Critical Review

Frans H. van Eemeren & Bart Garssen (Eds.). 2008. *Controversy and Confrontation. Relating Controversy Analysis with Argumentation Theory*. Amsterdam/Philadelphia: John Benjamins. (xiii, pp. 1-278; ISBN 978 90 272 1886; US$158.00/€105.00)

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1. Introduction

The plan and reason of this collection is to strengthen the connection between the study of argumentation and that of controversy. The latter deals with what a Pragma-dialectician knows as a "mixed difference of opinion" or a "persistent conflict" (p. 2): "It seems to us [the editors] intrinsic in a controversy that it concerns a difference of opinion that is perceived to have acquired a state of quasi-permanency—a state of 'lingering on'" (ibid.). Discounting the introduction, fourteen essays discuss theoretical and empirical aspects of the study of controversy, partly incorporating the Pragma-dialectical theory.

Already in the first paragraph of the preface, the editors are careful to point out: "*When taken together*, the papers provide a closer insight into the relationship between controversy and confrontation that deepen our understanding of the functioning or argumentative discourse in managing differences of opinion" (p. VII, *italics added*). It is the italicized part which makes the sentence true. Understandably, none of the papers is written from an encompassing perspective, as such has yet to be developed.

To this end, the editors provide a 24 page introduction featuring short summaries, brief mentioning of central names and themes as well as attempts at drawing thematic connections. They also provide two helpful tables. One lays out preconditions of strategic maneuvering in argumentative discourse in argumentative discourse types belonging to the clusters of adjudication, mediation

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and negotiation (p. 12). The other is a characterization of Dascal's ("the grand old man of the study of controversy", p. 1) three types of argumentative confrontation—discussion, controversy, dispute—as specific and prototypical cases of well-recognized argumentative activity types (p. 22).

These charts make for good teaching material and basically purport that Pragma-dialectics is (already) applicable to the study of controversy (perhaps with minor adjustments). The prospects of joint efforts will likely come down to adopting the analytical and evaluative tools provided by Pragma-dialectics in exchange for case studies. It is less clear what beyond examples the latter field might provide to the former.

In the following, the papers are discussed individually in the order in which they appear. My aim is to provide an accurate presentation of the gist, followed by critical remarks on main claim(s) of a paper. A brief overall evaluation is in the last section.

2. The Papers

2.1 In *Dichotomies and Types of Debates*, Marcelo Dascal argues that our persistence in treating dichotomies logically, in the sense of the excluded middle (*tertium non datur*), "is quite revealing about the aims and practices of various types of debate" (p. 30). He proposes to include *dichotomization* and *de-dichotomization*—not as entities occurring, but as strategies pursued—among the characterising features of the debate-types *discussion*, *dispute* and *controversy* (p. 47). In particular, de-dichotomization is said to be typical *only* of controversy, which is presented as a new, result-wise open, content-wise most flexible and otherwise most dogma-challenging debate-type, because "the questioning of basic assumption of all sorts is always possible" (p. 46).

Being faced, in contemporary discourse, with (the use of) dichotomies—read: mutually exclusive and purportedly exhaustive alternatives—is briefly traced to Plato, more precisely to his method of defining according to the *genus proximum plus differentia specifica* scheme. The method is then criticized for the arbitrariness of "determining where to stop division (be it dichotomous or not) while at the same time ensuring it provides a full account of reality" (p. 29). This leaves "the use of the notion of dichotomy as the flagship of [Plato's] dialectical method […] far from being able to provide this method with a rigorous formal foundation" (p. 33). Thus, as most would readily agree to: *Any* dichotomy always remains open to doubt.

According to Dacal's working definition, *dichomatization* is the "radicalization of a polarity by emphasizing the incompatibility of the poles and the inexistence of intermediate alternatives, by

stressing the obvious character of the dichotomy as well as of the poles to be preferred" (p. 34). *De-dichotomization*, on the other hand, consists in "showing that the opposition between the poles can be constructed as less logically binding than a contradiction, thus allowing for intermediate alternatives; actually developing and exemplifying such alternatives" (p. 35).

Next to anecdotal evidence, his examples of the strategic use of (de-)dichotomization include material from the natural right vs. historicism debate, the Newton-Hooke controversy over the theoretical interpretation of Newton's prism experiment (see Zemplén's essay, section 2.14) as well as Putnam's attack on the fact vs. value (Hume's is-ought) distinction. At this point, Dascal fully sides with Putnam's recommendation to *disinflate* dichotomies, until we are left with a simple distinction that may or may not be useful in a given context. In particular, we learn, one should relativize dichotomies to the context in which they are applied, rather than see them as absolute(s).

With *discussion* (characterized as a logical, rational and decidable scientific debate aimed at truth about contents) and *dispute* (a rhetorical, irrational, undecidable battle of wit about attitudes aimed at victory—catchphrase: *my* truth) claimed to be "two ideal types of debate traditionally viewed as dichotomously related" (p. 42), Dascal attempts (what he terms) a de-dichotomization at the meta-level, by introducing the notion of *controversy* as a third ideal debate-type. This occurs on the basis of the claim that "the models of *discussion* and *dispute* alone are not sufficient for an account of all varieties of debates" (p. 45) and that "the meta-level categorization of a debate will influence the actual conduct of the debate" (p. 42).

Inspired by work of Leibniz, Dascal locates the primary aim of controversy not in a decision (on truth or a debate-winner), "but rather [in] the construction and emergence of a solution through the dialectic cooperation of the debaters" (p. 45). Meta-theoretically, and *vis à vis* discussion and dispute, then, "the fundamental difference to be stressed is the fact that its [controversy's] defining parameters, contrary to those of its partners in the triad, are all non-dichotomous in nature" (p. 46).

In a nutshell, following a dichotomizing strategy leads one into either discussion or debate; following a non-dichotomizing strategy leads to controversy (which can be discrete or continuous). This two-pronged division of strategies looks suspiciously like another dichotomy, especially since Dascal re-uses the term '*de*-dichotomizing' (see the table on p. 47). Consider that '*non*-dichotomizing' may already be a good enough term which entails neither the absence or inexistence, nor the impossibility of $n+2$-ary options.

Consequently, Dascal's ensuing question ("Have we ended up re-dichotomizing what we undertook to de-dichotomize", p. 47) appears out of place—at least to the analytically minded reader. *Pace* the unorthodox genesis of controversy as a third debate-type through a process of (possible) *re*-dichotomization of what had been *de*-dichotomized, however, this I find little beyond a variation on the insight that thinking in and arguing with dichotomies is neither good nor bad, but mediocre.

2.2 In *Charles Darwin versus George Mivart, The role of polemics in science*, Ann Carolina Regner aligns her research with that of Marcello Dascal and Marcello Pera. From Dascal, she adopts the position that "controversies are (…) 'quasi-dialogical' in the sense that, over and above the participants, there is a third party, [namely] the audience as finale arbiter (the scientific community)" and that "[u]nlike dispute, the aim of controversy is (…) to win (…) by using rational persuasion, and that this condition (…) contingently shapes 'reasonable' argumentative praxis (…)" (p. 53). From Pera, she adopts that controversies are "necessary for rationality, whose scope goes much further than the limits of deductive demonstration", and advertises a "view of science based on the role of argumentation, rather than on method as a rigid set of rules (…)" (p. 54).

Regner's interest pertains primarily to the dialectical rules and rhetorical arguments in (Pera's) three party game of science, the rough game-plan of which is: 'We ask nature questions, nature answers, the scientific community decides the official answer'. This view, or so would be the strong claim, harbors insights richer than those to be gained on the "methodological view centered on strict deductive and inductive patters" (ibid.)—a view which, we are led to infer, *depends* on the dialectical and the rhetorical (see p. 56, second paragraph). Here, the terms 'dialectic' and 'rhetoric' are allegedly used in their Aristotelian meaning, arising from his *Topics,* as "the art of persuading, by means of which the dialectical debate is carried on" (p. 54.) and "the art and logic of arguing in a debate concerned with a change of belief, and of providing a code for judging good and bad arguments" (ibid.).

