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Science, Technology, and the Political: The (Im)possibility of Democratic Rationalization

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Abstract: In this paper, I elaborate on the very political dimension of epistemology that is opened up by the radical change of focus initiated by constructivism: from science as knowledge to science as practice. In a first step, this brings me to claim that science is political in its own right, thereby drawing on Mouffe and Laclau's framework of radical democracy and its central notion of antagonism to make explicit what is meant by 'the political.' Secondly, I begin to explore what this intrinsic political dimension of science might entail for democratic thought. I do so by connecting my preliminary explorations in the field of science with Andrew Feenberg's elaborate frame of thought on the democratization of technology. Interestingly, Feenberg is one of the few thinkers who have connected questions of power and ideology, typically treated of within the field of political theory, with a constructivist approach to technological progress. In this sense, this paper can be seen as a first attempt to expand Feenberg's framework of democratic rationalization from the world of technology to the world of science.

Key words: democratization of science and technology, politics of science, antagonism, composition, inclusion-exclusion dialectics

1. Science is Political!—Beyond the Slogan

'Science is political' could stand as a slogan for a critical aspiration shared within post-positivist philosophy of science, which gathered momentum with the publication of *The Structure of Scientific Revolutions* (Kuhn 1962). Kuhn pointed out that social factors such as scientists' concerns and interests play a prominent role in the emergence of scientific theories. Post-Kuhnian thinking, in particular social constructivism, has further concentrated on the social embeddedness of scientific practice.[1] Opening up the black box of scientific practice, constructivism has brought about what I call a first-order claim that science is political (Goeminne

2011a). Although this first-order claim comes in different flavors, its central gist is that actual scientific practice, because of its situated and human character, results in imperfect, value-laden and perspective-bound knowledge. In response to this first-order claim, which typically remains quite vague about what is precisely meant by the political character of science, it has repeatedly been argued that the expert world of science should be brought into democracy, where democracy is usually understood in terms of increased public engagement through reflexive communication and participatory approaches (see, e.g., Funtowicz and Ravetz 1993). A pithy summary of the argument for this widespread appeal to 'bring the sciences into democracy' is thus that science is political, that the political should be democratic and that the democratic should be participatory.

In my view, such constructivist-inspired calls for a democratic reform of science fall short because they are predicated upon an inadequate understanding of the politics of science, thereby failing to recognize how the politics of science challenges prevailing views of democracy. Indeed, as Winner (1993) has argued, constructivist thought all too often uses the concept of the political as a black box containing a variety of critical claims vis-à-vis science's self-proclaimed values of universalism and rationalism, thereby excluding the political from further analysis. Besides an apparent neglect of engagement with political thought, this points to a major shortcoming of most, and especially social, constructivist discourse: in its one-sided social-contextual approach of scientific practice, it does not allow for a symmetrical, critical analysis of the social-constitutive elements of science. This observation is moreover backed by an apparent lack of attention for science and technology on the side of political thought, analyses of which are, as Brown (2000) illustrates, generally restricted to questions of ideology and the general character of modernity. More specifically, Brown argues that contemporary political thinkers only rarely explore the social dimension of laboratory research and technological innovation and its ramifications for the main categories of political theory (Brown 2000: 198). In addressing the issue of political subjectivity, for instance, political philosophy usually restricts its attention to relations among subjects or subject positions; the relationship between subjectivity and the objectivity of science rarely enters the picture.

All this goes to show the need for a simultaneous rethinking of science and politics beyond dualistic ideas about either the politicization of science or the scientization of politics as threatening democracy. Observing that both the epistemological contribution and the political contribution of the first-order 'science is political' mantra remain limited, in this paper I aim to think beyond the slogan and I will do so on two levels. First of all, I will elaborate on the very political dimension of epistemology that is opened by the radical change of focus initiated by constructivism: from science as knowledge to science as practice. This will bring me to put forward as a second-order claim that science is political in its own right, thereby drawing on Mouffe and Laclau's antagonistic framework to make explicit what is meant by 'the political' (Laclau and Mouffe 2001). Secondly, I want to begin to explore what this intrinsic political dimension of science might entail for democratic thought. I will do so by connecting my preliminary explorations in the field of science with Andrew Feenberg's elaborate frame of thought on the democratization of technology. Interestingly, Feenberg is one of the few thinkers who have connected questions of power and ideology, typically treated of within the field of political theory, with a constructivist approach to technological progress. In this sense, this paper can be seen as a first attempt to expand Feenberg's framework of democratic rationalization from the world of technology to the world of science.

2. Science and the Political: An Antagonistic Approach

In this section, I want to put forward a second-order claim vis-à-vis the politics of science that, quite unlike the first-order claim's negative perspective, positively understands the situated, perspectival character of science. Beyond constructivism's recognition of the non-neutral, value-laden character of science, I will therefore focus on the role of concrete concerns in the emergence of scientific objectivity as being truly constitutive of the knowledge composed and as inevitably resulting in a dialectics between what has been taken into account and what has not. Conceiving of climate modeling as a concernful work of composition, as I will do in a second paragraph, will indeed illustrate how particular concerns that are present at the upstream end, such as globalism, simulation and prediction, play a crucial role in shaping the final form and content of the knowledge emerging at the downstream end. In a first step, however, I want to outline my interpretation of the value of constructivist thought and I will do this by briefly recapturing the paradigm shift between Aristotelian and Galilean gravitational theory. Rather than pursuing historical correctness, I will try to convey in rather general terms how I understand the view of science that has emerged from fifty years or so of constructivist science studies.[2]

2.1. Constructivism Revisited: A Story about Aristotle and Galileo's Concerns

In Aristotle's time, natural philosophy's interest lay with 'natural' phenomena, that is to say with phenomena that 'naturally' happen without human intervention. Therefore, when Aristotle looked at falling objects, his aim was to explain why heavy bodies fall faster than light ones and he did this in terms of the nature of the substance of the falling object. The Aristotelian theory of gravity states that all bodies move towards their natural place. Heavy substances such as iron and stone are considered to be primarily consisting of the 'element of earth' so their natural place, Aristotle claimed, is the centre of the earth, wherefore they 'naturally' fall towards it. This also explains why heavier bodies fall faster than lighter bodies: heavier bodies proportionally contain more of the 'element of earth' and therefore have a greater tendency to move towards their natural place. Whereas Aristotle tried to explain why bodies fall the way they do on earth in terms of the nature of their substance, Galileo, the founding father of modern experimental science, tried to describe the way bodies fall in terms of mathematical equations. Based on his famous inclined-plane experiments, some 400 years ago, Galileo formulated a mathematical universal law for falling bodies, i.e., $s = \frac{1}{2} gt^2$, which says that the distance covered (s) by a free-falling object is proportional to the elapsed time (t) squared, the proportionality given by one half of the gravitational constant (g). Based on this law and contrary to Aristotle, Galileo claimed that all bodies, whatever their weight or substance, fall equally fast.[3]

In this dehistoricized juxtaposition, the obvious question seems to be: who was right, Aristotle or Galileo? If one simultaneously drops a piece of paper and a book, the conclusion will be that Aristotle was right. However, one might object that Galileo postulated the validity of his law only for free-falling bodies, i.e., bodies falling in a void. Stripping them of particular earthly conditions, Galileo produced falling objects in an artificially controlled setup, which eventually yielded him a precise and universally valid mathematical description of their trajectory. At this point, we could imagine Aristotle protesting that such a law is of no use to him, as no single object will 'naturally' make such a free fall. So, once again, who was right: Aristotle or Galileo?

