

4-1-2008

Comments on Counterfactual Dependence, Thermodynamics and the Special Sciences

Valia Allori

Follow this and additional works at: <https://huskiecommons.lib.niu.edu/allfaculty-peerpub>

Original Citation

Comments on Jeffrey Dunn's paper: "Counterfactual Dependence, Thermodynamics, and the Special Sciences." APA Central Division meeting, Chicago, IL. April 17-20, 2008.

This Other is brought to you for free and open access by the Faculty Research, Artistry, & Scholarship at Huskie Commons. It has been accepted for inclusion in Faculty Peer-Reviewed Publications by an authorized administrator of Huskie Commons. For more information, please contact jschumacher@niu.edu.

Comments on Counterfactual Dependence, Thermodynamics and the Special Sciences by Jeffrey Dunn

In his paper, Dunn argues that he can fix David Lewis counterfactual account from the objection raised against it by Adam Elga suitably extending the Lewisian account including special science laws. I think that Dunn's account is interesting but it is not developed enough. In particular, it is unclear to me what his account of special science laws really is. It is also unclear to me what is the status of the special science laws in his picture. Moreover, the paper talks about the advantages over the alternative accounts without even mentioning what alternatives there are. To conclude, I would like to present a different proposal to fix Lewis's account, to understand the reasons why it does not work or it is worse than Dunn's account. I will try to be very brief in my comments, that mainly will be aimed to facilitate the discussion.

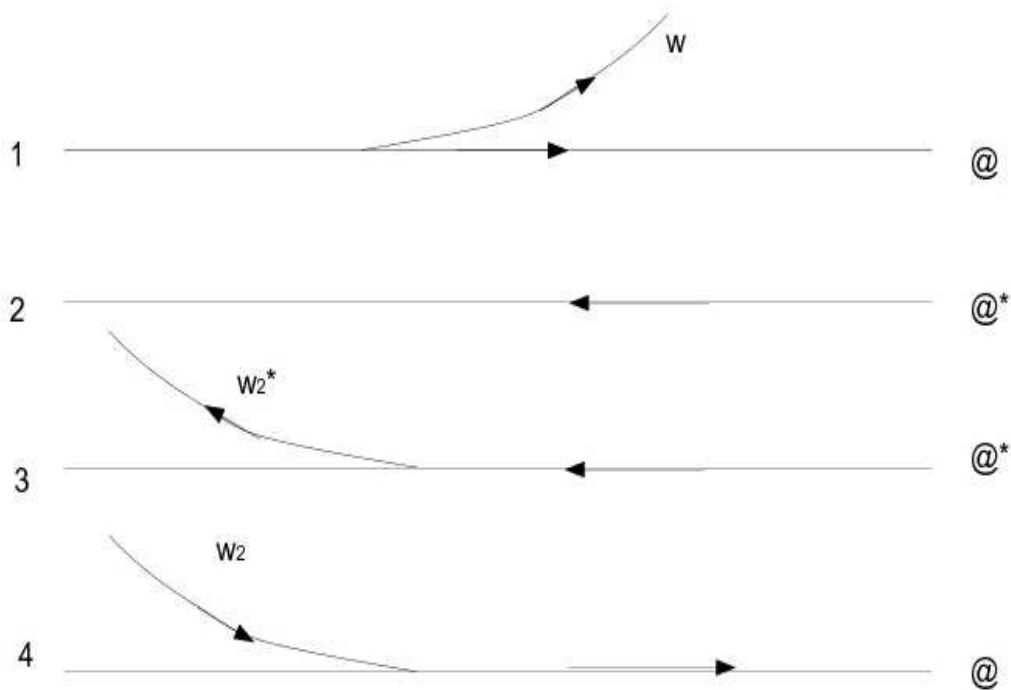
Before proceeding with my comments, I wish to summarize very quickly the main point of Lewis's theory, Elga's objection and Dunn's proposal. The account of Lewis is that a counterfactual $A > B$ is true just in case the world w in which A and B are true is the most similar to the actual world $@$ than any other world. The notion of similarity employed by Lewis is in terms of (in order of decreasing importance): big miracles, perfect match with facts, and small miracles. When using the locution *miracle* Lewis refers to the violation of the law that is required to make the counterfactual true. Elga's objection to Lewis consists in providing an example of a world w_2 in which the counterfactual $A > B$ is false but such that is at least as similar to $@$ as w , the world in which the counterfactual is true. Figure 1 shows a set of diagrams (borrowed by Barry Loewer's

