Evaluation of two entries for the 2012 Hanneke Janssen Prize

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- Pablo Acuña, *Empirical equivalence and underdetermination of theory choice. A philosophical appraisal and a case study* (Utrecht, 2012).
- Martin Jähnert, *Das Korrespondenzprinzip und die Transformation der Mechanik* (Berlin, 2011).

1. Acuña

Acuña's Master's thesis, as its subtitle indicates, consists of a philosophical chapter (Ch. 1, 37 pages) and a hybrid historical/philosophical chapter (Ch. 2, 101 pages). In the former, the author reviews and adds to the philosophical literature on underdetermination. In the latter, he first gives a clear and careful account, based on the existing historical literature, of the (pre-)history of special relativity (Ch. 2, secs. I–IV, 65 pages) and then addresses the question of what makes special relativity preferable to the empirically equivalent ether theory of Lorentz (and Poincaré) (Ch. 2, sec. V, 37 pages). After reviewing and rejecting some of the answers that have been given before in the extensive literature on the subject, he argues for his own answer, drawing on the philosophical analysis in Ch. 1.

As the author acknowledges repeatedly, for the historical part of Ch. 2 (which takes up nearly half the thesis) he relies heavily on my work in this area. In my opinion, however, the many references to my dissertation and several of my papers do not convey the full extent to which he does. This is especially true for the section on the Trouton experiment and $E = mc^2$ on pp. 93–97, as I will show in detail below. This problem can certainly be fixed by adding more references to my work, but that would also make it even clearer than it already is that the historical part of the thesis is totally derivative of the existing historical literature. The thesis, in my opinion, thus has be to judged solely on the philosophical parts, the ideas about underdetermination and their application to the historical case at hand. Unfortunately, in the 'application' part the author once again fails to do justice to previous work in this area, including once again, I regret to say, my own.

Whereas Acuña does cite (copiously) my historical work in this area, he ignores most of my work on the question he is ultimately interested in, viz. what makes special relativity preferable to the theory of Lorentz (and Poincaré). He discusses a few passages in my dissertation and a paper on 'Lorentz vs. Einstein' (Janssen, 2002a)[1] in this context, but ignores three subsequent papers in which I deal with

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[1]Acuña closely follows this paper for his discussion of the alleged *ad-hoc*ness of Lorentz's theory. Compare what he says on pp. 105–106 to what I say on pp. 432–433 of my paper. Footnote 123 ("In this point I follow Janssen 2002[a], 431–6") does not do justice to the extent to which the preceding two pages rely on my discussion of the same topic.

this question: (Balashov and Janssen, 2003), (Janssen, 2002b, COI Story III, pp. 497–507), and (Janssen, 2009). The third of these is a detailed response to Brown (2005) and Brown and Pooley (2006), which Acuña does cite prominently (and my paper with Balashov, in turn, figures prominently in this work of Brown and Pooley). Acuña also ignores an important paper by Norton (2008) criticizing Brown (2005). One could argue that Acuña is writing about Einstein vs. Lorentz and not about Janssen and Norton vs. Brown and Pooley, but I have argued[2] that these issues are closely related. Acuña, of course, may disagree even with this basic assessment, but in that case he should at least say so.

I have not scrutinized Acuña's arguments in the first purely philosophical chapter of his thesis, but the pattern I see in the philosophical part of the second chapter is not encouraging. A good philosophy essay anticipates and preempts the strongest arguments for the positions it attacks and against the positions it defends. In the key section of his Master's thesis (Ch. 2, sec. V), "On the reasons to choose," Acuña comes up short on both counts: he is too quick both to dismiss some reasons as "bad reasons" (in particular, in my opinion, Minkowski space-time and explanatory power [sec. V1.a]) and accept others as "good reasons" (the ether [sec. V2a], general relativity [sec. V2c]). As I will show with some concrete examples below, Acuña could have avoided some of these problems had he dealt more comprehensively with my own work.

Let me give the concrete examples I promised. I begin with the purely historical part and the example of the Trouton experiment and $E = mc^2$. Acuña discusses this on pp. 93–97, referring to my paper on the topic (Janssen, 2003) five times, twice for figures and twice for Lorentz quotes taken from it (in one case [note 102] misidentifying the relevant Lorentz paper) and once for discussion of the related but in this context irrelevant Trouton-Noble experiment. This does not begin to capture the extent to which Acuña's discussion is derivative of mine. I'll put a few key passages from the two discussions side-by-side to substantiate this charge.