Apparently, both were right, depending on the question science is supposed to answer: the question about falling bodies on earth or the question about falling bodies in an artificial, experimental environment. The crucial question that imposes itself here is thus not a question of logical truth, i.e., 'Who is right?' but rather a question of what Boehm (2002) calls "topical truth," i.e., 'What is the issue at stake?' or 'What is more interesting?' So, topical truth points to the idea that behind the logical truth of any scientific answer, there lies a different kind of truth, the truth of the scientific question: Is the topic that is addressed interesting or not? And if so, in what respect? (Goeminne 2011a).

In a—admittedly unnuanced—way, one could say that until the second half of the twentieth century, the philosophy of science and epistemology were dominated by a debate on the logical truth of scientific answers, investigating the status and value of scientific expressions and judgments. From the 1960s on, however, fuelled by the publication of Kuhn's *The Structure of Scientific Revolutions* (Kuhn 1962), focus has shifted away from scientific answers to scientific questions, that is to say the concrete ways in which science frames its issues. In the aftermath of Kuhn, the emergent field of constructivist thought, now generally known under the banner of Science and Technology Studies (STS), has further concentrated on the social embeddedness of science and this from perspectives as various as anthropology (e.g., Latour and Woolgar 1979), sociology (Pickering 1984), history and philosophy of science (Hacking 1983) and feminist studies (Harding 1986). Through their variety, these different approaches share a common focus, in that they all start from a close investigation of actual scientific practice in interrogating and rethinking objectivity, subjectivity and their interrelatedness in science and technology. This in turn led to a much more complex and 'human' view of the scientific world than was generally held until then.

This brought Latour to introduce the wonderful idea that scientific "matters of fact" are always also "matters of concern," thereby explicitly thematizing the inherently human, that is to say, 'concerned' character of scientific practice (Latour 2004). In other words, scientific facts are always already answers to a particular question, the latter expressing a particular way of being concerned with the world. Galileo, for instance, was concerned with revealing a universal mathematical expression for the speed of falling objects. Aristotle's concern, on the other hand, was with the causal relationship between the substance and natural places of falling objects on earth.[4]

What we can also see emerging here is a relevant division between the so-called internalities and externalities of a particular paradigm, that is to say between what it takes into account and what it does not. Galileo takes into account measurable amounts of time and distance, which were irrelevant in Aristotle's perspective. Just the mere idea alone of quantifying and measuring was strange to Aristotle's worldview, centered as it was on 'natural' phenomena. Vice versa, Aristotle's relevant concepts were substance and natural place, which were irrelevant to Galileo's concern. To put it briefly: Aristotle and Galileo, being differently involved with the world, had differing concerns. In a sense, one could conceive of this imaginary debate between Galileo and Aristotle as a struggle for what to be concerned about, a struggle, if you like, for topical truth.[5]

It is along these lines that I propose to view constructivism as a field of investigation into topical truth by laying bare the particular concerns that drive scientific practice, which is then conceived as a 'concernful work of composition.' I purposively use the notion of composition here as it hints at something going beyond both representation and construction.[6] Composing, analogous to the act of creating a piece of art, is understood to be the framing of different pieces together into a meaningful whole. Science does not represent any reality that is out there nor does it construct facts out of the blue. Rather, as illustrated by the above discussion on Aristotle and Galileo, it reflects a very specific, concernful involvement with the world that allows it to come up with meaningful compositions.[7] In this respect, it is important to see that the notion of a free-falling object, which constitutes the basic object of knowledge in Galileo's theory, is intertwined with his experimental procedure. Before Galileo, no such thing as a free-falling object existed. So, his universal law, rather than being the unique answer to an unequivocal question, should be understood as the contingent outcome of an interplay between problem framing and solution framing, this interplay being driven by Galileo's fascination with universal, mathematical expressions and mediated by his use of experimental technologies such as the balance and the inclined plane (Van Dyck 2006).

2.2. Climate Modeling: A Concernful Work of Composition

Having outlined its more general features, I will now further elaborate my 'compositionist' view on science and describe the 'scientific composition of global climate change' by focusing on the practice of climate modeling. Climate models are computer-based numerical representations of the climate and are heavily relied on in global climate policy. As such, climate modeling is an extremely interesting object in gaining an understanding of how particular concerns become built-in at the upstream end of science and whether, and, if so, how this is reflected at the downstream end in the actual practice of climate policy.

A closer look at the history of climate modeling[8] makes clear that the notion of a global climate and the idea that it could be understood through the use of modeling techniques are not separate ideas, but rather that they took shape simultaneously. Until the 1950s, climatology was pretty much a spatial issue focused on gathering local data such as temperature, precipitation, etc. and averaging them out in presumably stable geographical climate patterns. From the 1960s on, however, this regional, data-driven approach gradually evolved into a theory-driven, model-based and globally oriented discipline increasingly focused on forecasting the future. As I will come to explain, this evolution originated in the US and was nurtured by the availability of increasing computing power combined with a military interest in weather modification as well as emerging environmental concerns with climate change.

Initially, however, modeling techniques were used to introduce a certain degree of homogeneity in the vast array of disparate, local meteorological data. Averaging local data over time as well as space is a prerequisite for obtaining relevant information about global weather patterns. However, measurement standards as well as instruments and the way they are used may differ immensely over time and space. How does one compare data that result from different recording methods? And how does one deal with a mere lack of data, either in the spatial or in the temporal dimension? According to Edwards (2010), this requires two complementary tasks: collecting planetary data in standard forms to build images of global weather and oceanic and atmospheric circulation ("making global data"; Edwards 2010: 187–228) and building complete, coherent and consistent global data sets from incomplete, inconsistent and heterogeneous data sources ("making data global"; Edwards 2010: 251–86). Since the 1950s, modeling techniques have allowed climatologists to generate relatively homogeneous data sets, computer models serving as what Edwards (2010) calls "technical gateways" between heterogeneous sources.

In a second step, the use of computer models to understand—rather than create—global weather patterns was boosted by an increasing social interest in simulation and prediction, both from an environmental and from a military perspective. Until the 1960s, basic modeling research was mainly supported by military sources interested in controlling and possibly modifying weather patterns. From the 1970s on, however, atmospheric scientists deliberately tapped into growing public concerns about human impacts on the environment to secure funding for their research. As Demeritt noted, both perspectives are consistent with what Edwards (1996) called the "closed world discourse" of Cold War America: "a language of integrated systems, an image of global containment (of communism and environmental problems) and apocalypse, and a practice of technologically centralized management, communications, and command-and-control" (Demeritt 2001: 315). The basic idea, of course, is that simulation models enable so-called "virtual experiments" which allow manipulating variables at will and observing their effects and outcomes (Edwards 2010: 115). Indeed, in the case of climate change, the only way to demonstrate the anthropogenic character of climate change is to simulate what would have happened without humans adding greenhouse gasses (GHGs) to the atmosphere.