Counterfactual and the second law¹) that I hope will be helpful in better understanding how these worlds are related the one to the other. The first diagram shows graphically the actual world $@$ and the world w in which A and B are true: w shows perfect match with $@$ and then is departing from $@$ by a small miracle. The arrow in the diagram show the direction of time. The second diagram shows $@^*$, that is our world but in which all velocities are reversed: this world is therefore like a movie projected backwards. Consider a world w_2^* that departs from $@^*$ by a small miracle. This world is depicted in the third diagram of Figure 1. The world w_2 is readily constructed as shown in the fourth diagram: take w_2^* and reverse all the velocities: as a result, the past of w_2 (relative to the point of

1 B. Lower: Counterfactual and the second law, ms1 in Dunn's bibliography

intersection between the two worlds) does not match the one of @ but w_2 converges to @ by a small miracle so that in the future we have perfect match. Note that all time-reversed worlds are possible, given the time-symmetry of the equations of motions of the fundamental physical theory that is supposed to truly describe our world. Now, compare the first and the fourth diagram and ask yourself: which world is more similar to @, w or w_2 ? Lewis's account of similarity is not specific enough to allow for w to be the one, since also w_2 departs from @ by a small miracle and the size of perfect match between of @ and w_2 and @ and w is the same, even if one is in the future and the other is in the past.

Fig. 1



Dunn claims he can vindicate Lewis's intuition that there is something happening in w_2 that makes it less similar to @ than w . The idea is to modify the characterization of similarity to account for the fact that in w_2 there is a violation of laws that does not happen in w . The laws Dunn is thinking about are not fundamental laws but are the ones of thermodynamics. While it is the case that the laws of fundamental physics are temporally symmetric, the laws of thermodynamics, the laws that govern the macroscopic world, are not: all macroscopic phenomena are time-antisymmetric. This is accounted for by the second law of thermodynamics that states that entropy always increases: among all the possible histories only those in which entropy increases are selected. Lewis's account is therefore modified adding to the original constraints also the

following criterion for similarity: avoid violation of special science laws . A world like w_2 is a world in which entropy decreases, and therefore, if we take this additional criterion into consideration, w_2 will come out to be less similar to @ than w , saving the Lewisian account.

There are two main parts in the paper: in the first part Dunn argues that thermodynamics is a special science and in the second he provides an account of how the special science laws can fit in Lewis's account of laws.

Dunn argues that thermodynamics is a special science showing that its laws satisfy two features that seem proper of special science: the laws of a special science hold specifically at their own level of inquiry and they appear to be blind to the physics. I think that Dunn's arguments in this regard are convincing to me and I have no problem in granting him that thermodynamics is a special science. I should notice though, as a very minor comment, that Dunn's analysis is not new and not even complete. In fact, while it is true that he mentions Callender (in a footnote there is a reference to a paper of 1997 that is instead absent in the bibliography), he does not mention for example Barry Loewer's position on this². Loewer has in fact already argued that thermodynamics is a special science, considering additional distinctive features of special sciences laws that Dunn does not analyze. According to Loewer, the main relevant differences between fundamental dynamical laws and special science laws are these: The candidates for fundamental physical laws are (i) *global*, (ii) *temporally symmetric*, (iii) *exceptionless*, and (iv) *fundamental (not further implemented)* (v) *make no reference to causation*. In contrast, typical special science laws are (i*) *local*, (ii*) *temporally asymmetric*, (iii*) *multiply realized and implemented*, (iv*) *ceteris paribus*, and (v*) *often specify causal relations and mechanisms*. Thermodynamics is temporally asymmetric, local, and as multiply and heterogeneously realizable as it gets since it applies to gases, liquids, solids, [...], and so on. Moreover, the laws of thermodynamics are *ceteris paribus* because they apply to systems that are energetically isolated (this is the main missing feature in Dunn's discussion). As a consequence, Loewer concludes that thermodynamics is a special science. Be that as it may, I do not see any problem or puzzle here, so let us move on.