The message is that "[scientific] controversies are not deprived of rules, but the dialectical rules they obey are like Aristotle's *topoi*" (p. 56). It appears that, amongst others, it is this assumption which motivates Regner to state that "[t]he persuasive efforts of both proponent and opponent are "(…) *essential* to the verdict on the acceptability of a particular scientific explanation" (ibid*., italics added*). This view seems to leave no room for veridical considerations in (empirical) theory choice, nor does it assigns any role to constraints such as empirical adequacy, simplicity, scope, fruitfulness, etc. I therefore perceive this approach to treat the risk

of "discarding the baby with the bath water" rather carelessly, by seeing parties to be divided by "a rift that makes it impossible to achieve the ideal resolution or a reconciliation of their standpoints" (p. 74).

After all, it is well-accepted at least since Duhem (1906, 1954) that empirical theories are underdetermined by the data (phenomena) they are meant to cover (save). However, this is a far cry from regarding the acceptability of a scientific explanation to depend *essentially* on persuasive efforts. At this point, it does not help that the deductive scheme of prediction appears to be ill-understood. Thus consider:

> [I]f P [prediction] follows deductively from T [theory] and R [random sentence (?)], and T is accepted, the acceptance of P is a matter or persuasion, a resort to an argument from authority, unless R is also proved. Moreover, expressions like 'is based on', is 'consistent with' or 'logically follows from' are very vague, so that to render compelling an argument where they occur requires highly persuasive devices. (p. 55)

More realistically, acceptance of P will be a matter of observation paired with conventional definition, rather than proof. Moreover, 'consistent with' and 'logically follows from' really should *not* be considered vague; perhaps 'based on' might be so considered. At any rate, highly persuasive devices (whatever these are), if required, would seem to be required for other reasons than the ones Regner gives. Moreover, her use of the categories *pathos*, *ethos*, *logos* is difficult to follow and appears (to me) arbitrary.

In altogether 18 pages, Regner takes the reader through the Darvin-Mirvart controversy, by listing, in order, Darwin's and Mirvart's problems, answers, motivations, presuppositions, their respective general arguments and argumentative strategies, as well as their objections and responses. Though presented in rather rich detail, the author aims more at listing all sorts of interesting features in an exhaustive manner, rather than engaging with the arguments directly—a task that would go beyond the space allocated to her.

The paper will likely speak only to someone with a rich background knowledge of the particular debate. Precisely because detailed work cannot occur in a short paper, the reader is left wondering *exactly* how the author arrives at her conclusion, according to which "Darwin won the debate [or was it, rather, a 'controversy'?], not because the theory of natural selection was no longer disputed by his audience, but because of his attitude towards science and man" (p. 75).

All in all, we learn that Mirvart was looking for the "*tertium quid* to provide a comprehensive and conciliatory view of the

genesis of species which 'will completely harmonize with the teachings of science, philosophy and religion' (Mirvart 1871, p. 15)" (p. 59). Darwin, on the other hand, sought to explain the natural entirely without the supernatural. Both used various argumentative strategies.

Short of studying the debate/controversy ourselves, we must hope that the author will lay out her views in a book-length treatment soon. Such a book might consider, whether (the explanatory power yielded by) the three basic tenets of Darvinism (*random variation, inheritance, natural selection*) were simply too weak to avoid controversy, without thereby confirming that theory choice/theory acceptance is *essentially* dependent on rhetorical and dialectical factors. Again, my point is not there clearly is no such dependence at all, but that it takes more to argue for the strong positive thesis than she provides in this chapter.

2.3 In *Scientific demarcation and metascience, The national academy of sciences on greenhouse warming and evolution*, Thomas M. Lessl treats the debate on the demarcation of science from pseudo science—contributions to which he calls "boundary-work"—*vis à vis* the global warming debate. He does so with particular respect to rhetorical effect, treating in some detail two of the American National Academy of Science's publications: the 1998 *Teaching about Evolution and the Nature of Science* and the 2001 *Climate Change Science: An Analysis of Some Key Questions*. The former is said to be addressed to a general audience; the latter is meant primarily for government officials.

In brief 14 pages, he breifly examines "how the constitutive effects of boundary-work detected in one scientific publication intended for broad distribution might affect public judgement of another message that demands greater discernment" (p. 80) and considers "what would result if the understanding of science developed in the first publication [on science vs. pseudo-science] were to have effective presence for those reading the second NAS publication [on global warming] (…)" (p. 81). Here, 'effective presence' denotes an idea of Perelman's (1982, p. 35f.), according to which "arguments intended to achieve immediate persuasive goals may also have presence in other contexts which their authors cannot foresee" (p. 80).

Lessl's argument is roughly as follows: In the science/pseudo-science publication, the national academy of science claims evolutionary theory to be supported (technically: confirmed) by ever more precise measurement-based evidence in a way which renders the theory beyond doubt and to portray the process leading to this state of affairs as one that fits under a (naïve) *accumulation view of scientific knowledge* (see Kuhn 1970).

The author's criticism is that the meta-scientific view adopted in the first NAS publication is that of Francis Bacon which, in turn, is outdated and, basically, false. "A simplistic Baconian model which views theories as springing up spontaneously from data is preferred [in this publication] in spite of the clear inability to genuinely 'save the phenomena' of scientific history" (p. 86). Rather, e.g., for the specific case of Galileo's heliocentrism—widely accepted as *the* historical step in distancing modern science from both religion and Aristotelian conceptions of nature—, it rather is true that "it was an argument that marshalled all the available means of persuasion, hard evidence as well as soft speculation" (p. 86). The suggestion, backed by citing Pera (1994), then, is that scientific progress is in essential respects rhetorical (see Regner's article, section 2.2).

Lessl's ensuing "hermeneutical thought experiment" (p. 86) consists in speculating that, in the debate on climate change caused by human pollutants (a.k.a. 'global warming'), the meta-scientific standards pertaining to the evolution debate fill the (metaphorical) vacuum presumed to characterize the mind of the public. Quite simply:

> [S]ince the greenhouse gas debate does not invite such considerations [as are present in the debate on the nature and demarcation of science], public participants will be inclined to fill this vacuum with conceptions of science that they have appropriated elsewhere. In such rhetorical situations metascientific work such as we have seen in the NAS book on evolution will be drawn into this vacuum – thus having effective presence. (p. 87)

Furthermore, as the second NAS book is of a much more "prudent tone" (p. 88) than the first, and—or so is Wessl's claim—seeks to describe the human-factor in global warming as merely a tentative answer of rather substantial uncertainty, what would the lay person come to think, given she applies the meta-scientific standards used in the evolution booklet to the issue presented in the global warming booklet?

Alternative to thinking whatever it is that the public thinks if one lets it—here nicely glossed as "conventional modes of judgement" (p. 90)—, the public would be "just as likely to fill this empty conceptual space by bringing to this [greenhouse] message conceptions of the nature of scientific knowledge that come from sources like the NAS book on evolution" (ibid.). Of course, the immediate result would be: "Were they to do so, they would likely judge as weak a case for greenhouse gas emissions as the factor responsible for rising global temperature" (ibid.).

The moral: At the end of the day, it remains the scientist's job to educate the general public on "how to best judge their findings" (ibid.), i.e., to provide "scientific literacy", although "we should likewise expect that the metascientific tools with which they equip the American public will not be up to the task of discerning complex issues like global warming" (ibid.).

It appears that the basic insight behind Lessls criticism of the meta-scientific standards presumably at work in the evolution case is what I have cited above (section 2.2) as Duhem's idea, according to which an empirical theory is always underdetermined by its evidence. This means, roughly, that there are several ways to fit a curve to a given set of data points. Mathematically, there are infinitely many ways—some of which are considered ugly. It is this very argument which one can reconstruct in debate contributions from global warming skeptics.

Moreover, Lessl's exemplary observations, e.g., on the non-confirmability of Newton's first law (bodies not acted upon by external forces move in straight trajectories with constant acceleration) and the non-falsifiability of the second law (the net force of a system of motion equals the system's masses times their accelerations) are simply correct. However, these observations are *not* very widely appreciated outside the history and philosophy of science community. Generally, the non-metalevel scientist continues to confirm her theories naïvely—come hell, high water or inductive logic.