Here is how Acuña introduces the Trouton experiment:

> In 1900 Frederick Trouton, based upon an original idea by George F. Fitzgerald, designed an ingenious experiment in order to look for an ether-wind effect. The interesting feature of this particular experiment was that, unlike many of the attempts to detect an effect of the motion of the Earth across the ether, it was not based on optics: according to Fitzgerald, if a capacitor in motion through the ether is charged or discharged it should suffer an impulse. Trouton's experiment was designed to measure this effect. In a terrestrial laboratory which, of course, moves with respect to the ether a hanging capacitor at rest is connected to a battery. When the battery is switched

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[2]"My work has focused on a comparison between the theories Lorentz and Einstein actually proposed. This comparison is nonetheless relevant to the evaluation of Brown's proposal" (Janssen, 2009, p. 27).

on an electromagnetic field is produced between the plates of the capacitor. If it were at rest in the ether, only an electric field would be induced, but its motion through the ether adds the generation of a magnetic field. Fitzgerald reasoned that the extra energy needed to produce the magnetic field had to come from the kinetic energy of the capacitor, and the loss of kinetic energy must result in a jolt in the direction opposite to the motion across the ether (Acuña, p. 93).

Compare this to the opening paragraph of my paper and to the beginning of sec. 2 of my paper, both of which definitely should have been cited here:

> In the Fall of 1900, Frederick T. Trouton started work on an ingenious experiment in his laboratory at Trinity College in Dublin. The purpose of the experiment was to detect the earths presumed motion through the ether, the 19th-century medium thought to carry light waves and electric and magnetic fields. The experiment was unusual in that, unlike most of these so-called ether drift experiments, it was not an experiment in optics. Trouton tried to detect ether drift by charging and discharging a capacitor in a torsion pendulum at its resonance frequency, which he hoped would set the system oscillating. The basic idea behind the experiment came from George Francis FitzGerald, whose assistant Trouton was at the time. According to FitzGerald, a capacitor moving through the ether should experience an impulse, a jolt, upon being charged or discharged. ... Figure 2 [the figure reproduced on p. 93 by Acuña] illustrates the basic idea behind the Trouton experiment. A battery is used to charge a capacitor. If the power is switched on, an electromagnetic field is produced largely confined to the volume between the plates of the capacitor. If the system is at rest in the ether, the charges will only produce an electric field; if the system is moving, the charges will also produce a magnetic field ... FitzGerald thought that the energy for the magnetic field would come from the capacitor's kinetic energy. Elementary Newtonian mechanics tells us that in that case a moving capacitor upon being charged should experience a jolt in the direction opposite to its direction of motion (Janssen, 2003, p. 27, pp. 31–32).

More seriously, Acuña uses, without attribution, key elements of my analysis of the experiment, most importantly the basic dilemma posed by the experiment and its resolution through $E = mc^2$. Here is how Acuña introduces the dilemma:

> If the outcome of the Trouton experiment is positive, the jolt of the capacitor constitutes a clear violation of the center of mass theorem; but if the result is negative, then the law of conservation of

momentum is violated. The dilemma consists in that it seems that a theoretical explanation of the experiment necessarily implies the abandonment of one of these two core tenets of classical physics (Acuña, pp. 93–94).

. This is how I put it:

If we put Larmors and Lorentzs accounts of the Trouton experiment side by side, we arrive at the following dilemma. If the effect predicted by FitzGerald does occur, the center-of-mass theorem is violated (as is the relativity principle, one may add). That is what Larmor tells us. If, however, the effect does *not* occur, momentum conservation appears to be violated. That is what Lorentz tells us. It seems that we have to choose between the center-of-mass theorem and momentum conservation, two laws that are essentially equivalent in Newtonian mechanics (Janssen, 2003, p. 29).

This is how Acuña explains that $E = mc^2$ provides the way out of the dilemma:

[The] explanation can be carried out only by considering $E = mc^2$. If energy has mass, a transfer of energy from the battery to the capacitor is also a transfer of mass from the former to the latter; and in a frame in which they are both moving, a transfer of momentum is involved as well. Therefore, by charging the capacitor, it gains an amount energy, mass and momentum, while the battery loses the same amount of these quantities. Total momentum is then conserved. However, if energy has mass, the momentum circulation within the system does not imply a change in the velocity of its parts, for the increase of the capacitor momentum is given by a mass-gaining, not by a change in its velocity. On the other hand, and from the point of view of Fitzgerald's interpretation of the experiment that the extra energy needed to produce the magnetic field came from the kinetic energy of the capacitor, not from the battery, the kinetic energy lost by the capacitor and which is taken away by the magnetic field is, indeed, accompanied by a loss of momentum of the capacitor which is compensated by the electromagnetic momentum of the field just as Lorentz said. Nevertheless, the loss of momentum of the capacitor means that it loses mass, not velocity and the center of mass theorem still holds (Acuña, p. 95).