From this rough historical sketch, one may already conclude that current climate modeling, and the image of a global climate it produces, is not the unique answer to an unequivocal question. The prevailing scientific construction of climate change should rather be viewed as a contingent social outcome of an interplay between problem framing and solution framing, this interplay being driven—but not determined—by particular, contextually bound concerns such as globalism, simulation and prediction.[9]

Actual climate models, the so-called General Circulation Models (GCM), simulate the behavior of the climate system by dividing the atmosphere into three-dimensional grid boxes (approximately 100 square kilometers in surface area and a few kilometers in height) and using supercomputers to solve mathematical equations representing the climate's so-called core physics. The latter mainly deals with energy transfer between different grid boxes and is based on fluid dynamics of the oceans and the atmosphere. Models founded on such a three-dimensional grid intrinsically neglect all possible processes on smaller scales. So, the grid scale of a climate model introduces a first aspect of a separation between internalities and externalities which, as will be argued, I see as an essential characteristic of scientific practice. Whereas 'grid-scale processes' such as the energy transfer between grid elements are internal to the climate model in the sense that they are modeled from the bottom up applying fluid dynamics, sub-grid-scale processes such as cloud physics or the transfer of water vapor between water surfaces and the atmosphere are thus external to the dynamical core of climate modeling.[10] However, sub-grid-scale processes are 'internalized' by means of exogenously specified parameters that capture the large-scale effects of smaller-scale processes without effectively modeling them. These parameters, such as the amount of clouds present within one grid box, are typically based on empirical data and further adjusted or—using climate science jargon—'tuned' in the course of the modeling process.[11] This tuning process, however, does not turn the external sub-grid-scale processes into genuine internalities of the model. As empirically parameterized entities they are not internal to the model and thus remain external to the understanding of climate change such models provide.

Another interesting separation between internalities and externalities happens on the level of GHG emission treatment within climate models. It is only by excluding the messy social relations that drive GHG emissions and by focusing narrowly on their universal physical properties, Demeritt (2001: 314) argues, that atmospheric scientists, concerned as they were about homogeneity and predictability, have been able to capture the issue of climate change. Whereas 'residence time,' 'radiative signature,' and 'photochemical reactivity' are thus considered relevant properties of GHG emissions to climate modelers, the historical origins of these emissions are not, installing a mere indifference between, for instance, luxury and subsistence emissions.

In illuminating the constitutive role played by particular concerns such as globalism, simulation and prediction in the historical emergence of climate modeling and its intrinsically emergent pattern of inclusions and exclusions, I have firmly adhered to my compositionist understanding of scientific practice as explained above. Indeed, along the lines sketched here, I tend to argue that climate modeling composes climate change as a global-scale problem caused by the universal physical properties of GHGs. Quite similar to Galileo's free fall and his experimental procedure, the prevailing image of what global climate change 'is' and how we can 'know' it was co-shaped through the approach of computer modeling.

2.3. Science Is Political—Second-Order Claim

In elaborating on my compositionist account of climate modeling, I have been working towards a particular conception of the politics of science, which can now be summarized as follows. Science, conceived as a concernful work of composition, is necessarily political as it entails a dialectic separation between what has been taken into account in the composition (internalities) and what has not (externalities). I call this a second-order claim that science is political to distinguish it from the first-order 'sloganesque' version outlined above.

Indeed, it should first of all be clear that a differing interpretation of the 'situatedness of science,' i.e., the awareness that science is a human and therefore necessarily perspectival and value-laden endeavor, is at the heart of what differentiates my second-order claim from most first-order claims. First-order claims tend to—negatively—understand the situatedness of science as resulting in a restricted, suboptimal knowledge of the problem at stake, a knowledge that becomes the more 'contaminated' by uncertainty and value-ladenness, the more 'situated' the issues get. Turning the first-order argument upside down, my second-order claim positively understands the situated, human and—as I have argued more specifically—concerned character of science as being truly constitutive of the knowledge composed and as inevitably resulting in a division between what has been taken into account and what not. Thus, rather than being a secondary, negative property that keeps it away from being perfect, the political character of science has to be understood as a primary, positive quality of all scientific knowledge. The case of climate modeling affirms this: what it means to observe climate change is inextricably intertwined with the concerns of homogeneity, simulation and prediction that have guided climate modelers in their daily practice. In a recent article, confirmed constructivist Brian Wynne clearly argues along this line when he says that "woven into the disciplined attempt to understand what nature is saying to us about changing climate processes are always ancillary but constitutive concerns and commitments" (Wynne 2010: 291).

A second major characteristic of my second-order claim is its grounding in a dialectical relation between inclusion and exclusion: externalities are constitutive of internalities and vice versa. As already argued, neglecting the political economy that drives GHG emissions enabled climate scientists to understand the effects of increasing GHG concentrations on the climate system. "Such physically reductionist abstractions render the world analytically manageable," Demeritt (2001: 314) says, adding that "it is probably something that we cannot do without." A similar point can be made about the differentiation between grid-scale and sub-grid-scale processes. Put quite bluntly, climate modeling would simply have been impossible without the introduction of a grid-scale and the associated pattern of inclusions and exclusions. It might be tempting to regard this separation between internalities and externalities as a deliberative process scientists perform in order to cope with the practical limits they are confronted with (time, computing power, etc.) in view of the infinite task of modeling 'the world.' But this would miss the point that it is precisely their concernful involvement with 'the world' that allows climate scientists to compose a meaningful conception of what climate change is. Indeed, such an account of externalities as negative side effects of the imperfect character of actual scientific practice, would bring us back to the first-order claim that science is political because in practice it cannot live up to the idealistic view of the uninterested, unconcerned scientist who faithfully represents a reality that is independent of him. In my view however, the incompleteness of scientific knowledge has to be regarded as a primary, constitutive characteristic: both the indifference towards sub-grid-scale processes as towards the political economy of GHGs are to be understood as blind spots that act as a background against which the scientific image of global climate change stands out.[12]

Thirdly and finally, it is important to see that my understanding of the political does not necessarily entail a societal connotation in the sense that exclusions would intrinsically constitute an issue for socio-political concern. Although externalities such as the unequal political economy that produces GHGs may act as the germ of a political identification, as illustrated by the so-called Climate Justice movement (see below), this does not have to be so as may be clear from the example of the 'excluded' sub-grid-scale processes. In stating that science is political, I thus merely claim that it differentiates between the internalities and externalities of its composition and that this differentiation process lies at the very heart of the scientific practice.[13]

This point reflects a similar distinction between "political" and "politics" made by so-called "radical democracy" theorists such as Mouffe and Laclau (Laclau and Mouffe 2001). For them, the political is an ontological notion referring to that which enables the constitution of identity as a hegemonic construction. Politics, by consequence, should be conceived on an ontic level as the very terrain of constituting identity (Daly 1999). At the heart of the radical democracy project stands a fundamental critique of the idea of the political holding sway over a great deal of contemporary democratic thinking, characterized as it is by rationalism and universalism. Invoking the term "post-politics," Mouffe (2005) and Rancière (1998), amongst others, lament the evacuation of antagonistic notions such as exclusion, adversary and contestation from the political sphere, because it reduces politics to a mere instrumental conception focused on the consensual administration of environmental, social, economic or other domains. Recently, Swyngedouw (2010) convincingly illustrated how international climate policy serves as a pre-eminent example of such a technocratic management approach, tellingly characterizing it as post-political populism: invoking the threat of a scientifically framed doomsday scenario, it paralyzes the struggle between differing and opposing socio-environmental visions, making everybody toe the line of neoliberalism. Contrary to such an instrumental conception, radical democracy theorists thus argue that the political should be conceived as an ontological dimension that determines our very human condition because every identity is constituted in and through its necessarily antagonistic relations with diverse others. Indeed, according to Mouffe, human society is essentially political, first of all, because "the need for collective identifications will never disappear since it is constitutive of the mode of existence of human beings" (Mouffe 2005: 28) and, secondly, as "in the field of collective identities, we are always dealing with the creation of a 'we' which can exist only by the demarcation of a 'they'" (Mouffe 2005: 15).