Overall, with respect to the meta-scientific standards actually at work and, one hopes, to be taught in class rooms, I believe that Lessl has merely scratched the surface. In particular, Michael Friedman's (2001) notion of *methodological* or *relativized a apriori* could be fruitful to (update) science education, which here appears somewhat mid 20th century, really. At the same time, e.g., on Friedman's view for example, it is hard to see that enough room for genuinely rhetorical considerations opens up that could *not* equally be covered by considerations of scope, adequacy, fruitfulness, etc. Again, the reader may find that the *strong* claim for the rhetorical *nature* of the scientific enterprise (as opposed to the human enterprise, for example) needs more support.

2.4 In *Reforming the Jews, rejecting marginalization – The 1799 German debate on Jewish Emancipation in its controversy context*, Mirela Saim presents "a 'triangular' controversy between [the] contemporary opinion leaders" (p. 93) David Friedländer, Wilhelm Abraham Teller and Friedrich Schleiermacher as "a case study in the history of controversies" (ibid.) which revolved around a praxis known as *baptism of convenience*. The controversy arose with Friedländer's "proposal of having Jews convert collectively to Christianity, yet without fully endorsing the dogmatic content of

the Christian (Protestant) religion" thus constituting a "sort of 'baptism light' [which], clearly opportunistic, would impose only limited doctrinal restrictions while offering full civic integration in the mainstream Berlin society" (p. 95).

Friedländer's open letter, which is addressed to Teller, "a liberal Protestant thinker" (p. 100), is said to criticize the Halachah (the normative side of Judaism), and thus the late 18th century European Judaism, as backwards oriented, thereby explaining the sad economic and social state of the majority of Jews under the diasporic (stateless) condition. Saim interprets Friedländer to propose that religion be viewed "as an index of public expression (…) [rather than] private 'inner' religious belief (…) [thereby] "creating a space of *indifference* towards the authenticity of religious commitment" (p. 100).

Teller replies that social integration of Jews does not require formal conversion to Protestantism and, or so we learn, that Jewish moral progress is a matter only for the Jewish community to deal with, thus "rejecting formal conversion as an unnecessary and (…) inauthentic solution" (p. 101). Schleiermacher replies by asking if Friedländer's proposal, next to being unnecessary—"it must be possible in many ways to be a citizen and a non-Christian" (p. 102) —is perhaps meant as a "rhetorical scheme devised to attract attention to the plight of the Jews" (p. 102). Thus, or so is Saim's interpretation:

> Both theologicians (…) deny that in a tolerant society baptism —in any form—could be used as a modality of access into civil society. Their refusal of a convenience conversion is motivated by their symptomatic assessment of the new social reality that, in their experience, already grants tacitly to the 'Enlightened Jews' the enjoyment of equal civic rights. (p. 104)

Saim submits an interpretation of the controversy as one which was "inspired by the search for a fast and practical solution to a degrading reality" which then came to consider all possibilities, "no matter how extreme or unlikely" (p. 105). This, we learn, are the signs of discussion *and* debate at the same time, "thus qualifying as a controversy" (p. 105). As a historical note, in 1812, citizenship rights were granted to Jews in Germany.

Although 'frame', 'strategy' and 'argumentation', for example, are used as key terms, this terminology seems to shed little light on the controversy. Though well organized and plainly written, it seems to me that Saim's article has not altogether delivered on the second part of her initial claim, according to which the "1799 debate does signal a crucial stage in the long tradition of interfaith Jewish-Christian debates and for this reason alone it deserves an

argumentation scrutiny, concerning both its topical design and its procedures for assessment and validation" (p. 94).

Finally, the author seems to hold a traditional view of 'rhetoric', for example when writing: "In view of the parallel texts coming from the French revolutionaries (Abbé Grégoire, amongst others) and from various other Jewish thinkers I tend to think that Friedländer was only examining Jewish options …rhetorically!" (p. 106). The term 'rhetorically' is here used in the meaning of 'deceivingly' (as opposed to 'truly' or 'truthfully'). I suggest that 'rhetorically' can, without loss to her analysis, be replaced by 'strategically'. It would then denote a mode of (inter)action which is no longer associated only with the field of rhetoric. In my opinion, this would be an improvement.

2.5 In *Communication principles for controversies*, *A historical perspective*, Gerd Fritz briefly summarizes research on and discusses two examples of communication principles (see below), as they are forthcoming in historical case studies, mainly on academic texts from the 16th to the 18th century. Very roughly, these principles are Grice's maxims of conversation (p. 110) enriched by Hintikka's (1986) and Kasher's (1976) rationality principle, which Fritz treats in *Historical Pragmatics*. These principles

> form highly complex families which are differentiated according to social groups (e.g., scholars vs. courtiers) and types of texts (pamphlets vs. reviews) etc., and which, for good reasons—this is a basic assumption of this paper—are historically variable. (p. 110)

Thus, or so one may understands the key idea, if it is agreed that controversies are historical events constrained by communication principles, then the study of controversies yields insights into their temporal dynamics.

Listing no fewer than 24 examples, in passing, Fritz mentions a typology of communication principles (logical, dialectical, rhetorical, hermeneutical principles, those of text production, linguistic and politeness principles), but cautions that "these labels only give a vague indication of the type and background of the respective principle" (p. 111). Some of his examples can readily be discerned as *specific* norms of good argumentation (e.g., one should relate one's argument to the main question; the critic carries the burden of proof), others as *general* norms of felicitous communication (e.g., one should write clearly and comprehensibly).

The relation of these principles to the study of fallacies is immediate. Some of these principles are claimed to belong to the "hardcore of principles taught within the tradition of academic

disputation (…) [which] were transferred also into controversies outside university life" (p. 112). One is led to infer that Fritz's preferred typology distinguishes principles that "form the backbone of the common-sense theory of controversy" (p. 112) from "principles directed against the committing of fallacies" (p. 113), from "efficiency principles" and those concerning "the relationship between two antagonist" (ibid.), e.g., politeness principles.

We learn that, empirically speaking, these principles "are just as often violated as they are followed" (p. 114), that "we often find a conflict of principles" (p. 115), that "certain principles hold for some types of communication or text types and not for others" (ibid.), that "the application of principles is to some extend negotiable" (ibid.), that understanding the status of communication principles requires knowledge of their "context of justification" (ibid.) and the "consequences of their application" (p. 116), that principles can form systems of values (p. 116) and, finally, that their "ranges and mode of application are historically variable" (ibid.).

As a 16th century (partial) survivor into our day and age, Fritz next discusses advantages and disadvantages of the *point-by-point principle*, according to which a proponent must answer (i) all points of criticism (ii) in the order in which they were raised by an opponent, including (iii) irrelevant ones and those which amount to no more than gibberish and to do it in such a manner that (iv) the new information has to somehow fit into the framework of topics set by the opponent and (v) such that the opponents original points can be understood by an audience that did not have the original text. In sum, Fritz holds the principle's disadvantages to outweigh its advantages, therefore judging it to be self-defeating, yet he observes that we find its traces still today: "[A]uthors can be found complaining that their opponent in a controversy did not take up all the important arguments in their favour" (p. 118).

With respect to *politeness principles*, Fritz reports that insult and aggressive behavior, though regularly criticized at that time, seem to have been the norm among 16th and early 17th century academics. Politeness becomes an issue in all European societies only in the middle of the 17th century and originates, on the one hand, in the Christian religion (or newer religious forms, e.g., the Pietist movement), on the other, in the "trend towards the cultivation of politeness that was founded on courtly traditions" (p. 119). Moreover, a new social expectation was that "educated persons should be fit to act in public office and at court. And in these surroundings cavilling and pedantic scholars were not acceptable" (p. 120), thus effectively suspending verbal aggression in favor of polite conduct. In sum:

> In the course of the 18th century, awareness of the inherent problems of the traditional point-by-point principle and the new discussion of politeness principles seem to have conspired to weaken the position of the disputation pattern as a scholarly form of communication and the [longish] pamphlet as its prototypical textual form. So we have here an example of a remarkable change in forms of communication that is closely linked to changes in communications principles". (p. 120)

In the concluding section, Fritz lists useful methodological detail for the empirical study of controversy in a historical perspective which, we may infer, is a field in its beginning.