Dunn's next step is to show how the special science laws fit in the Lewisian account. The idea is that a special science law is a theorem in the best systemization of the phenomena the special science describes when we limit ourselves to the vocabulary of the special sciences. For example, special science laws in biology are theorems in the best system that is formulated using biological

2 B. Loewer 'Why there is Anything except Physics' manuscript.

terms such as 'cell' or 'organism' . Given that this is the core of the paper (establishing that thermodynamics is a special science is, I think, not that new or controversial) I think that it is necessary to dedicate more time to elaborate on this claim. Instead of doing this, unfortunately, Dunn just has a footnote that give reference to the website of Dr. Markus Schrenk, post doc at the university of Nottingham. And, more unfortunately, the relevant file is not present anymore (or at least, I could not find it). My problem is that Dunn's account seems very surprising, not to say implausible. Many have argued (starting from Philip Kitcher in his 1953 and all that: a Tale of Two Sciences ³) that the scientific practice in sciences that are not physics, like biology for example, are not easy to be seen as proceeding using a deductive method to go from axioms to theorems. Kitcher's arguments in the article quoted are against reductionism in genetics and are based on the failure of classical genetics to even have laws. What I am saying is not that Dunn is necessarily wrong in claiming that special science laws are theorems in the best system when we suitably limit the language. I am just saying that I do not see the argument of why I should think it is the case. Indeed, others have suggested that is not. I think that it would be useful and interesting to dedicate most of our discussion time in trying to understand together how Dunn's claim can be true and how his argument can be improved.

Another puzzle is this: it is unclear to me how to think to the special science laws. What is their status? Are they metaphysically over and above the kinds and laws of physics? Or is their role merely conceptual, epistemological? I think that the first position would be close to the attitude that Jerry Fodor has with respect to special sciences⁴. And it seems to me that it is somewhat odd for a Lewisian to hold such a view: after all, Lewis's Humean supervenience thesis, the core of his account of laws, counterfactuals and causation, is motivated by physicalism, roughly the idea that facts and laws about a special science obtain in virtue of physical facts and laws. It has been argued that Fodor's position is incompatible with physicalism as just defined⁵. So if one should intend the special science laws as metaphysical, then there is a lot to be said more about how is that compatible with physicalism. I think that, in this framework, it is more plausible that the status of the special science laws is epistemological: special science laws are expressed in a vocabulary that is conceptually independent of the vocabulary of physics but the special science laws are completely specifiable by physical laws. The special sciences don't add to the nomological structure [...] but rather they characterize aspects of the structure generated by the fundamental physical laws that are