In putting forward my second-order claim that science is political, I am thus expanding this non-essentialist, antagonistic thesis from the social to the natural sphere, arguing that the construction of order more generally (identity, knowledge, etc.) is relational, its condition of existence being the affirmation of an exclusion. Or, as Daly (1999: 64) argues, "all identity, order and objectivity, must be considered as fully discursive: that is, as phenomena which are wholly the result of articulatory and political practices and which are ultimately prone to other articulatory practices." In connecting a Latourian-inspired compositionist account of scientific practice with an antagonistic notion of the political, I thus clear the way for a simultaneous rethinking of the natural and the social order as one political order that is held together by the constitutive role of concern. Understanding the question of 'what should we be concerned about?' in this 'political' way as an a priori question introducing a differentiation, shifts the attention from a debate over matters of fact in terms of true and false (logical truth) towards a debate over matters of concern in terms of who and what to take into account (topical truth). Is everything political then, one might ask, because it seems that for there to be some *thing* there must be a concern. My answer is unequivocal: yes, everything is political in the ontological sense I propose here. This also means that, quite unlike the first-order claim, my second-order conception of the political character of science does not necessarily pertain to a particular set of 'socio-political' values or concerns. As already hinted at and as will be elaborated in the next sections, it is rather the kind of resistance that externalities provoke that renders a matter to a specific terrain of politics, be it scientific, economic, ethical, cultural, religious or indeed socio-political.[14]

3. The (Im)possibility of Democratic Rationalization: Meeting Points with Feenberg's Critical Theory of Technology

Although explicitly dealing with the political, so far my analysis has not touched upon what could be called a politics of science and technology, which—following Mouffe and Laclau's distinction between politics and the political—should be understood in the sense of societal reform or steering. In this section, I want to take up a double task. First of all, I will deal explicitly with the politics of science and technology by confronting my vision of science with Feenberg's critical theory of technology, which is explicitly aimed at democratic reform. Under the banner of "democratic rationalization," Feenberg has extensively elaborated on the intrinsic possibilities for democratic engagement embedded in technological networks, assuming that the latter always already contain the citizen-as-user. On this point, and this constitutes the second part of my task, I want to investigate if and to what extent Feenberg's theory of democratic rationalization can be extended to the domain of science. Interestingly, this will not only lead to a comparison of our political approach, but also to the question whether there is a need to differentiate between (the democratization of) science and technology. I will proceed along three so-called 'meeting points' between Feenberg's critical theory of technology and my compositionist account of science, revolving around the conceptualization of the 'political,' the dialectic dimension and the notion of participation, respectively. In illuminating points of convergence as well as friction, I aim to further explicate my own approach while, hopefully, also adding something relevant to Feenberg's elaborate and comprehensive body of thought.

3.1. Indeterminism as the Foundationless Foundation of the Political

A first meeting point I want to discuss is that both Feenberg and I explicitly claim that technology and science, respectively, are political in themselves rather than this being a secondary characteristic. Although our respective notions of the political may differ in some respects (see below), both our claims are based on a constructivist recognition of the irreducibly indeterminist character of scientific and technological progress.

In my antagonistic approach to science sketched above, I indeed focused on the constitutive role of concern in the contingent emergence of scientific objectivity, this being reflected in the inscription of inclusion/exclusion pairs in the final composition. In this view, scientific objectivity has no ultimate foundation, neither in an external human-independent reality nor in an internal human-centered subjectivity. Rather, objectivity is relational in the sense that its construction, or rather its composition, necessarily entails the demarcation of what is excluded and thus not taken into account. This absence of an ultimate foundation eventually serves as the very foundation of my political account of science; the latter's political moment being situated at the point of deciding what to take into account. I thus abstain from defining science or, more accurately, scientific objectivity, by politicizing it.

Drawing on the notion of "topical truth," I have indicated that science can indeed be regarded as a scene of struggle for what to be concerned about. Understood this way, Galileo's law of the free fall is as universally objective as it is contingent. In defining the free fall through his experimental procedure, Galileo can be regarded as having made a decision in a field of undecidability, thereby demarcating what he considered relevant and what not. Based on my earlier account of climate modeling, a similar argument can be made with regard to climate change: defining climate change implies the contingent installation of a—now politically understood—differentiation.

In this respect, it is illuminating to dig a little bit deeper into radical democracy's theoretical framework, which I already touched upon in explicitating my first-order claim that science is political (see above). Interestingly, Laclau and Mouffe (2001) argue that their approach is grounded in privileging the moment of political articulation, or what they also call "hegemonic" articulation. Drawing on the deconstructionist notion of undecidability, hegemony is put forward by them as "a theory of the decision taken in an undecidable terrain" (Laclau and Mouffe 2001: xi). The very condition for a hegemonic relation to become possible, Laclau and Mouffe argue, is that a particular social force assumes the representation of a totality that is radically incommensurable with it. Such a representation is then characterized by what they call a "political universality" in the sense that it depends on "internal frontiers within society" (Laclau and Mouffe 2001: xiii). As already argued, my central point that the prevailing structuration of the natural order is founded in the irreducible political moment of deciding what to take into account and what not extends this view on hegemonic relations from the social to the natural sphere. In this sense, my account of Galileo's law of the free fall as being as much universally objective as it is contingent could be interpreted along the lines of Laclau and Mouffe as a scientific representation possessing a similar kind of political universality or, more aptly, 'political objectivity.' Indeed, further resonating with my compositionist account of science, they write that "In order to have hegemony, the requirement is that elements whose own nature does not predetermine them to enter into one type of arrangement rather than another, nevertheless coalesce, as a result of an external or articulating practice" (Laclau and Mouffe 2001: xii). This view is also reflected in Daly (1999: 75) where he writes that: "objectivity cannot be identified positivistically but is shown to grow out of negativity and antagonism as its anti-foundationalist 'foundations.'"

Although making no explicit reference to the work of Mouffe and Laclau, Feenberg politicizes technology in a very similar way when he says that "What a technology is depends on what it is for, and that is often in dispute" (Feenberg 2010a: 11). Similar to my view on science, Feenberg's constructivist account of technological progress depicts technology as a scene of struggle for what to be concerned about. "Technical design," he argues, "is not determined by a general criterion such as efficiency, but by a social process which differentiates technical alternatives according to a variety of case-specific criteria." "This social process," he continues, "is not about fulfilling 'natural' human needs, but concerns the cultural definition of needs and therefore of the problems to which technology is addressed" (Feenberg 1999: 83). Put briefly, where I abstain from defining scientific objectivity, Feenberg does not give a definition of technological efficiency either. Rather, this is politicized in the sense that technology is conceived as a scene of social struggle where the very definition of a particular technology is precisely that what is at stake. In other words, if technology does have an essence for Feenberg, it can only be found in its very political character: "I will define the essence of technology as the systematic locus for the socio-cultural variables that actually diversify its historical realizations" (Feenberg 1999: 201). One example, taken from Pinch and Bijker (1984), Feenberg frequently employs is that of the history of the bike, where two competing bike models existed in parallel, each with their own set of values attached. As we all know, the bike with two equal-sized wheels, at that time conceived as a transportation tool with a main focus on safety, won out over the one with a high-front wheel, which was regarded as a sports bike being less safe but enabling higher velocities. Quite similar to my argument about climate change and falling objects, it was the very definition of the bike that was at stake here.