The text is brief, readable, and free of jargon, suggesting that the author has successfully aligned his writing with current academic communication principles. If there is anything a reader might find wanting, it is an overarching normative dimension. From a contemporary point of view, it is less than clear if *normative historical pragmatics* is possible without turning into a normative history.

2.6 In *On the role of pragmatics, rhetoric and dialectic in scientific controversies*, Adam Ferreira presents an example of a month-long controversy among three natural scientist on whether a particular dynamic system fell under a standard definition of decoupling (A system is decoupled if and only if its initial state does not determine its final state). The example is said to be typical of the *real* activity of scientist whose actions, Ferreira claims, are under-described by traditional positivist or post-positivist models.

Against Popper, who held the discovery phase of a scientific hypothesis or theory to be of negligible theoretical significance and the context of justification to be of sole importance, Ferreira aligns himself with Nickels' (1985) model of *generative justification* which presupposes a feedback mechanism between both contexts. He seeks to extend this model by adding a slot in which to respect controversy amongst scientist who, following Laudan (1977), are conceived of not as *truth*, but as *solution seekers* (a.k.a. problem solvers) whose time constrained work is guided by search heuristics. Running predominantly over established background theories or approaches, heuristics and cognitive aims vary between individuals, thus giving rise to the substance of scientific controversy.

Finally, as "scientificity cannot be disclosed by the language used in science in the form of scientific statements [i.e., empirical theories or their empirical claims]" (p. 127), natural language is of "paramount importance" in controversy and "the toolbox to use for its analysis should contain (…) pragmatics, rhetoric and dialectic" (p. 128).

The three scientists, whose controversy Ferreira studied by means of interviews, seem to have initially run into severe communications problems. "Each participant complained that he could not understand the point of the others, and (…) [was] misunderstood by them" (p. 129f.). Over the course of four weeks, polemics waned and defending the three incompatible positions gave way to "soft rationality" (p. 130). A weaker definition of decoupling was slowly entertained which participants could agree upon. Apparently, this occured after one participant adopted the point of view of another.

Not much else about pragmatics, rhetoric and dialectic in scientific controversies, is communicated in this eight page paper. Generally, some of Ferrera's claims are difficult to make sense of, because they are not always written as clearly as they might be. At least one is false. Consider: "Up to the first half of [the] last century, the scientific endeavour and the resulting theories have been considered as epistemically certain and undisputable" (p. 125). However, the most widely accepted empirical theory in the natural sciences, Newtonian mechanics, had already been criticized well before the early 19th century and, therefore, should count as far from epistemically certain or undisputable at that time (see Zenker 2009: chapter three).

2.7 In *A "dialectic ladder" of refutation and dissuasion*, Christina Marras and Enrico Euli contrast (what they call) the traditional model with their proposal of a non-violent dissuasion model in the context of conflict resolution. Here, "[d]issuasion simply has to do with affecting a hearer's future actions and beliefs whereas refutation regards affecting in particular the contents [of belief]" (p. 137). The traditional and presently dominant model is claimed to advertise tolerance "as a method to compensate for otherness" (p. 138), however, it "generally retains differences" (ibid.) and "does not entail genuine equality" (p. 139). Rather, settlements of conflicts occur "according to the ethical frame of the 'major' side, which is deemed unquestionable" (ibid.).

Violence, subsists on the traditional model, we learn, insofar as the manipulation of behaviour through "treaties, rules, arbitrations, negotiations, and diplomacy" (p. 137) either hides the conflict or, in case manipulation is deemed impractical, exposes it. "Differences are thus settled not by reason, but by power, since, in most cases, the apparent equality between sides is merely assumed and not genuine" (ibid.). The authors "believe that most differences of opinion are ultimately reconcilable" (p. 140), yet hold that "dissuasion methods have not gained significant progress towards real pacification" (ibid.).

Based on brief mentioning of the most diverse examples (from marriage to terrorism), they call for a change in the "paradigm of

conflict resolution" (ibid.), since they "believe it [is] possible to effectively apply a modified version of the Dissuasion Model, in which relapse towards violence is forestalled" (ibid.). The proposed model is fitted to a conflict-type of "*gradual* exacerbation" (p. 135, *italics added*). The image of a ladder is presented: Six rungs represent *increasingly* difficult situations of conflict between parties.

Analytically, the model can be reconstructed by varying (i) the parties' mode of behavior on a scale ranging from *similar* over *different* to *completely opposite* with the function or aims of their behavior or actions on a scale ranging over the values *analogous*, *partially analogous*, *similar*, *incompatible* and *opposite*. The resulting products are called: *similarity*, *convergence*, *analogy*, *compatibility*, *keeping distance* and *non-violent struggle*, thus reaching an order of conflict types at ordinal level.

Meaningful distance relations are *not* immediately forthcoming. Therefore the ladder image (p. 144) is potentially misleading. After all, non-metaphorical ladder rungs are normally an *equal distance* apart. Now, *this* equi-distance information is implicitly claimed to transfers to *these* conflict types. Yet, if such transfer can be supported at all, it cannot (without circularity) be supported by recourse to the image of a ladder.

We are led to believe that each conflict type offers distinct possibilities for agreement (even the non-violent struggle does so), but also demands distinct aspects of a conflict to be put in focus. Likely owed to the brevity of the exposition, I could not make out what these aspects amount to in the particular case. More critically, it appears that the cases cited as negative evidence for the traditional dissuasion model are, so to speak, all located at the upper or the uppermost end of "the ladder". The new model generally advertises non-violence.

It sounds reassuring that "[t]his model can be instilled through lucid, metaphorical and creative forms of education, mediation training, peace education, conflict management and even in everyday behavior and practice" (p. 145). However, this is likely an understatement of the problem situation. Generally, behavior change is the most difficult thing on earth. Worse, those already able to change will not need the model, while those unable do not seem to be more likely to change *by virtue of this model*.

2.8 In *Responding to Objections*, Ralph H. Johnson discusses argument quality from a dialectical perspective, proposing "that one key indicator that an argument is a good one is that it can withstand strong objections" (p. 149). Here, a speaker's argument is seen as an invitation (to the hearer) to respond with appropriate reasons (objections/criticism) that challenge her to provide further support for her claim. This scenario then provides for a (dialectical)

"test of that argument" (ibid.), because under normal conditions the hearer's objection will serve to specify the speaker's dialectical obligations to address the other's (critical) responses. Hence, Johnson deals with the questions "What makes for a strong objection[,] (…) what are the possible responses (…) and what factors determine the strength of a response to an objection" (p. 150).

In answering the first question, five responses are distinguished, two of which are limiting cases (deny the force of the objection; ask for time out). In the three remaining cases, the objection has force, but is (i) *minor* and *no change* to the argument is *necessary* or (ii) *major* and the argument *can be revised or* (iii) it *cannot be revised*. A revised version of an argument, forwarded in response to an objection, then yields the notion of a *dialectical successor*. *Slight* modification of the original argument is said to preserve its *integrity*, only affecting its *identity*. Identity criteria for an argument are its propositional content and its inferential links: a change in either breaks identity (p. 156).

The identity-integrity distinction allows Johnson to make the strength of an objection more precise, thus addressing the second question. The answer is straightforward: "As long as the arguer is able to preserve the integrity of the argument while responding satisfactorily to an objection, then the objection was not a strong one" (p. 158).

> [I]f responding satisfactorily to the objection would force the arguer to change, not just the identity (the wording, the order), but the *integrity* of the argument, then that indicates that the objection is a strong one. It is the nature of a strong objection to force a reworking (or perhaps even an abandonment) of the argument, whereas an objection that can be accommodated by a minor change in the argument is a weak one. (p. 158)

On Johnson's view, then, "the integrity of an argument is a property that *emerges* as a result of that argument's being subjected to testing or criticism" (p. 159, *italics added*). In particular cases, the integrity can be traced diachronically in the sense of an argument's "dialectical history" (p. 160), such that the "ability to withstand criticism is a crucial test of an argument's value" (ibid.), while the range of criticism it elicits (the argument's *fertility*) is another (see p. 161). Fertile arguments are said to be situated in a densely populated *dialectical environment*.