This is how I put it:[3]

The dilemma that we arrived at in the preceding section is easily resolved once we realize that energy has mass. Qualitatively, the argument runs as follows. If energy has mass, a transfer of energy

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[3]Similar statements can be found in my dissertation and in the appendix of (Janssen, 2002a), two sources also cited in Acuña's bibliography.

> from the battery to the capacitor means a transfer of mass, and, in
> a frame of reference in which battery and capacitor are moving, a
> transfer of momentum. ...When the moving capacitor is charged,
> it gains a certain amount of energy, mass, and momentum, while
> the moving battery loses that same amount of energy, mass, and
> momentum. The total amount of momentum is conserved. Contrary
> to what Lorentz thought in 1904, this does not require the capacitor
> to change its velocity. The increase in the capacitor's momentum
> corresponds to a change in the capacitor's mass, not to a change in its
> velocity. Hence, there is no violation of the center-of-mass theorem.
> Once the inertia of energy is taken into account, a strictly negative
> result of the Trouton experiment is thus seen to be compatible both
> with momentum conservation and with the center-of-mass theorem
> (Janssen, 2003, p. 38).

Another element that Acuña borrows without attribution (on pp. 96–97) is the connection between the Trouton experiment and what is now often called the 'photon box' thought experiment with which Einstein proved $E = mc^2$ in 1906.[4] The most convincing consideration I can think of that this is a non-trivial connection, for which Acuña should have provided a reference, is that I explicitly acknowledge Stachel for having pointed it out to me (Janssen, 2003, p. 49). In fact, this was the main reason I published this paper not in a refereed journal but in a *Festschrift* for Stachel.[5]

I turn to the philosophical part and to examples of Acuña failing to anticipate and preempt objections to the position he is defending and reasonable come-backs to objections he raises against positions he is attacking. I start with the (lesser) former problem. In arguing that the retention of the ether was the real problem for Lorentz's theory, Acuña ignores two *prima facie* objections, one historical, one philosophical, both mentioned in my dissertation. The relevant section (sec. 3.5.7, entitled "Lorentz's arguments for preferring his own theory over Einstein's") starts by quoting Lorentz saying that "[t]he aether has been introduced in order that it may serve as a substratum." I then point out:

> This is a very strong and time-honored argument. As we will be-
> low, even Einstein would eventually come to accept it [in *Ether and
> relativity*, his inaugural lecture in Leyden in 1920]. A particularly
> clear version of the argument ...is given by Maxwell in his article
> Ether for the ninth edition of the *Encyclopaedia Britannica* of 1875.

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[4]Nowadays, one typically uses it to derive, *assuming $E = mc^2$*, that a photon with energy $h\nu$ must have momentum $h\nu/c$.

[5]In the acknowledgments, I write: "I am greatly indebted to John Stachel for his perceptive comments ...(cf. notes 3 and 5)" (Janssen, 2003, p. 49). Note 3 reads: "John Stachel first drew my attention to the connection between the Trouton experiment and this [1906] paper by Einstein" (ibid.).

After quoting Maxwell, I continue: "Variants of Lorentz's and Maxwell's argument are routinely rehearsed by modern philosophers of space and time. It is perhaps the single most important argument in favor of manifold substantivalism." I then quote from Earman's *World enough and space-time*:

> When relativity theory banished the ether, the space-time manifold M began to function as a kind of dematerialized ether needed to support the fields. In the nineteenth century the electromagnetic field was construed as the state of a material medium, the luminiferous ether; in postrelativity theory it seems that the electromagnetic field, and indeed all physical fields, must be construed as states of M. In a modern, pure field-theoretic physics, M functions as the basic substance, that is, the basic object of predication (Earman, 1989, p. 155).