All this goes to argue that Feenberg's critical theory of technology and my compositionist account of science share a common view on their respective political character.[15] Paralleling my thematization of the questions of science (rather than its answers) invoked under the notion of topical truth, Feenberg also makes the problems to which technology is addressed into his major theme. Rather than asking whether a technology is the right solution to a given problem, the needs to which a particular technology is attached are questioned with respect to an environmentally sustainable and socially just society. Indeed, just as I have argued that topical truth cannot be but a relational measure (Goeminne 2011c), such a questioning of technology's needs can only be relational invoking societal goals such as justice or sustainability. In this way, making room for the 'topical question' of what we should be concerned about in the scientific and technological sphere eventually develops into a question about the possibility of external, socially informed reform of science and technology.

The possibility of reform, which I will turn to in a next paragraph, is perhaps the ultimate common ground between Feenberg's work and mine. Why, after all, does Feenberg not give a determinist definition of technology and why do I not do so for scientific objectivity? I am putting it as a question because I am convinced one can give such definitions and present convincing deterministic histories of technological as well as scientific progress. In my view, however, in the end, the choice between a constructivist and a determinist approach is a matter of decision rather than of looking at the historical evidence. *If* one understands the Minitel—to take another example Feenberg frequently uses (see, e.g., Feenberg 2010a: 100–04)—in its final, socio-technical shape as a given, in the sense that it could not have been different, then one can trace back its history revealing all kinds of inevitable (design) choices and evolutions that lead to the Minitel as we know it. However, *if* one takes the Minitel as but one possible outcome of a contingent process, in the sense that it could have gone different ways (following the so-called symmetry principle of constructivism), then one will come up with all kinds of 'points of multistability,' that is to say branching points where 'it' (the Minitel in its makeshift socio-technical shape) might well have taken a different historical direction. Of course, in that case, the Minitel wouldn't have been the Minitel. By looking at the factual history, one will never find decisive 'proof' for the truth of either of these 'theories;' the historical facts only become facts when viewed and interpreted from a particular (historical) perspective. In line with my compositionist account of science, the question whether technological evolution is determinist or constructivist thus appears to me to be a false question, rather it is a matter of a decisive choice. Feenberg's project, as well as mine (see, e.g., Goeminne 2011a) is explicitly political in the sense that it openly argues for the need of an alternative society—more equitable, more sustainable, etc. But in order to plead for change, one must think from a framework that allows it. If you want to shed light on how alternative paths of development might be pursued, moving from an existing situation to a future one, you cannot but think in a constructivist way. Feenberg is genuinely convinced things can and need to be changed—no wonder he is a constructivist! Or, as he himself writes, rephrasing Sartre: "In a society where determinism stands guard on the frontiers of democracy, indeterminism 'enlarges the field of the possible'" (Feenberg 2010a: 13). Interestingly, in other instances, the last part of the sentence is replaced by "indeterminism cannot but be political" (Feenberg 1995: 8).

3.2. Democratic Rationalization: A Dialectical Approach to Technological (and Scientific?) Reform

As already hinted at, the possibility of reform constitutes a second meeting point between my work and Feenberg's critical theory. Under the banner of "democratic rationalization,"[16] Feenberg has extensively elaborated on the need and possibility of technological reform. Further exploring our shared dialectical approach to scientific and technological progress, respectively, I want to analyze if and to what extent the theoretical framework of democratic rationalization can be extended to science, something Feenberg has touched upon only sporadically in his work.

As argued, my antagonistic approach to science revolves around a dialectic between inclusion and exclusion, extending Mouffe's claim that "every order is political and based on some form of exclusion" from the social to the natural sphere. Mouffe further builds on the idea that exclusion represents the condition of possibility of inclusion (and vice versa) to argue for the very possibility of counter-hegemonic practices, that is to say "practices which . . . attempt to disarticulate the existing order so as to install another form of hegemony" (Mouffe 2005: 18). Exclusions thus act as what Mouffe calls a "constitutive outside" around which a "collective identification" can arise striving for an alternative hegemonic order. In this sense, the theoretical potentiality for reform is always already embedded in the installation of a particular order. Daly (1999: 76) words it as follows: "The question of objectivity and identity—its failure and undecidable recomposition—is always going to be a political matter in which present certainties are essentially vulnerable to future subversion." And he goes on to say that "there is no possibility of any identity or objectivity establishing a final closure; in consequence there is always the possibility (indeed an essential possibility) of contingent political disruption and subversion" (Daly 1999: 78).

Feenberg very similarly argues that in cases "where the social definition of major technologies is in transition," typically, "social groups excluded from the original design process articulate their unrepresented interests politically" (Feenberg 2010a: 20). In line with the Mouffian idea of a constitutive outside, the notion of resistance between included and excluded groups is crucial to Feenberg's view on reform. "That resistance," Feenberg writes, "takes many forms, from union-struggles over health and safety in nuclear power plants to community struggles over toxic waste disposal to political demands for regulation of reproductive technologies. These movements alert us to the need to take technological externalities into account and demand design changes responsive to the enlarged context revealed in that accounting" (Feenberg 2010a: 26). Feenberg's examples of technological regimes that got transformed through users reappropriating a technology are well-known: the Minitel in France that was hacked and redefined from a top-down, one-way information service to a communication tool, as well as medical treatment that got redefined by AIDS patients from a rather technical treatment issue into a broader socio-technical caring issue. Like the environmental movement, Feenberg writes, "these resistances challenge the horizon of rationality under which technology is currently designed" (Feenberg 2010a: 28). This dimension of resistance is key to what Feenberg calls democratic rationalization, arguing that this "requires technological advances that can be made only in opposition to the dominant hegemony."

It is important to see that Feenberg's approach revolves around excluded social subgroups in technological progress. In fact, Feenberg's focus on the user-participant as an intrinsic dimension of technology underlies the distinction he makes between science and technology. Discussing Marcuse's ideas about a possible "successor science," he argues to maintain a clear distinction between science and technology (Feenberg 2002: 170–75). Feenberg sees a fundamental difference in the sense that a clearly distinguishable and limited scientific community governs scientific progress whereas technological progress always already involves a user community. "Despite their growing interconnections," Feenberg writes, "science and technology are very different institutions. The difference shows up in the reform programs that sound plausible in the two cases: political reform for technology and reform from within for science" (Feenberg 2002: 164). Whereas, according to Feenberg, technology is always already social in the sense that technology only is what it is in some user-context, science is not. Based on this difference, Feenberg argues that his democratic rationalization approach does not lend itself to science, as it would, first of all, be regarded as an unjustified socio-political intervention from the outside and, secondly, run up against the critique of being a "threat to rationality as a whole" (Feenberg 2002: 175).

In the following, I want to challenge Feenberg's distinction between science and technology on two levels, a theoretical and a practical one. This will allow me to argue that his democratic rationalization approach lends itself to be applied to the scientific sphere as well, at least in principle. First of all, on a theoretical-conceptual level, I think it is not correct, and at best a reactionary idea, to conceive of the scientific community as a relatively closed sphere that is isolated from society (cf. the sociological idea of the 'core set').[17] Although he grants social, cultural and economic constraints an indirect role in scientific debates, Feenberg argues that epistemic tests (experiments, peer review) are the principal measure of competing ideas, saying that "it is only when science leaves the laboratory and enters society as technology, it must serve many other interests besides the interest in knowledge" (Feenberg, forthcoming). This sounds to me pretty close to what I have called a first-order claim that science is political in that it sees value-ladenness, bias etc. as secondary, suboptimal characteristics of science, keeping the ideal of a non-biased, value-free science very much alive. Although Feenberg explicitly says that he does "not mean that scientists arrive at absolute truth" (Feenberg, forthcoming), he does seem to be much less a constructivist about science than about technology. Based on my second-order claim, I disagree on the grounds that concerns and interests are always already part of science, even in its deepest core.[18] Interestingly, my argument can be rephrased in terms of Feenberg's instrumentalization theory, which holds that, in the case of technology, the dimensions of function and meaning, captured under the notions of primary and secondary instrumentalization, respectively, can only be separated analytically as, in reality, they are always already intertwined. Whereas Feenberg's claim of science and technology being different could be recast as science being only a matter of primary instrumentalization—or perhaps more aptly, objectification—I would argue that science, similar to technology, always already involves a secondary objectification, giving way to what I have called a political objectivity (see above).