Through the notions of integrity and fertility, one has taken a first step towards making more precise how argumentative strength may be understood dialectically. Both notions provide partial explications of *dialectical strength*. Along these dimensions, our assessment of a given argument-objection-response triple—or so

we must assume for the time being—will have to be *non-formal*, insofar as no clear measure seems available by which to determine the distance between an argument and its dialectical successor. After all, constancy of propositional contents and inferential links fall outside the integrity measure. That is to say, the dialectical successor which came about in response to a strong objection is, so far, not clearly distinguishable from the mildly deviating one (in Johnson's notation Arg1* *vs*. Arg2). Of course, practically, agreement may be reached without a formal measure.

Overall, Johnson has provided basic theoretical reflection on dialectical strength which, however, remain in sum rather weak. After all, one would like to (somehow) make more precise the comparative distance between an argument and its successor(s). One might then be able to understand the distance between the predecessor and the successor as a *function* of a particular objection to a particular argument. Such a measure in hand, the strength of an objection would become an objective property (relative to the measure employed). As Johnson acknowledges, "[a]ll of these notions need further work" (p. 161). At the same time, it seems safe to claim that future work on argumentative strength will not get around recovering these insights.

2.9 In *Pragmatic inconsistency and credibility*, and premised on the Pragma-dialectical model of a Critical Discussion, Jan Albert van Laar discusses the type of personal attack or *ad hominem* argument which occurs, "as a kind of confrontational maneuver" (p. 169), when someone who "has displayed specific behaviour has become inconsistent with a particular standpoint" (p. 163). As a crucial example (see p. 171), consider a smoker advising others against smoking or a child molesting priest preaching the gospel. If perhaps only insufficiently understood, such behavior is quite generally regarded to embody an inconsistency between *what one says* (or "preaches", p. 136) and *how one behaves*.

More specifically, one's behavior is said to be reconstructable such that words and deeds, when taken together, incur *conventional commitments*. These commitments are expressible by natural language sentences, such as to allow a judgment whether any such pair is mutually consistent. Thus, *pragmatic* inconsistency is analyzed by relying on the absence of *logical* consistency (see condition four on p. 170) between sentences, each of which expresses at least one (reconstructively incurred) commitment.

Should such an inconsistency charge be raised by a protagonist (the critic) who raises doubt towards a standpoint, then—so van Laar usefully specifies (see p. 166f.)—her dialectical aim may be understood to lie in establishing the claim that the "antagonist's concessions [whatever they are] commit her to (…) [an] absurdity" (p. 167). Such absurdity, we learn, threatens the arguer's interest

"to strive after a (pragmatically as well as logically) consistent position" (p. 171) in three distinct ways, namely: (i) with respect to maintaining *credibility*; (ii) with respect to maintaining *consistency* throughout several discussions, and (iii) with respect to maintaining an image of sincerity and capability as a discussion partner (p. 171f.).

Technically, the absence of credibility or sincerity is analyzed as a flouted higher order condition[1] (see pp. 172f.), such that, according to van Laar, the charge of pragmatic inconsistency comes down to claiming that a precondition for a rational discussion according to the Pragma-dialectical model is not fulfilled, thereby declaring the arguer "unfit" for the role of a protagonist in such a discussion. This claim is established through an instance of strategic maneuvering in what van Laar calls a meta-discussion: "Pointing out pragmatic inconsistency is a device for excluding persons from defending particular standpoints of from defending particular formulations of them" (p. 175).

Four soundness conditions for the felicitous charge of pragmatic inconsistency are discussed (p. 176f.). In summary, they can be rendered as follows, where *A* stands for an action, *S* for a standpoint, while *C*(*A*) and *C*(*S*) stand for the respective commitments: (i) *A* and *S* must not be falsely presented as such; (ii) *C*(*A*) and *C*(*S*) must be logically inconsistent speaker commitments; (iii) there must (normally) be additional evidence for ascribing "insincerity or incompetence" (p. 176) to a protagonist. Finally, (iv) "an attempt to discredit the protagonist should not be presented as implying that there is no convincing case for the arguer's standpoint or that the standpoint is false" (p. 177). To violate any of the four conditions amounts to committing a fallacy.

The above conditions purport that pragmatic inconsistency either isn't an interesting problem at all, because the arguer's standpoint is, after all, *independent* of his actions, or we are not dealing with a critical discussion to begin with (in which case pragmatic inconsistency would not seem to be an interesting problem either). Especially condition (iv) shows that, on this analysis, the presence of a pragmatic contradiction *should be* without effect to the proponent's standpoint.

I think that the kind of (allegedly pragmatic) inconsistency which is discussed in van Laar's paper is a seriously hot topic which, however, is significantly "cooled down" to fit the idea of strategic maneuvering. The problem, as I see it, is that the analysis remains unenlightening with respect to the question whether, for example, a smoker advising others against smoking for the sake of

[1] On the role of the higher order conditions in Pragma-dialectics as an open ended list which serves to register new criteria for fallacies hitherto unidentified in the theory, see Zenker (2007).

their health is, in any sense, less than an ideal *arguer* (as opposed to ideal character). Consider that, *ceteris paribus*, smoking *is known* to increase the risk of cancer. Perhaps understandably, such questions are neither addressed nor excluded in a motivated manner.

Lastly, the definition of a *pragmatically inconsistent position* (p. 170) appears (to me) too narrow to be general, as it precludes that such inconsistency can arise between *acts*. This seems too restrictive.[2] The definition thus only *purports* to treat a relation between an act and a sentence, because the basic definitional move is ultimately reductive, such that pragmatic inconsistency, if it pertains at all, holds as a logical relation over particular natural language *sentences* (not acts!), each presumably self-consistent, which express conventionally incurred speaker commitments, agreement on which is, in some sense, intersubjectively forthcoming.

2.10 In *Reasonableness in Confrontation, Empirical evidence concerning the assessment of* ad hominem *fallacies*, Frans. H. van Eemeren, Bart Garssen and Bert Meuffles report the results of empirical studies regarding speaker's judgments of the *ad hominem* argument/fallacy, which Pragma-dialectics analyzes as a violation of the *freedom rule* (Parties are not allowed to prevent each other from advancing standpoints or casting doubt on standpoints). Conducted "to determine empirically which norms ordinary arguers use *or claim to use* when evaluating argumentative discourse and to what extend these norms are in agreement with the theoretical-critical norms of [P]ragma-dialectics" (p. 181f., *italics added*), the studies investigate the *conventional validity* of the Pragma-dialectical theory, i.e., the extent to which *ordinary arguers*[3] are in intersubjective agreement, if not with the theory's explicit rules, then with their normative import (broadly construed). This type of validity stands in contrast to the rules' effectiveness in resolving a difference of opinion (*problem validity*) which, we learn, "is primarily a theoretical issue" (p. 182), i.e., depends on intersubjective *expert* agreement rather than ordinary arguer agreement.

The claim (to be tested) is that ordinary language users reject instances of the *ad hominem* fallacy (see the examples on p. 183f.)

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[2] See also condition 3: "[H]aving performed A[ction], P[rotagonist] *cannot* avoid committing himself to [assertion] T, *when asked to do so*" (p. 170, *italics added*). It remains unclear what the conditions are under which the protagonist *can* avoid said commitment. The examples on page 176 do not really help, because they leave it to the critic to be convinced, thus making pragmatic inconsistency hearer-dependent.

[3] Non-specifically-argumentation-theory-trained *students* aged 15-19; see note 1, p. 182 and table 1, p. 185.

for the reason that they constitute unreasonable discussion moves. The rival hypothesis (to be ruled out or, at least, weakened) is that the very same claim can be explained entirely by politeness considerations. Other alternative hypotheses, e.g., (logical) irrelevance, seem not to have been tested for.Thus, although rejecting an instance as a reasonable discussion move, language users might do so for a different reason altogether. After all, based on the overall figures (table 2, p. 186), the rival hypothesis is evidently compatible with the finding that the mean reasonableness-score (measured on a 7 point scale; 1=very unreasonable, 7=very reasonable)[4] of the well-known three variants of the *ad hominem* fallacy *decreases* in the following order: *tu quoque*, *circumstantial*, *abusive*. This order clearly mirrors the order reached when ranking test-items according to considerations of comparative politeness.