3 P. Kitcher, 1953 and all that: A Tale of Two Sciences, *Philosophical Review*, 1984

4 J. Fodor, Special sciences *Synthese* 28, 1974

5 For a discussion of this, see B. Loewer Why is There anything except Physics , ms2 in Dunn's bibliography.

especially salient to us and amenable to scientific investigation on languages other than the language of physics⁶. If we intend the special science laws as epistemological, then Dunn's account seems to be connected to (and should be contrasted with) other accounts of counterfactuals discussed in the literature. Just consider the one that both I and Dunn (give he quotes it in his biography) are more familiar with: Loewer's statistical mechanics account⁷. The basic idea of Loewer's original approach is to keep the Lewisian account of counterfactuals together with its criteria of similarity and be careful to take into account all the fundamental laws involved. That is, Elga's examples appears to be a counterexample to Lewis's account only because in evaluating the similarity with @ we forgot to include in our evaluation some relevant fundamental laws. The fundamental relevant laws that Loewer has in mind are called PROB and PH. PROB is a probability distribution over the initial condition of the universe; PH, the past hypothesis, is the assumption that the universe started off in a state of low entropy. Loewer, rephrasing Boltzmann, argues that we need to assume PROB and PH to be among the fundamental laws in order to be able to explain the time-asymmetry of macroscopic phenomena given time-symmetry of the fundamental laws of physics. The reasoning can be understood as follow with the aids of two pictures, figures 2 and 3.

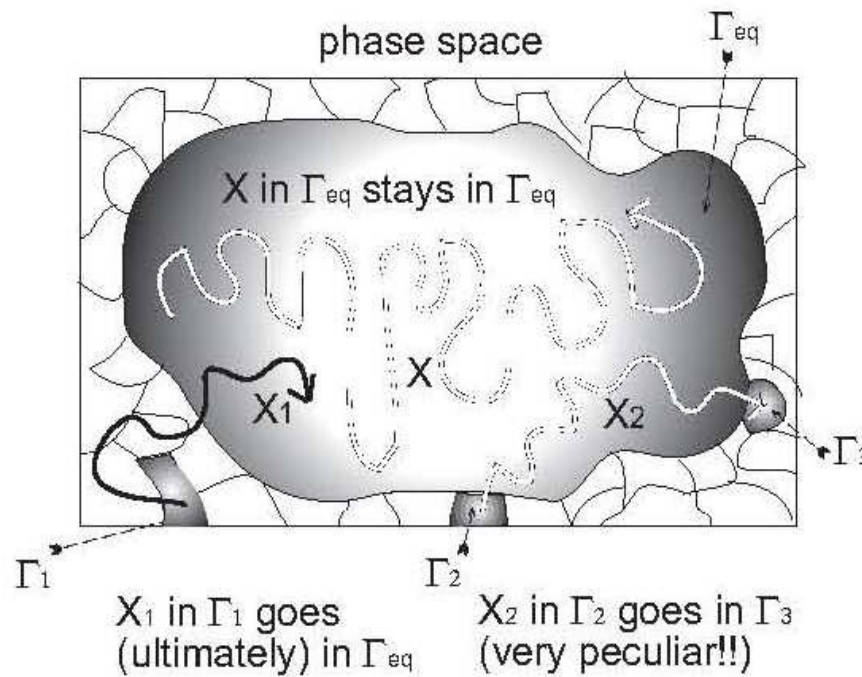


Fig. 2

6 B. Loewer Why is There anything except Physics, ms2 in Dunn's bibliography.

7 B. Loewer counterfactual and the second law ms1 in Dunn's bibliography.

First consider figure 2: the partitions of the space (which is phase space, the space in which the points represents all and only the possible states of the universe) are the macroscopic states (a macrostate is the set of microscopic states that can be macroscopically described by the same thermodynamical variables, like temperature, pressure and volume), their size being proportional to the entropy. Given the probability distribution on the initial condition PROB, we can define exactly what is meant by size of the macrostates. It should be clear from this diagram that it is overwhelmingly likely that a macrostate will evolve towards a macrostate with a larger entropy (given that they are bigger) until it reaches the largest state of all, the equilibrium state. But this is not enough to account for the time-asymmetry of macroscopic phenomena, given that it is also overwhelmingly likely for a state to have evolved from a larger entropy state, as shown in figure 3. Therefore, we also need to postulate PH, namely that the initial state of the universe was one of a very low entropy.

