior’). Others, instead, do not exhibit this attitude, and cautiously guard other economists against claiming “more than [they] can deliver” (Binmore, 1999, F17). And third, the issue whether SI contributions exhibit dismissive hegemonic arrogance towards the targeted disciplines seems to have only indirect and limited relevance for the justifiability of such contributions. In particular, one may agree that dismissive hegemonic arrogance is a normatively objectionable attitude, and yet maintain that many SI contributions are justifiable irrespective of whether their proponents exhibit such attitude. This does not exclude that violations of the institutional constraint may bear on the justifiability of some SI contributions (see e.g. Kidd, 2017, and Mäki and Marchionni, 2010, on cases where positions of academic hegemonic dominance confer to the dominant disciplines competitive advantages that do not reflect their actual epistemic performance). Still, it makes it questionable whether the institutional constraint licenses informative verdicts regarding the justifiability of many SI contributions.¹¹

¹⁰ To give one example, consider Mäki’s claim that “within an appropriate institutional framework there is little reason to worry about imperialistic trespassing” (2013, 337). This claim seems *prima facie* plausible, yet specifies neither what an ‘appropriate’ institutional framework consists in nor by means of what criteria one is supposed to establish whether any given institutional framework is ‘appropriate’ in the to-be-specified sense. As a result, different authors may nominally endorse such claim and yet radically disagree as to what SI contributions are justifiable and what criteria one should employ to assess the justifiability of SI contributions.

¹¹ This informativeness concern exacerbates when one examines the proffered attempts to apply the institutional constraint in concrete situations. By way of illustration, consider Mäki and Marchionni’s claim that “too much homogeneity

Finally, the *epistemological* constraint “advises against dogmatic commitment and recommends a strong sense of fallibility and openness to critical conversation across disciplinary boundaries” (Mäki, 2009, 373). The idea is that since it is difficult to determine whether SI contributions meet the other three constraints, one should subject the background assumptions of SI contributions (e.g. what constitutes an adequate explanation of the examined phenomena, whether the employed modelling techniques yield reliable insights concerning these phenomena) to criticism and revision even if she believes that these contributions meet such constraints (see e.g. Mäki, 2009, 370-2, and 2013, 336-7). I view with favour the anti-dogmatic commitment recommended by the epistemological constraint. However, I think that attempts to ground opposition to SI on this constraint face at least two major challenges. First, the formulation of the constraint crucially relies on generic metaphorical expressions (e.g. ‘strong sense of fallibility’, ‘openness to critical conversation’), which hinders the constraint’s ability to license informative verdicts regarding the justifiability of many SI contributions. And second, the following dilemma hampers attempts to specify what sets of background assumptions should be subject to criticism and revision for SI contributions to be justifiable.

On the one hand, simply claiming that one should subject *some* of the background assumptions of SI contributions to criticism and revision is overly vague to license informative verdicts regarding the justifiability of many SI contributions. On the other hand, claiming that one should subject *all* (or even *most* of) the background assumptions of SI contributions to criticism and revision imposes overly stringent requirements on the justifiability of SI contributions. For in primis, scientific theorists and practitioners frequently lack a shared cross-disciplinary framework to subject all (or even most of) the background assumptions of their SI contributions to criticism and revision (see e.g. Fumagalli, 2016b, on how the lack of a shared explanatory framework across economics, psychology and neuroscience impedes the evaluation of some neuroeconomists’ SI contributions to economic modelling). And second, significant modelling and explanatory advances have been prompted by SI contributions that did not subject all (or even most of) their background assumptions to criticism and revision. To give one example, social epistemologists have made significant advances in systematically extending rational choice models to explain scientists’ division of cognitive labor (see e.g. Muldoon and Weisberg, 2011, Weisberg and Muldoon, 2009, and Zollman, 2007). Yet,

and closed dogmatism [...] would discourage the creation and pursuit of [...] possibly fruitful lines of inquiry [whereas] too much heterogeneity and criticism would also be inadvisable” (2010, 12). This claim seems *prima facie* plausible, yet clarifies neither what ‘too much’ homogeneity and heterogeneity consist in nor by means of what criteria one is supposed to establish whether the homogeneity or heterogeneity found in specific cross-disciplinary contexts is ‘too much’. As a result, different authors may nominally endorse such claim and yet radically disagree as to what SI contributions are justifiable and what criteria one should employ to assess the justifiability of SI contributions.

these SI contributions do not subject all their background assumptions to criticism and revision, and it is hard to see why this fact alone would make those SI contributions unjustified.¹²

7. Conclusion

Over the last few decades, several objections have been put forward to ground opposition to SI and identify what features allegedly make SI unjustified. In this paper, I argued that these objections provide a valuable basis for opposing some instances of SI, but do not yield cogent reasons to think that SI in general is objectionable or unjustified. If correct, this result supports at least three wide-ranging implications for the ongoing philosophical debate about the justifiability of SI. First, the critics of SI should provide more informative criteria for assessing SI and ground their opposition to SI on more plausible empirical and normative presuppositions. Second, what (if anything) makes some SI contributions objectionable does not lie in their imperialistic character, but rather relates to putative flaws in their empirical or normative presuppositions and to the unwarranted social or pragmatic implications some derive from such contributions. And third, the justifiability of SI contributions is best judged on the basis of specific case studies rather than general evaluative criteria that abstract away from the modelling and explanatory practices of the examined disciplines. These three implications do not exclude that one may gain informative insights by assessing specific sets of SI contributions *qua* imperialistic contributions. Still, they should make scientific theorists and practitioners wary of general calls either in favour of or against SI contributions. In particular, they challenge all those who debate about SI to further refine the criteria proposed for evaluating the justifiability of SI and focus their evaluations on specific subsets of SI contributions.

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¹² The fact that social epistemologists' SI contributions do not subject all their background assumptions to criticism and revision has led some to criticize these SI contributions for relying on questionable background assumptions (see e.g. Alexander et al., 2015). Still, most of the critics concur that the justifiability of those SI contributions depends not so much on whether their proponents subject all their background assumptions to criticism and revision, but rather on the actual empirical and normative plausibility of such assumptions.

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