That politeness does not play a crucial role—this is the contention reached in this study—involves "tackling" the rival hypothesis from different angles by generating, through distinct methods, "independent" data sources to determine, if they "point in the same direction" (p. 187) or, more generally, *cohere*. The method or research strategy is known as *convergent operationalism*. Discussing it and reporting its results—yes, broadly construed, the direction is the same—takes up the larger part of the article (pp. 186-193). Readers interested in a critical discussion of empirical methods find delight here.

The tests, two of which were repeated ("duplicated"), had been conducted in the native languages of five countries: Netherlands (number of subjects = 92/24), UK (n = 60), Germany (n = 41), Spain (n = 47/30) and Indonesia (n = 50). They consisted of either 24 or 48 discourse fragments, distributed such that one fourth comprises non-fallacious items, and three fourth comprise clear cut instantiations of the three fallacy types. Linguistic or cultural differences appear to fall below the significance level.

As for results, "[t]he general pattern in the data is invariably the same: the fallacious *ad hominem* moves are judged in general as less reasonable moves than the non-fallacious moves in which no violation of the freedom rule takes place" (p. 185). Moreover, the five independent methods[5] suggest that politeness can explain

[4] Although unstated, we may assume that the scale was titled as follows: 2=unreasonable, 3=somewhat unreasonable, 4=neither reasonable nor unreasonable, 5=somewhat reasonable, 6 = reasonable

[5] Method 1: Adding *ad hominem* indicators (like "are you out of your mind"), which are normally perceived to indicate impoliteness, to the fallacious arguments. Method 2: Varying the discussion context (from *domestic* over *political* to *scientific*) under the assumption that, if politeness considerations are at work, impoliteness in the domestic domains will be perceived as less unreasonable than in the other domains. Method 3: Ask subjects to explain their

either no data at all or, plausibly, only a small part thereof, e.g., 11% (cf. discussion of the third method on p. 189), to reach the following:

> [T]hese results provide strong support for the contention that, generally, (1) ordinary arguers consider discussion moves persuasive only if they are reasonable, and (2) the reasonableness conceptions of ordinary arguers are *largely* in agreement with the theoretical critical norms of [P]ragma-dialectics. (p. 195, *italics added*)[6]

Here, the first claim (reasonableness strongly correlates with persuasiveness) is backed by a .72 median correlation (p. 194)—which is, at best, decent—, while the second claim (ordinary arguers agree with the results of the theoretical norms) is backed by the difference in reasonableness scores obtained upon comparing speakers' judgments for non fallacious instances with those for fallacious ones.

With respect to the second claim, this reviewer is stuck with the following: While the number four is the middle position on a seven point scale, instances of the *tu quoque*, *circumstantial* and *abusive* variant of the *ad hominem* received *averaged* reasonableness scores of 4.54 (.72), 4.21 (.78) and 3.81 (.87), respectively. Numbers in brackets give one standard deviation[7] (see table 7 on p. 194). The mathematical meaning is that approximately *two thirds* of ordinary arguers participating in the tests ranked the fallacious(!) *tu quoque* instances *between* 5.26 and 3.82, approximately two thirds of data points for the fallacious(!) *circumstantial* variant lie between 4.99 and 3.43, approximately two thirds for the abusive(!) *ad hominem* between 4.68 and 2.94.

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verdict by a given selection of explanations, amongst them politeness considerations. Method 4: Compare fallacious with non-fallacious arguments that both feature direct attacks under the assumption that the fallacious attack should be less reasonable than the non-fallacious attack if and only if politeness considerations do not play a role. Method 5: Ask participants to also judge the politeness of an argument and statistically estimate the influence of the politeness score.

[6] See also the rather cautious formulation: "[T]he [P]ragma-dialectical discussion rules seem to have (*some degree of*) conventional validity" (p. 182; *italics added*). For a more detailed treatment and a slighly bolder overall claim – "[T]he norms that ordinary arguers use when judging the reasonableness of discussion contributions corresponds *to a rather large degree* with the pragma-dialective norms for critical discussion" – see van Eemeren, Garssen and Meuffles (2009: 224, *italics added*).

[7] *One* standard deviation is the number which, by independently adding and subtracting it to/from the mean, yields the upper and lower boundaries of an interval within which one third of all data can be found to the right and one third to the left of the mean. Thereby, the standard deviation and the mean yield information about the distribution (the "spread") of data. See also the following footnote.

These data points seem to be spread over in the "wrong" region of the scale. After all, it these are fallacious instances, then we should not find two thirds of data in these intervals.

Consider in addition that, since measurement is performed on an ordinal scale, the fractions (.54, .21, .78, see above) do not have meaning, to begin with. After all, subjects were limited to select among *seven* options only. For short, one would like to see the raw data. There likely were (a good few?) points with a value of at least 6, meaning: 'rather reasonable'. If not in contradiction, then this stands at least in tension with the authors' claim that ordinary arguers are *largely* in agreement with the theoretical norms.

Two ensuing questions are: How to interpret "minority data" that does not really fit, and why don't ordinary arguers, if they *were* (largely) in agreement with the theory's normative import, use the scale's endpoints when presented with seven options.[8] That is to say, if the theory "predicts" a discourse item to be a fallacy, how come our *n*s keep responding at scale regions *dis*preferred in light of the theory? In all fairness, the problem is not distinct to this study, but it has no less bearing on the claims raised in the study.

More generally, exactly why is it important for a theory of argumentation to be confirmed by the judgments of ordinary (untrained) arguers? The article does not provide an answer. *If* it is important to have ordinary arguers assent to (the result of applying) given rules, can one then be satisfied with results, the interpretation of which oscillates between '*all* speakers are *somewhat* in agreement', '*most* are rather in agreement than not' and '*some* speakers are in *full* agreement'? This indicates (to me) that we have not yet fully understood how ordinary arguers judge argument quality.

Most generally, if one has managed to interpret data such that a particular hypothesis can "explain" a good part thereof, there still is an infinity of alternative hypotheses (not all of which are readily meaningful) to also explain the data. Therefore, the claim that (results of applying) the Pragma-dialectical rules are, in their normative import, *accepted* by ordinary speakers should, I believe, be treated with more caution.

2.11 In *Managing disagreement space in multiparty deliberation*, Mark Aakhus and Alena L. Vasilyeva analyze and evaluate a discourse exchange from a meeting between community leaders and housing developers in the north-eastern USA with respect to the managing of disagreement, broadly construed. We learn that:

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[8] When normally distributed, 95% of data points lie within *two* standard deviations (SD) to either side of the mean. Hence, e.g., for the fallacious *tu quoque* variant (mean: 4.54, SD: .72), 95% of data must have been between 5.98 and 3.10. Therefore, *if* endpoints were used, then only by a maximum of 5% of subjects.

> While the parties *appear* to be entertaining and vetting a proposal in their [unofficial] meeting, they ultimately avoid the commitments and obligations involved in initiating and accepting a proposal. How they accomplish this (…) is explained by developing the concept of disagreement space (…). (p. 197, *italics added*)

The discourse starts with a proposal (summarized on p. 189f.) by one of the developers (which took 18 minutes), followed by discussion (90 minutes long) in which doubt is raised. As parties had different interests, the notion of 'disagreement space management' is called upon to explain how "community leaders and developers expand and manage their disagreement and with what consequences" (p. 200). The authors' idea is to understand the opening speech as "an example of strategic maneuvering (…) to shape the topical potential of the meeting and the unfolding events of the community controversy" (p. 202).

The authors suggest three ways of managing disagreement expansion: (i) "[L]ines of reasoning similar to those used in producing the [initial] speech are used in producing doubts and disagreements about the speech" (p. 208); (ii) "the framing of the meeting does not draw attention to the opening speech as a formal proposal but to the fact that the parties are engaging with each other over things proposed" (p. 210); (iii) "community members [re-]frame the opening speech as an incomplete proposal (p. 210). See also the first paragraph on p. 212.

Briefly and in summary: "When the speech is framed as less than a full proposal, the proposal remains in a state of development and the parties are not obligated to working out the proposal together. This manages their obligations to each other (…)" (p. 211). Throughout the text, the authors present six discourse snippets ("sub-dialogues", p. 205), featuring discussion moves of up to seven turns, to exemplify their claims.