This then is the philosophical consideration that would seem to call for some comment. The historical one is the famous quip by Ehrenfest that I quote in my dissertation and paraphrase in my 'Einstein vs. Lorentz' paper (Janssen, 2002a, p. 431). "We are asked to subscribe to the following three articles," Ehrenfest wrote. The first two are the postulates of special relativity. To which Ehrenfest added a third: "We declare that the combination of these two assertions satisfies us" (Ehrenfest, 1913, p. 19). In other words, Lorentz was not alone in feeling the need for some material carrier for electromagnetic fields.

In presenting his third "good reason" for preferring special relativity over Lorentz's theory, the support special relativity receives from general relativity, Acuña ignores two considerations I gave in my 'Einstein vs. Lorentz' paper:

> As Lakatos and Zahar concede, greater heuristic power must translate at some point into empirical successes not matched by the competition. Since Lorentz's mature theory and special relativity are empirically equivalent, Zahar has to turn to general relativity to find such empirical successes. This means that he has to assume that there is sufficient continuity between special and general relativity to consider them as belonging to one research program with a core theoretical commitment and a set of strategies for the development of the theory that stayed more or less fixed. As Zahar realizes, this assumption is problematic. There is an even more serious problem. The acceptance of special relativity by the physics community in the early decades of this century had nothing to do with general relativity. Consequently, an account of what makes special relativity preferable to Lorentz's theory that depends on virtues of general relativity cannot shed any light on the rationality of the choices that were made. But the point of Zahar's study was precisely to capture that rationality (Janssen, 2002a, p. 438)

In addition, Lorentz, of course, thought his ether theory was as compatible with general relativity as Einstein's special relativity (Janssen, 1992, pp. 345–347).[6]

As an aside, I note that Acuña is hardly the first to point out that quantum theory played an important role in deciding between Lorentz and Einstein (Acuña's second "good reason"). I have routinely made the same point in my papers—without dwelling on it because it is rather obvious—and I'm sure others have too: "Special relativity overthrew Lorentz's interpretation of Lorentz invariance, but it did not replace—nor was it ever intended to replace—Lorentz's theory with a theory of comparable scope. What eventually superseded Lorentz's theory was quantum theory, not relativity theory" (Janssen, 2002a, p. 431); "Variants of the COI [Common Origin Inference] laid out above, *in conjunction with arguments from nascent quantum theory*, removed Lorentz's theory from serious consideration" (Janssen, 2002b, p. 499; emphasis added); "Lorentz's theory was a comprehensive constructive theory of matter and fields in a Newtonian spacetime. Special relativity did not replace that theory with one of comparable scope, nor was it ever intended to. Quantum mechanics eventually would. Special relativity only eliminated certain parts of Lorentzs theory, notably the ether and remnants of Newtonian theory" (Janssen, 2009, p. 39).

I turn to the more serious problem: Acuña's failure to anticipate and preempt plausible come-backs to objections against positions he is attacking. This problem (to be more careful: instances of it that immediately jump out at me) largely result from him ignoring the three papers of mine that I mentioned above (Balashov and Janssen, 2003; Janssen, 2002b, 2009). The key notions here are: explanatory power, principle theories vs. constructive theories, Minkowski space-time. These notions play a central role both in Acuña's thesis and in my debate with Brown. I will briefly indicate where I think Acuña is a few steps behind of where the debate currently is.

On explanation, he simply follows Van Fraassen's pragmatic position (see p. 2, p. 117). This is a widely held position, but I have explicitly argued against it in the context of the 'Lorentz vs. Einstein' case. It is one of the central points of my COI paper:

> I take issue with the widely held view among philosophers of science that the explanatory power of a theory does not count as evidence for that theory, i.e., that explanatory power should not affect a scientists decision to accept or reject a theory ...Philosophers, like van Fraassen, who endorse the idea that explanatory power does not have epistemic but only pragmatic value typically assign epistemic value exclusively to empirical adequacy. In scientific practice,

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[6]Acuña does not cite this paper but he does cite Kox (1988) from which he could have learned the same thing.

however, explanatory power and empirical adequacy both have epistemic value, and the latter does not automatically trump the former (Janssen, 2002b, p. 458, pp. 462–463).