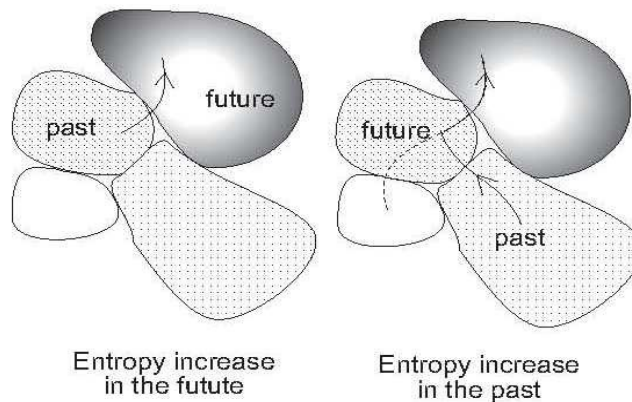


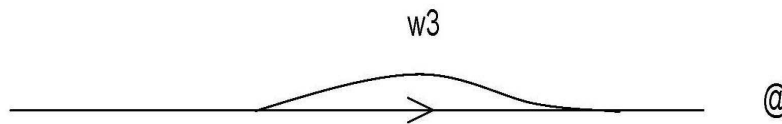
Fig. 3

With the addition of these two fundamental laws, Loewer can “repair” the Lewisian account: w_2 counts as less similar to @ than w because w_2 violates PH and PROB. Dunn, at the end of the paper, has a section in which he lists the advantages of his view. This is odd, since he does not discuss any alternative view to his account: advantages with respect to what? So, given that there are alternative accounts and in particular the one described above seems to share with Dunn's account the epistemological character of the special science laws, a comparison between the two seems to be appropriate. Of course one could deny that Dunn's account the special science laws are epistemological. But that would mean, as already noticed, that they are metaphysical in nature, which would make the account, in my view, more difficult to reconcile with the traditional Lewisian

account.

Be that as it may, let us compare the two account. On the one hand, one advantage that I see of Dunn's proposal over Loewer's original proposal⁸ is that Loewer's account has (arguably) a counterexample, while Dunn's account does not: consider the world w_3 presented in figure 4. It is a world that departs from @ due to a small miracle just like w but that, after A takes place, converges back again to @ with another small miracle. While it is unclear whether the dynamical laws can rule out a world like w_2 , Dunn does not have this problem: w_2 is less similar to w given that it would violate the laws of thermodynamics. But on the other hand, if Loewer's account works, it is an attempt to understand and explain the temporal asymmetry of macroscopic phenomena in terms of the fundamental physical laws. According to Loewer, we can fix the Lewisian account without postulating much in addition but just being careful about the fundamental laws to consider. Dunn's account instead adds to Lewis's criteria of similarity a fourth one that just refers to the existence of special science laws, without any attempt to provide an account of where they come from. One wonder how satisfactory is Dunn's account or how it actually arrives to the heart of the matter.

Fig 4



Here is connected remark. When discussing the advantages of his account, Dunn mentions the following: his account is in accord with Lewis's original suggestion about what goes wrong in Elga's example, an account of special science laws in the Lewisian framework is needed independently, correctly locates what is odd in Elga's example, and it does not stipulate the asymmetry but rather “tells us to take account of any special science laws in the evaluation of similarity”. Leaving aside the first consideration, it seems to me that the fourth one, in light of the discussion of the Loewer's account, is questionable. What “this is not a stipulation” is supposed to mean? To say that there are thermodynamical special science laws without explaining where they come from looks like a stipulation to me. Maybe a clearer discussion is needed here. Also, even if it is true that the account

⁸ It should be noted that Loewer recognizes and solves this problem with a more sophisticated account that considerably departs from Lewis's account in a recent version of *Counterfactual and the second law* .

correctly locates what's odd in Elga's example, it seems to do so by cheating: the intuition is that w_2 is not similar to @ because w_2 is anti-thermodynamical world, and this intuition is “clarified” by an account that claims that thermodynamical laws are to be taken into account? This is like claiming that in order to explain the fact that the sky is blue we should take into account that the sky is blue. Concerning the claim that an account of special science laws is needed in the Lewisian account is needed, I am not really sure that it is the case: after all, if the driving motivation of the typical Lewisian is to ground anything in physics, special science laws are just conceptual laws at best and as such they do not really need a special place in the overall schema. And even if the account is needed, as I said previously, Dunn needs to say more before being able to claim that he has given one.