On a more concrete-practical level, I would argue—against Feenberg—that science can be as much socially embedded as technology can be isolated from society. "The reasons for the difference between the role of the public in science and technology is simple," Feenberg writes, "while scientific theories are abstractions and experiments confined to the lab, technologies supply environments with which ordinary people live" (Feenberg, forthcoming). In other words, technology is for Feenberg always already a medium forming the lifeworld of ordinary people, whereas science remains a specialized activity, only affecting them through technology (Feenberg 2002: 174). However, I find it as hard to imagine a non-knowledge-mediated world as a non-technologically mediated one. What to think of scientific knowledge used in policy-making or medical care: does it not mediate our relation with the world, shaping both our perceptions as well as our actions? And even in a less 'applied' fashion: does climate change knowledge not shape our lifeworld? I have argued before that without climate science, we would never have thought of a thing as a global climate. Now, some forty years later, we all feel somehow connected to it, co-shaping for instance our perception of a warm late autumn day as well as our energy-consuming practices. The other way around, I think one may well argue that technological activity can be relatively fenced off from broader society, as is for instance the case in nanotechnology research into new materials.[19] In challenging this difference, I see no reason of principle not to apply Feenberg's framework of democratic rationalization to science. A clear example can be found in the realm of climate change. I have already explained how climate modeling involves a neglect of the socio-economic context in which greenhouse gas emissions originate. As Agarwal and Narain already argued in 1991, this indifference persists on the level of climate policy: the attempt to create a global carbon trading scheme predicated upon the scientific universality of GHG emissions in effect erases the historical origins of the emissions, making no difference between luxury and subsistence emissions (Agarwal and Narain 1991). The so-called Climate Justice movement has taken issue with this externality of current climate policy, arguing that such an approach, however cost-efficient it may be, is not equitable. In Mouffian terms, this grassroots movement can be regarded as a collective identification around the constitutive outside of the reigning order of science-based climate policy.

At this point, I propose to replace Feenberg's rather absolute distinction between science and technology by a differentiation in 'social embeddedness,' that is to say the degree to which a product of science or technology acts as a medium.[20] In my view, this is a more relevant differentiation, as it is precisely this degree of embeddedness that plays a decisive role in whether or not externalities, be they scientific or technological, can give rise to a collective identification in the social sphere.[21] In this respect, it is interesting to have a closer look at Kuhn's theory of scientific revolutions as an account of 'internal' scientific progress. From my compositionist point of view, Kuhnian revolutions can be regarded as a taking into account of the anomalies (violations of the paradigm-induced expectations that govern normal science) of a contested paradigm through the composition of an alternative paradigm. In challenging and addressing the anomalies of Aristotelian physics for instance, Galileo can be regarded as coming up with a new paradigm, a new composition, which, although incommensurable with Aristotle's composition, still embraces it. Indeed, although Aristotle used notions such as 'substance,' 'matter' and 'natural place,' all of them incommensurable with Galileo's paradigmatic way of looking at falling objects, his theory is perfectly explainable from a Galilean point of view by introducing the notion of friction. All this adds up to a dialectical picture of scientific progress, quite analogous to the case of the Climate Justice movement. Indeed, at the core of both examples is a repoliticization of science by challenging the reigning pattern of inclusions and exclusions. However, whereas the Climate Justice case arguably qualifies as democratic rationalization, the Kuhnian revolutions do not, the decisive factor being their respective degree of social embeddedness. Perhaps they could be regarded as 'expertocratic rationalizations.' As pointed out with regard to climate science, exclusions, such as the sub-grid-scale processes of climate modeling, do not necessarily entail a socio-political concern. This brings me to argue that while science and technology are both political by definition, they are only potentially susceptible to democratic rationalization, the latter depending on whether people take issue with them or not.

In this respect, it is worth noting that my account of the political character of science and technology is more encompassing than that of Feenberg. Indeed, Feenberg restricts technology's political character to the social sphere, focusing as he does on the social definition of technology and its redefinition through democratic resistance. That is also the reason why, wrongfully in my opinion, he does not grant science the same political or constructive character as he does for technology, arguing that science does not penetrate the social sphere the way technology does. In this sense, my conceptualization of the political and the inherent possibility of dialectic reform—whether 'expertocratic' or 'democratic'—it embraces, could be regarded as an extension of Feenberg's democratic rationalization; an extension, as should be clear by now, not so much from technology to science, but rather from the social sphere to the natural sphere, blurring the boundaries between both. In this way, substituting a differentiation in the 'mediumness' of their products for a clear distinction between science and technology themselves will also do away with Feenberg's warning against external political interference for science, while at the same time embracing the idea in the case of technology (Feenberg, forthcoming). As I will discuss in the next paragraph, in my view ordering up functions is potentially as dangerous as ordering up truths.

3.3. On the Notion of Participation and the Illusion of Total Inclusion

A third and final meeting point revolves around the issue of participation, looking closer into the more practical channels along which Feenberg thinks democratic rationalization can take place. As already mentioned, his politics of technology takes its point of departure in the resistances emerging when people take issue with the externalities of the hegemonic way a technology is embedded in society. In the foregoing I have tried to extend this approach to the field of science, arguing—contrary to Feenberg—that it is not so much the distinction between science and technology but rather the extent to which their products shape the lifeworld of people that determines whether they are susceptible to democratic reform. In my antagonistic terms: as both science and technology are always already political in the sense that actual scientific and technological (arti-)facts inherently imply a relevant differentiation between internalities and externalities, they always already constitute a potential issue for politics, depending on whether people take issue with them or not. To the extent that citizens can be regarded as intrinsic participants, it is the technoscientific network itself that thus serves as an alternative locus of democratic agency enabling people to respond by resisting, transforming or otherwise challenging the values embedded within the technologies, as expressed in what Feenberg calls their technical code (Brown 2009: 145). The word 'alternative' is of the essence here, as it reflects Feenberg's differentiated approach to conceive of complementary democratic channels taking the world-defining characteristics of technology to heart and rejecting the populist naïve belief in substituting a representative system for direct democracy (Feenberg 1999: 133–35).

Feenberg sees three main channels for this kind of democratic participation, all of them extendable to the scientific sphere: technical controversies, creative appropriation and innovative dialogue. I have already hinted at the Climate Justice movement as a case of scientific controversy contesting the narrowly scientific framing of climate change that is centered on the universal physical properties of GHGs. Likewise, the relatively new research field of ecological economics can be regarded as a counter-hegemonic 'knowledge' that creatively appropriates the prevailing economic paradigm embracing the latter's societal externalities, such as environmental degradation, through the adoption of fundamentally different founding parameters (Costanza and Daly 1987). Finally, participatory approaches to science such as those going under the name of 'post-normal science' or 'sustainability science' have also been put forward as an innovative way to merge expert and lay perspectives in the production of socially relevant knowledge (Goeminne 2011a).