I elaborate on how I see the role of explanation in sec. 1.1 of my response to Brown (2005):

> In his response to Brown (2005), John Norton [2008] sidesteps "the explanatory issues that have dominated discussion elsewhere [since] they seem only to lead to futile disputes over just what it means to explain" [a sentiment shared by Acuña]. Explanation is a notoriously tricky subject in philosophy of science, so why not follow Norton's lead and re-stage the debate in a different venue? Unfortunately, explanation is tied up with inference, which is absolutely central to the scientific enterprise ... Explanations are answers to why-questions. Physicists seek answers to such questions in part no doubt for the sake of those answers themselves, but mostly to find clues and pointers in them for further research. In other words, scientists rely on explanations for guidance in their work ... As the case study in sec. 3 of this paper [on the velocity dependence of electron mass] will illustrate most clearly, the seemingly arcane explanatory issue that Brown and I are arguing over can actually make a difference in scientific practice (Janssen, 2009, pp. 26–27).

On Acuña's use of Einstein's famous distinction between principle theories and constructive theories (p. 2, pp. 115–116), I will note only that he is conflating this distinction and the one between kinematics and dynamics in the same way that Brown (2005) is, whom Acuña follows on this point. This conflation is addressed at length in my response to Brown (Janssen, 2009).[7] Again, my complaint is not that Acuña disagrees with me (so does Brown), but that he does not respond to challenges brought to the position of Brown that he is endorsing.

On Minkowski space-time, Acuña also follows Brown (2005) and Brown and Pooley (2006). Acuña (p. 115) quotes the following passage from my dissertation:

> The reason for calling this a 'common cause'-type argument rather than a common cause argument, is that Minkowski space-time does not seem to be a common cause in quite the same sense that a shrimp cocktail contaminated with the salmonella bacteria is the common cause of the sudden death of half the population of a cheap Dutch old folks home.

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[7]As I argued there: "The principle-constructive distinction is a red herring in the end. By focusing on it, Lorentz missed a much more important difference between his own theory and Einstein's, namely the difference between kinematical and dynamical accounts of a whole class of phenomena" (Janssen, 2009, p. 38).

> Although the status of the 'common cause' obviously needs further
> philosophical clarification, it is safe to say, I think, that this is a very
> strong argument for preferring special relativity over an empirically
> equivalent classical ether theory

He then comments:

> Unlike Janssen, I think that the need for philosophical clarification of
> the status of the common cause invoked makes it *unsafe* to use it as
> an argument to decide between the theories. At first sight, it seems
> that a *substantivalist* position is taken with respect to space-time.
> As most of the philosophical debates, the one about the ultimate
> ontology of space-time is open, and to offer a criterion for theory
> choice which is based on a specific position in the context of an
> open philosophical debate is, I think, quite risky. Actually, and in a
> *relationist* spirit, one could say that there is no way in which space-
> time can be a cause, and that it is the Lorentz-invariance of physical
> laws what explains the metric of space-time rather than the other
> way around (Acuña, p. 115).

This is fair criticism of these remarks in my dissertation. But I have addressed
these objections in subsequent work, most carefully and explicitly in my response
to Brown (2005). First, on the issue that I would be committed to a particular
ontology of space-time:

> Special relativity *as a physical theory* is agnostic about the ontology
> of space-time. I want to argue that the orthodox version of this
> physical theory is preferable to the alternative proposed by Brown
> because it provides better guidance for further research. Given that
> my argument is ultimately about such methodological issues and not
> about ontology, it had better be independent of whether one is a re-
> lationist or a substantivalist about Minkowski space-time (Balashov
> and Janssen, 2003, p. 341, note 11).[8] The challenge in this case is to
> produce an argument that works for the relationist. The substan-
> tivalist can always make that same argument work by reifying the
> relevant relations. I can thus join the debate with Brown on his own
> relationist turf without compromising the focus on methodological
> issues (Janssen, 2009, p. 28).

Second, on the issue of how Minkowski space-time can explain Lorentz invariance
without it being a cause or a substance:

> Brown (2005) writes: "The real issue is . . . whether physical geome-
> try . . . when it is absolute and immune to perturbation as in Newto-
> nian and Minkowskian space-time . . . offers a *causal* explanation of
> anything" (p. 26, my emphasis). I claim that Minkowski space-time

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[8]See also (Janssen, 2002b, p. 468).

> explains Lorentz invariance. For this to be a causal explanation,
> Minkowski space-time would have to be a substance with causal ef-
> ficacy. Like Brown, I reject this view (Janssen, 2002b, p. 468). As
> I hope to make clear, Minkowski space-time explains by identify-
> ing the *kinematical nature* (rather than the cause) of the relevant
> phenomena (Janssen, 2009, p. 28).[9]

Again, Acuña may well disagree with me. But then it is incumbent upon him to say so *and*, given the weight of the issue in his own argument, to explain (no pun intended) why.