This interesting discussion made me think about an alternative way to save Lewis's account. In the original Lewisian account we care about the microscopic match and not to violate the fundamental laws but it has the problem of the Elga's world. Loewer's proposal cares about microscopic match and not to violate fundamental laws but, while it does not suffer from the Elga objection, still it is unclear whether it can account for worlds like w_3 . In Dunn's proposal, we care about the microscopic match and not to violate the laws, fundamental or not, and the account does not have the problem with w_2 and w_3 . As a matter of methodology, the approach that I find more promising is the one of Loewer for the reasons that I provided above: the attempt to account for the time-asymmetry of macroscopic phenomena in terms of fundamental physical laws is, if successful, seems to go at the heart of the matter, much more than Dunn's approach. But if it cannot be worked out (and this should be investigated carefully) we remain with Dunn's proposal that, even if unsatisfactory in certain respects, is still a nice account. But now ask yourself a question: if we have to give up the idea of accounting for macroscopic phenomena in terms of microscopic facts and fundamental laws, why should we care about the microscopic match at all? In other words, if Dunn's strategy is the one to be used, then why keep the first three Lewisian constraints as they are and not simply claim that all we should look at in order to evaluate similarity to @ is macroscopic match and small violations of the macroscopic (i.e. thermodynamical) laws? I don't see why we shouldn't. And if so, we end up with an account (pictorially represented in figure 5) that, just like Dunn's account, doesn't have any problem with w_2 or w_3 but it is also simpler.

Valia Allori, Northern Illinois University

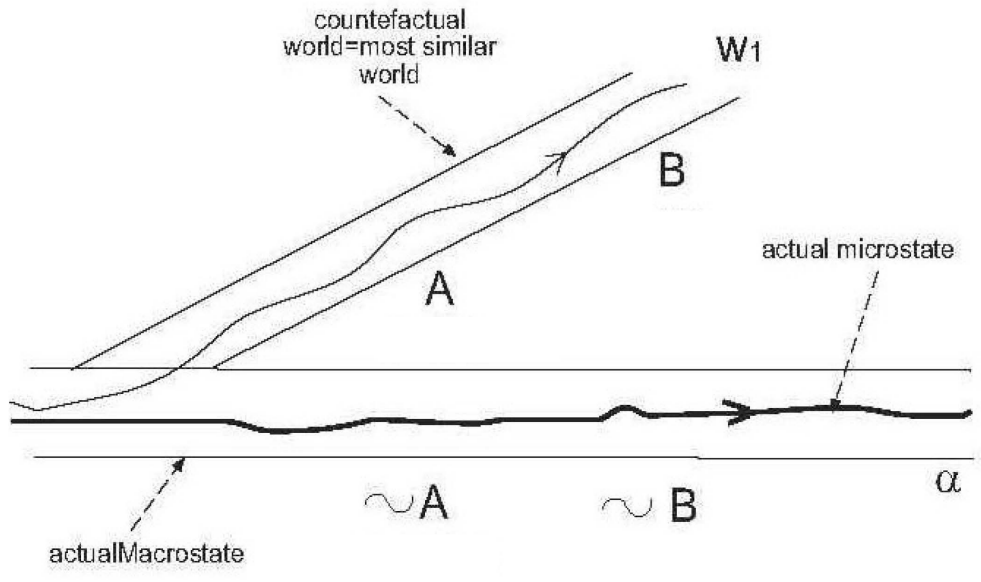


Fig. 5