In extending Feenberg's political framework from technology to science, it would seem that my compositionist account of science and Feenberg's critical theory of technology seamlessly align. In my view, however, an element of friction remains present on comparing my specific antagonistic approach with Feenberg's focus on participation in technology as a means to achieve the inclusion of a range of interests as wide as possible. Central to my dialectic approach to scientific reform—whether internally or externally informed—is the awareness that exclusion and antagonism are at the same time its condition of possibility and the condition of impossibility of its full realization (Mouffe 1993). In other words, the composition is—by definition—never complete; only a political universality can be attained. Building on Lukács's dialectical concept of the "mediated totality," Feenberg (2002: 165–70) also rejects the idea of an originary totality that could be recovered. Indeed, I could not agree more when he writes that "totality is thus not a synoptic view or conceptual myth . . . but the basis of an immanent critique" (Feenberg 2002: 167).

And still I cannot rid myself completely of the impression that the possibility of an 'inclusive totality' is occasionally lurking in the background of Feenberg's work. For instance, when Feenberg introduces the notion of decontextualization as one of the four reifying moments of the primary instrumentalization, he writes: “The tree conceived as lumber, and eventually cut down, stripped of bark and chopped into boards, is encountered through its usefulness rather than *in all its manifold interconnections* with its environment and the other species with which it *normally* co-exists” (Feenberg 1999: 203; emphasis mine). Here, Feenberg seems to imply an originary totality or *normality* where a tree could be encountered in *all* of its manifold interconnections with its environment. In my view, the other tree qualities that Feenberg lists, such as a habitat or a source of shade, can also only exist as such as the result of an—albeit different and incommensurable—(primary) instrumentalization. In my compositionist terms: without a concern for ‘usefulness’ there would not even have been a tree to cut down; nor would there have been a tree to relax under without there having been a concern for some kind of protection or shelter. And yet other qualities of the tree cannot even be imagined precisely because the pertinent instrumentalization has not been made yet. Similarly, when Feenberg writes that “since decontextualization predestines technology to serve capitalist power, socialism must recover some of those contextual elements lost in the narrowing of technology to class-specific applications” (Feenberg 2002: 184), I cannot but think of a non-narrowed technology that has recovered all of its lost contextual elements, serving no power at all.

In my view, this idea of an apolitical, originary totality that can be recovered by including all possible perspectives also reverberates in Feenberg’s take on “innovative dialogue” as a possible channel for democratization (Feenberg 1999: 123–25). In bringing designers (the ‘primary instrumentalizers’) and users (the ‘secondary instrumentalizers’) together, Feenberg sees the promise of a “fundamental solution to the conflict between lay and expert” (Feenberg 1999: 125). Resonating with Verbeek’s postphenomenologically inspired idea about actively giving shape to technological mediations through participatory design (Verbeek 2008), Feenberg also seems to fall prey here to the illusion of total inclusion in apparently presuming that innovative dialogue could shortcut the—in my view necessary—process of exclusion, by ex ante including the excluded. “The range of interests represented by those in control of technology,” he writes, “must be enlarged so as to make it more difficult to offload externalities from technical action onto disempowered groups” (Feenberg 2010a: 80). In my reading of instrumentalization theory, this sounds as if function and meaning could be molded together through an imaginary anticipation of their distinction, which is only analytically possible. Although I agree with Feenberg that participatory design and innovative dialogue could constitute novel democratic channels, I would like to complement the argument by pointing towards the need of treating these channels as being intrinsically as political as the expertocratic channels they are supposed to replace and thus of keeping them open to contestation.

Indeed, according to my compositionist account, any technoscientific procedure, whether democratically reformed or not, should not neglect the irreducible political moment that is situated at the point of deciding what 'the issue at stake' is or should be. As I have argued at length elsewhere in the context of participatory approaches to science (Goeminne 2011a), the idea of injecting values into the design process in order to arrive at an unbiased procedure relies—quite symptomatically—on the same correspondence view of representation as the expertocratic politics it aims to replace, appealing to a conception of political representation as a mirror of the pre-existing reality of either popular will or public interest (Brown 2000). In that sense, I am as skeptical about ordering up functions 'on command'—even if it is a participatory informed command—as Feenberg is about ordering up truths by keeping science under social restraint, as happened in the USSR with Lysenkoism (Feenberg, forthcoming). It is precisely Mouffe and Laclau's antagonistic approach and their inherent critique of consensus-driven "post-politics" that makes me very sensitive—and perhaps oversensitive with respect to Feenberg's work—to participatory approaches that lack political self-awareness. Indeed, "the central role that the notion of antagonism plays in our work," Laclau and Mouffe write, "forecloses any possibility of a final reconciliation, of any kind of rational consensus, of a fully inclusive 'we'" (Laclau and Mouffe 2001: xvii), which is why they stress that "it is vital for democratic politics to acknowledge that any form of consensus is the result of a hegemonic articulation, and that it always has an 'outside' that impedes its full realization" (Laclau and Mouffe 2001: xviii).

It is indeed symptomatic of their incapacity to think of the excluded that participatory approaches often turn out to be exclusive in a dogmatic way. In my view, this is exactly what happened at the COP16 climate summit in Cancun (December 2010) where the blame was pinned on Bolivia for its fierce resistance against a weak agreement. Rather than as a subversive act, Bolivia's resistance should be understood along the political line I have put forward here. In resisting a weak agreement, I contend, Bolivia fundamentally questioned some of the externalities of the Western industrial development pattern and the way these threaten their own development. However, within the post-political contours of technocratic consensual decision-making where externalities are at best internalized in terms of market corrections, a demand for these externalities to be taken into account as part of an alternative socio-environmental composition can only appear as radical opposition. In Cancun this eventually led to the point where Bolivia was accused of putting humanity's fate at stake.

This is, however, not to say that Feenberg is on this post-political line. Indeed, at the very point where he argues that "the problem is not primarily one of legal rights but of initiative and participation," it immediately follows that "legal forms may eventually routinize claims that are asserted informally at first, but the forms will remain hollow unless they emerge from the experience and needs of individuals resisting a technocratic hegemony" (Feenberg 2010a: 26). Indeed, Feenberg's theory of democratic rationalization depicting technology as a public scene of democratic struggle where conflicting hegemonic views can contend could be seen as a further concretization of Mouffe's rather abstract view of public space as the place where there is an opportunity for the expression of conflict and dissent rather than the place where we try to reach consensus (Mouffe 2005). In pointing towards what I think are at most potential 'post-political pitfalls' in Feenberg's work, I have thus no intention at all to criticize Feenberg's overall project. Quite the contrary, in doing so, I hope to add some 'antagonistic weight' to his theoretical framework. Perhaps, I am inviting Feenberg to add an eleventh paradox to his magnificent "Ten Paradoxes of Technology" (Feenberg 2010b), posing the question whether democratization is most genuine when it arises organically through collective identification around the constitutive outside of a hegemony or when it is conducted via institutional reform from above (Brown 2000).[22]

4. Conclusion

In his preface to *Questioning Technology*, Feenberg writes that the notion of radical democracy, although promising, "is so abstract it determines no substantive policies, and in fact does little more than certify the left credentials of the very divisions it hopes to overcome." "But perhaps," he adds, "it can begin to take on content through the question of technology" (Feenberg 1999: xiv). I fully agree and I think the current paper shows how not only technology but also science poses a major challenge to current democratic thought, even in its radical form. Indeed, radical democracy, although offering a rich theoretical framework for dealing with the antagonistic dimension of our human condition, does not get much further than empty calls for "a pluralism of modernities" (Mouffe 2005: 123–25) or "the articulation of divergent, conflicting and alternative trajectories of future socio-environmental possibilities" (Swyngedouw 2010: 16). While the nature-culture dualism persists, restraining thought on political subjectivity to the social sphere, it will remain rather vague where such alternative possibilities might originate other than from the occasional enlightened mind. What is lacking, to use the words of Daly (1999: 82), is "a conception of the practical forms of institutionality which would be capable of sustaining the grammar, or power system, of radical democracy." In this regard, I am convinced that Feenberg's approach of democratic rationalization is hugely relevant to current times, which are increasingly characterized by confrontations with the externalities of neo-liberal capitalism. Rather than falling back on the now omnipresent calls for 'radical otherness,' Feenberg's approach illuminates how radical change could take its very point of departure in what already is, the technoscientific network itself serving as an alternative locus of democratic agency enabling people to respond by resisting, transforming or otherwise challenging the values embedded within technologies and epistemes. But perhaps, as I have tried to show, there is also something to be learned from radical democracy and its sensitivity to the irreducible political character of human existence—even, and perhaps even more so, if such existence has become increasingly technoscientifically conditioned.