Other than being impressed by the choice of certain of the authors' terms, e.g., "trajectory of the discussion" (p. 212), and although not having seen the entire transcript, I suspect over-interpretation. The text excerpts do not quite exemplify the authors' claims, which therefore appear somewhat contrived. The problem is that the analysis interprets the text such that what is said does *not* exhaust what is being done. This is understandable, given the authors claims, but no less problematic.

Moreover, it seems warranted to assume that *both* discourse parties met knowing full well that this *unofficial* meeting was held for the only purpose of "feeling the temperature", not of making decision. Presumably, none of the participants said so clearly. Instead they overtly treated the opening speech as a proposal

which, however—the authors call this an anomaly (p. 212)—is not brought to its end, i.e., to a decision on the proposal. To identify ways of managing a disagreement space, of course, remains a legitimate endeavor, but comes across as overblown, if it is clear that, on this occasion, resolution of a disagreement was not an agenda item, to begin with. After all, the speech act type *proposal* is "brought to the text" by the analyst. And if she finds that her categories are less than perfectly instantiated, she needs to seek an explanation. At any rate, I find nothing anomalous about the interaction.

2.12 In *Predicaments over politicization in the debate over abstinence-only sex education*, Sally Jackson defends the claim that "resolution of a disagreement as originally framed is not the only outcome that can be usefully returned from argumentative discussion" (p. 218). The basic idea is to accept predicaments as "situation[s] in which *all* moves available to a participant seem to lead away from resolution of a disagreement" (p. 218, *italics added*) and, rather than resolve disagreement, seek to improve it. In pictures and with respect to strategy: "Falling prey to an argumentative predicament is like falling into a trap in chess" (p. 219).

The lesson is that arguers—here: scientists taking a stance on (funding-relevant) politics—would do well to recognize the risk that comes with disagreeing, i.e., should learn to *manage* the disagreement, especially when addressing politicians and the social sphere. After all, "[t]he risk to the arguer is in each case associated with opening a 'disagreement space' (...) that the opponent can exploit with devastating effect" (p. 219).

Generally, argumentation is seen as an *expansion of speech act sequences*. Thus, any specific form of disagreement creates opportunities for subsequent expansions, some of which may (forseeably) turn out to be unfavorable to the arguer and, from a strategic point of view, should therefore be avoided. Thus, as you disagree now, project the possible responses in order to reach the 'disagreement spaces' in which you want to be, or which you want to avoid.

Concretely, Jackson presents a case study consisting of (i) a 2004 report by the Union of Concerned Scientist (UCS) who, amongst others, claim that the Bush administration "push[ed] an approach on sex education [which] purposely ignores the relevant science", as "abstinence-only sex education is ineffective" (see Fig 1., p. 221); (ii) a response by John H. Marburger, chief science advisor to the Bush administration, who was smart enough to respond that the UCS (and presumably the larger scientific community) "does not consider abstinence to be a worthwhile goal

—(…) [which] is a moral position, not a scientific one" (p. 224); (iii) the UCS's response to Marburger.

Jackson's critical evaluation contends that the UCS response is wanting, insofar as it (i) fails to adequately address Marburger's claims, (ii) at best succeeds only in producing doubt, rather than demonstrating the UCS's claim, and (iii) is left with nothing but a resort to silence on one of Marburger's responses (see pp. 221-225). Overall, her verdict is that "[b]y failing to comprehend the critique of science inherent in the Bush administration positions, scientists expose the political (or at least moral) presuppositions of their own choices" (p. 225).

As for practical advice on managing the disagreement space in such a way that an unfavorable extension of the argumentation by the other party can (presumably) be avoided: "[R]efrain from opening any disagreement space at all around other people's motives for acting as they do" and "search for what makes sense about the opponent's position" (p. 225). In fact, the former is claimed to be helpful in achieving the goal of the latter. Ultimately, rather than scoring points, the "wise strategists will concentrate (…) on pushing towards questions that are truly worth answering" (p. 228).

In Jackson's opinion, a better outcome is the search for common understandings, of which a "greater responsiveness on the part of science for the social agenda" (p. 227) is claimed to be a part. An important theoretical point is that disagreement *need not* start with the speech act 'assertion', but, as illustrated by this case, e.g., with a 'request' for proposal by a funding body.

This reviewer finds Jackson's point to be solid, at least with respect to the case. Her hope for a greater responsiveness on the part of science for the social agenda, on the other hand, might be illusory, perhaps romantic. Having worked through some jargon (e.g., "argument as a particular kind of expansion of projected speech act sequences", p. 216), it remains unclear to me whether the *specific* theoretical background invoked here motivates the results (and recommendations) or whether it is rather the other way around.

2.13 In *Rhetoric of Science, pragma dialetics and science studies*, Gábor Kutrovátz criticizes the predominance of (what he calls) rhetorical approaches within science studies. In their stead, he seeks to advertise the usefulness of dialectics for this field. His aim is to show the Pragma-dialectical potential for science studies as a theory that replaces (rather than enriches) the currently used methodology. Though he readily admits the insufficiency of the "theoretical arguments" (p. 245) delivered in his paper—"The usefulness of dialectics to science studies is to be *demonstrated* by providing detailed and informative case studies of argumentative dialogues in science" (ibid., *italics added*)—, he sees "the basic

commitments of pragma-dialectics [to be] in fine agreement with several characteristic of recent trends in social studies of science" (ibid.).

One may perhaps best understand these trends as a reaction to the predominantly epistemological debate on the progress of science which—*pace* Kuhn and his interpreters—still seeks to analyze theory change as a matter not at all to be informed by social or cultural factors, but to be conducted "in terms of empirical content, experimental adequacy, logical coherence, etc" (p. 232). In contrast:

> [T]he essential social nature of science implies that scientific knowledge is produced and practised in a social space, that social processes are constitutive of the workings of science, and also, that scientific cognitions is, on the whole, not suitable to be described in traditional individualistic epistemological terms. (p. 232)

At the same time, Kutrovátz denies that much of use can be found either in Bloor's so called 'Strong Program' in the sociology of science (which seeks to make do with the term *cause* rather that the normatively loaded *reason*), nor in being carried along by the so called 'third wave of science studies' (which seeks to integrate analyst with actor normativity in the concept of 'expertise'). Basically—this is the implicit critique—, the strong program runs the risk of "revealing" *all* actor categories as historical delusions (which is too strong), while expertise still appears too weak (or too broad) to provide genuine insight into the *discursive* practices of science. However:

> A special form of expertise used both by scientists and analysts of science is argumentative expertise: the use of arguments is a commitment that is shared by scientist and those who study them, and thus may offer a promising way to bridge the gap between actors and analysts. (p. 233).

This, then, is the relevant proposal on how to make sure that the explanatory toolkit used by science studies *does* coincide with reflexive tools used by scientist when describing their own activity. See p. 232, where this relation *not* obtaining is levied as a criticism against the rhetorical approach to science.

Kutrovátz goes on to identify Pera's approach to argumentation in science as rhetorical and argues that, since Pera lacks a fully explicated theory of argument, his analyses amount to historical reconstructions which remain "at a rather intuitive level" (p. 236). In the author's opinion, Pera's position can be located between "two extremes, [namely] methodological absolutism and descriptive relativism" (ibid.). This position would be improved

upon by endorsing the pragma-dialectical theory. To show as much, Kutrovátz discusses the latter's basic tenets (*externalization*, *socialization*, *functionalization* and *dialectification*) with respect to the object of science studies, arguing that genuine advantages over the current methodology may be expected. Generally, the idea is that:

> [D]iscourse theorists have *more* competence in analysing and evaluating arguments than the scientists who formulate these arguments: while scientist's discursive competence stems from tacit practice and experience, scholars of argumentation derive their expertise from explicit, conscious, and systematic reflection. (…) We can identify the realm of shared assumptions (…), identify the space of disagreement (…), map the conceptual order (…), follow the reasoned moves that result in changes in the conceptual order [and] [f]inally, we can evaluate discursive situations, and provide feedback to scientists from which they might benefit eventually. (p. 240)

As for "terrains of application" (p. 241), we are led to believe that letters, but in particular journal papers provide a promising material basis and that "their analysis can be a very useful help in understanding the dynamics of knowledge production" (p. 244). In a nutshell, the message is: 'Dialectics to the rescue!'