Notes

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1. In discussing (social) constructivism, it is common to distinguish between weak and strong versions. Whereas weak social constructivism holds the view that human representations of reality are social constructs, strong social constructivism claims not only that representations are social constructs, but also that the entities themselves to which these representations refer are socially constructed (Goldman 2010). Throughout the next sections I will explicate my own 'compositionist' constructivism, distancing myself from both the weak and the strong version.

2. In the following, I will depart from an admittedly oversimplified picture, contrasting Aristotelian and Galilean gravitation theory as temporal snapshots and neglecting historical continuities between them. In a first instance, this may seem to lead to an artificially dualistic image where Aristotelian and Galilean paradigms are radically opposite and in no way connected. While this preliminary dualistic image will serve the purpose of introducing my compositionist account, a further explication of the latter will nuance this oversimplified image. Throughout the next sections, when my account of the political character of science takes full shape, it will become clear that it also inherently contains the possibility of scientific reform through Kuhnian-like

revolutions (see, in particular, paragraph 3.2). For a comprehensive archaeology of Galileo's theory of motion, the reader is referred to Van Dyck (2006).

3. As extensively argued in Van Dyck (2006), the actual transition from Aristotelian to Galilean gravitation theory was much less radical with Galileo gradually moving to a position that, although being incommensurable with Aristotle's, still embraces it (see also paragraph 3.2.). As Van Dyck (2006) nicely explains, the very concept of weight underwent such a stepwise transformation under the Aristotle-Galileo transition.

4. It is important to add that, as Van Dyck (2006) extensively illustrates for Aristotle and Galileo and as I will do for the case of climate modelling in the next section, such concerns are always also expressions of a particular worldview.

5. As already mentioned, this is an imaginary debate, not only because the protagonists lived in different times, but also because I have purposively staged it in this dualistic way.

6. In a recent article entitled "An Attempt at a 'Compositionist Manifesto,'" Latour argues that after all the critique, deconstruction and debunking of postmodernism, it is now time to compose again (Latour 2010). Very similar to how I try to elaborate the notion of composition and its corollary notion of political objectivity (see further) in a way that conveys the idea of going beyond mere representation and construction, he argues that compositionism should "take up the task of searching for universality without believing that this universality is already there, waiting to be unveiled and discovered. It is thus as far from relativism as it is from universalism" (Latour 2010: 474). See also endnote 1.

7. With respect to the distinction between weak and strong constructivism made in footnote 1, it could be said that my compositionist perspective sees the weak version as too weak and the strong version as too strong. Whereas weak constructivism still holds on to the idea of a human-independent reality out there, the strong version basically maintains that the social context unidirectionally determines reality, the latter leading to an epistemic (and moral) relativism where humans could construct reality at will. My compositionist position comes perhaps close to what Kukla has called "constitutive constructivism" which holds that scientific facts, and with that all knowledge, are by necessity facts about human activity (Kukla 2000: 21). To me, detailed constructivist studies of scientific practice precisely show that all (scientific) knowledge of the world is preconditioned on a very specific, concernful human involvement with the world. In that sense my compositionism also rejects both strong and weak social constructivism for their shared one-sided focus on the scientific content as the explanandum, leaving the social context as 'explanans' unquestioned ('sociological reductionism'). In the following I will use the notion of constructivism in a broad sense to refer to the various post-positivist approaches that focus on the social embeddedness of science and technology.

8. The brief account presented here is mainly based on *A Vast Machine*, Paul Edwards's illuminating monograph on the history of climate modelling (Edwards 2010).

9. I use the word 'driven' here to discern my view from both strong and weak constructivist views, which hold that the social context determines the scientific content. Indeed, particular concerns can only be identified retrospectively as they themselves are also outcomes of one and the same composition process. The way, for instance, 'global' is now commonly understood arguably owes a great deal to the current scientific understanding of climate change as a 'global' issue.

10. It is important to note that the grid scale is imposed by the available computing power. In this case, indifferences with regard to externalities are thus not merely installed by the human, but rather by the human+technology. In recent times, science has become more and more technologically mediated, so it is important to take this technological dimension into account, in particular its non-neutral role in co-shaping the resulting knowledge. Concerns are thus co-shaped by technology, in this case by computing power.

11. In this respect, it is interesting to note that climate models are also data-laden just as, vice versa, climate data are always also model-laden.

12. To illustrate this view, one might think of the well-known figure-ground images such as the faces-vase drawing by Edgar Rubin. Also here, the 'external' ground image acts as a constitutive background against which the 'internal' figure image can stand out. And also here, it is merely impossible to see both images at the same time or, in my compositionist terms, to internalize the ground image in a genuine figure-like way.

13. See Goeminne (2011b) for an elaboration of this argument in the context of experimental nuclear physics.

14. See paragraph 3.2, where I elaborate on the idea of a Kuhnian revolution as expertocratic, i.e., internal scientific reform besides the possibility of a socio-politically informed reform of science. In the following, I will use the notion 'socio-political' to refer to this particular social terrain of 'conventional' political practices in order to distinguish it from my notion of the political.

15. This commonality will be nuanced in the next paragraph, where I will argue that my notion of the political is more encompassing.

16. Feenberg uses the notion of subversive rationalization interchangeably, thereby pointing towards the intrinsic user dimension of technology as the starting point for radical reform.

17. In Goeminne (2011a), I extensively argue why the view of science as an isolated, separate societal sphere is a reactionary idea. In this paper, I develop a constructive critique of 'post-normal science' and other scientific reform attempts by challeng-

ing the conception of normal science that underlies their argument and qualifying it as a reactionary idea harking back to an illusory 'pure science.'

18. Philosopher of technology Don Ihde is on my side here when he says in an interview that although he and Feenberg both draw from constructivism and are often very close to agreements, he claims to reject more deeply the older theory-bias of classical philosophy of science which touts 'rationality' than Feenberg does. For Ihde "all science, including natural science, is hermeneutic (critically interpretive) all the way down" (Ihde and Ralon 2010).

19. I use the word 'relatively' as, evidently, I argue that contextual concerns also play a constitutive role in this kind of specialized research.

20. It may well turn out that the higher 'medium' end of this continuum is mainly populated by 'technologies' rather than by 'scientific facts' but, as I will come to argue, I do not think that the science-technology difference is of the essence with respect to democratic rationalization being 'applicable' or not.

21. In this sense, the rich collection of public controversy studies within the field of Science and Technology Studies could be regarded as examples of how publics form and reform around new technologies as well as new epistemes.

22. Feenberg's scheme already contains a so-called 'democratic paradox,' which, however, does not deal with the question of 'how' to democratize technology.

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