I deeply appreciate the challenges incurred in trying to explain scientific progress (Zenker 2009: chapter 6) and I agree that discourse scholars might, in some sense, be expected "to be better at" analyzing arguments than are working scientist. However, for the proposed integration of argumentation theory into science studies to have the effects that the author expects, some basic constraints might be considered:

First, count the number of working scientists and compare to the number of working science studies scholars. The ratio will be embarrassing, hence one should reasonably expect to the scholars to be able to *analyze* only a small fragment of scientific discourse. *Second*, consider that teaching argumentation theory to scientist is likely the better method to benefit them *with respect to their argumentation*. *Third*, consider that to engage in the kind of work suggested here—lest it become a mere variation on the *history* of science (and then produce yet further analyses of "surpassed" theories)—, it takes a good bit of scientific training to understand the contemporary working scientists' communication, not to speak of evaluating it. Where does that knowledge come from?

Generally, the suggested line of research is quite a handful and might be presented, especially to the working scientist, in a more modest tone. Otherwise, if a prophecy is allowed, science studies run the same risk as current philosophy of science, namely of becoming uninteresting to working scientists, thus irrelevant to

science and the public, and eventually just another self-referential discourse with the term 'science' in its title.

2.14 In *Scientific controversies and the pragma-dialectical model, Analysing a case study from the 1670s, the published part of the Newton-Lucas correspondence*, Gábor A. Zemplén presents a dialectical analysis of a part of the Newton-Lucas letters. He seeks to show its superiority over a rhetorical analysis. The latter, he submits, is deficient for his purpose, insofar as "a rhetorical approach is unidirectional, and breaks up interaction into separated attempts at persuasion" (p. 253). In contrast, on the dialectical approach (in particular the Pragma-dialectical one), "texts [here: letters] in a scientific controversy (…) can be treated as small speeches that together constitute a dialogue" (p. 254).

The benefits, claimed to be of special (positive) relevance to a constructivist view of science (cf. p. 254), are as follows:

> Functional roles can be assigned to elements of the utterances, and changes with respect to the various issues can be mapped. As a result, it becomes possible to relate the different "speeches" to one another. Furthermore, one can account for some of the changes in the positions as responses to the argumentative moves of the opponents. The pragmatically informed dialectical reconstruction—as opposed to a rhetorical one—allows the analyst to see and point to the active participation of antagonists in the production of knowledge. (p. 254f.)

The demonstration of these benefits is "far beyond the possible scope of this paper" (p. 255). Zemplén aims at showing "some of the promises of such an approach" (ibid.).

The controversy centers on Isaac Newton's prism-lens experiments and his corresponding theory of light (Slogan: "Light consists of Rays differentially refrangible", p. 250), which stood in opposition to the then accepted view, the so-called modificationist account of light, according to which "homogeneous white light is somehow *modified* to give rise to colours" (p. 251, *italics added*). On Newton's view, rather, homogeneous white light is *constituted* by heterogeneous rays.

The ensuing controversy, roughly lasting from 1672 to 1715, is treated as "one of the first major debates in a scientific periodical [Philosophical Transactions]" (ibid.). How Newton's theory came to be accepted *vis à vis*, in particular, Lucas's criticism—or so we learn—has been extensively discussed by authors in the history and philosophy of science (see p. 257 f.), though without unanimous results. "Even eminent contemporary historians seem to disagree on what the significance of Lucas's critique is" (p. 258).

At face value, the author makes use of the Pragma-dialectician's 'mixed difference of opinion' to characterize the relation between the reconstructed standpoints of Newton and Lucas. Specifically, he identifies three issues: (i) elongation of prism-image (when replicating the experiment, Lucas claims not have achieved an *elongated* image); (ii) the shape of the image (Newton's reported ratio is 1 : 5, Lucas' is 1 : 3.5); (iii) Lucas demands additional experimental support for Newton's theory.

Pragma-dialectics in hand, Newton's published response is construed in relation to these three issues. Here, the first seems to have dissolved (by the time of Newton's response), as his opponent had also achieved elongated images (cf. p. 263). The second can seemingly be defused by the claim that Lucas used prisms whose refraction capacity was different from those of Newton (ibid.).

It is with respect to Newton's answer to the third issue, then, that the analytical or reconstructive part of the Pragma-dialectical theory is used extensively. Zemplén presents a detailed reconstruction (altogether one page) of Newton's argument for the standpoint "It is not necessary to reply specifically to Luca's experimental objections" (p. 266), i.e., the third issue. Zemplén declares the argument "a beautiful example [of] how methodology (like the notion of crucial experiments) acquires specific functions in the course of a controversy" (p. 267).

In yet later responses, we learn, Newton can be observed to "maneuver" with the significance of a so-called crucial experiment (*experimentum crucis*), i.e., an experiment which can *decisively* test a theory. Thus, "Newton's shifting the burden on the crucial experiment can be tracked and analyzed easily in a dialectical model, and the functions of these shifts can also be connected to the specific situations" (p. 268). This fine grained analysis—such is the claim—, provides insights which surpass and thus improve the current understanding of this controversy.

It is worth noting that Newton appears to have ended the exchange with Lucas. In fact, he held some responses back from publication. The last letters were published only in the second half of the 20th century. Equally noteworthy are Zemplén's observations with respect to the extended Pragma-dialectical model:

> As it presently stands, the pragma-dialectical approach is far from incorporating all or most rhetorical insights. Early steps have been made with the concept 'strategic manoeuvring' (…). Even in this extended pragma-dialectical approach, however, the rhetorical aspects only find their way in[to] the reconstruction as subordinated to the resolution-oriented dialectical goals. (p. 265)

As Zemplén notes, his paper is (perhaps primarily) a *testimony* to the fruitfulness of the Pragma-dialectical model in studying scientific controversies (p. 270). And, indeed, his felt "obligation to provide some background and details relevant for experts in both areas [argumentation theory and history of science] (…) runs the danger of eclecticism" (p. 250). The reader may find the text to provide either too little or too much detail.

The general impression is that the Pragma-dialectical model can be advertised as fruitful, *mainly* because he uses the 'rhetoric' in a narrow and, perhaps, somewhat outdated meaning. It may very well be that "[a]part from a rather loosely understood interest in 'rhetoric', analysts have few methods of analysis and reconstruction to study the fine details of arguments that influence the way scientist (…) come to hold the views they do" (p. 250). Yet, it thereby does not become any clearer why the author's insights could not also be gained by using a rhetorical approach. Hence, Zemplèn's main stated reason (one is not able to construe an exchange-like structure, p. 253) appears too much like the central part of a straw-man strategy. After all, it remains at best unclear why Zemplén believes that such a structure could not come about in a rhetorical approach which he seems to view as inherently monological.

3. Brief Evaluation

The collection of essays stands witness to the ongoing and generally very successful efforts undertaken by the Pragma-dialectical school to engage neighboring fields. The effort is to be applauded. With respect to quality, the contributions exhibit variance; sometimes, fancy terms have to stand in for a clear exposition of a central concept. *Pace* the integration of rhetorical insights in the extended Pragma-dialectical theory, throughout the book, a non-traditional use of the term 'rhetoric' appears absent – a fact which is unlikely to improve the prospects for interaction between the fields. Rhetoric, more or less, comes across as the art of good speech and the skill of using language strategically, possibly for the sake of deception.

There is no doubt that the collection provides valuable insights into argumentative discourse, its analysis and evaluation. In particular, some of the case studies should serve well as models for future research. Such are needed in order to move the field further away from the contrived and oftentimes ridiculous toy examples currently dominating it—the reader may select a random

introductory text to find examples.[9] In this respect, the book indeed succeeds in filling a gap.

Overall, and despite the rather excellent editor's introduction, when taken together, the papers do provide *no more* than a closer insight into the relationship between controversy and confrontation. As has been argued at length here, what this means *precisely* is currently at best unclear. As the editors point out at the beginning: "[A] cautions start has been made by utilizing insights in the prevailing methods for dealing with controversies and other specific types of argumentative activity" (p. 24).

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[9] For this reviewer's attempt at studying a case rather than a toy, see Zenker (forthcoming).

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