In view of the problems above, I don't think Acuña's thesis, though clearly a perfectly competent piece of work by a promising student, is prize-worthy (*unless* judged so purely on the strength of the philosophical first chapter).

2. Jähnert

In his Master's thesis, Jähnert develops nothing less than a new account of the transition from the old quantum theory to matrix mechanics. The slogan guiding the effort and characterizing the result is "transformation through application." As he puts it in the conclusion of his thesis:

> Was die Geschichte des Korrespondenzprinzips aufzeigt, ist, dass
> die Arbeit innerhalb einer Theorie, *obwohl dabei der begrifflichen
> Rahmen niemals verlassen oder radikal verändert wird*, die Theorie
> selbst verändert und so eine Transformation einer Theorie in eine
> andere ermöglicht (p. 94, my emphasis).

The italicized clause may be too strong (I would argue, for instance, that the transition from Bohr's original model of the hydrogen atom to the virtual oscillators of the Bohr-Kramers-Slater (BKS) theory *is* a radical change), but the basic *Gestalt* strikes me as spot-on. As the author acknowledges (p. 4, note 1), he is taking the concept of "transformation through application" from Joas, Lehner, and Renn, the leaders of an international project in the history of quantum physics that has its epicenter in Renn's *Abteilung* I of the *Max-Planck-Institut für Wissenschafts-geschichte* in Berlin. Jähnert is a junior member of this core Berlin group. I am a member of the larger international group associated with this project. In the context of this project (as we dutifully acknowledge in our publications), physicist Tony Duncan and I have written a number of papers on the history of quantum mechanics, focusing on matrix mechanics and avoiding wave mechanics so as not to duplicate the efforts of Joas, Lehner, and Renn working on wave mechanics. The first of these papers, "On the verge of *Umdeutung*" (Duncan and Janssen, 2007), like Jähnert's Master's thesis, is on the transition from the old quantum theory to matrix mechanics.

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[9]I elaborate on these points in the conclusion of the paper (Janssen, 2009, p. 49).

I want to make a few comments on the concept of "transformation through application" and Jähnert's use of it. The concept is very much in the spirit of the approach of Renn and his colaborators, an approach known as "historical epistemology". In several talks,[10] Joas (together with Jeremiah James) has argued that the development of solid-state physics should not be seen as an *application* of quantum mechanics, understood as a well-established definitive theory, to solids and other many-body systems, but that quantum mechanics itself changed in the process of being used in attempts to analyze such systems. Lehner has likewise emphasized the importance of seeing quantum field theory as a further development of quantum mechanics rather than as an application of it. Jähnert, to my knowledge, is the first to apply the "transformation through application" concept to the old quantum theory. This application (no pun intended) is not nearly as original or provocative as those of Joas and Lehner. After all, the old quantum theory was never really seen, neither by its practitioners, nor by historians, as a well-established definitive theory in the sense that the quantum mechanics of the mid- to late 1920s was. It was clear to almost everyone involved and almost all later commentators that it remained, at best, a work in progress. This then is a first indicator that Jähnert's new account is not quite as new and revolutionary as he makes it sound. Jähnert contrasts his account with (his version of) a Kuhnian account. As he writes in his introduction, "Damit verabschiedet sich diese Arbeit von einer Kuhnschen Perspektive" (p. 3). And in the conclusion, he reiterates: "Die Wissenschaftstheorie der Kuhn'schen Tradition, die Theorien als etwas verstand, das einen absoluten und unverrückbaren Rahmen für jedwedes Problemlösen festsetzt, hat keinen Platz für einen Prozess der transformation through application" (p. 93). Kuhn, as I read him, introduced the concept of a paradigm, in the sense of what he later called an 'exemplar', precisely to get away from the idea of rigid formal theories guiding research,[11] but Jähnert, of course, is hardly the first and won't be the last to use a cardboard version of Kuhn as a convenient straw man.

At the end of the day, however, as I will explain in somewhat greater detail below, Jähnert's new account is really not fundamentally different from the one given by Duncan and myself in "On the verge of *Umdeutung*." And our account was not a radically new account of this episode, nor was it meant to be. Instead, using work by Van Vleck as our guide, we tried to amplify, clarify, and, in a few important places, correct the received view of how matrix mechanics grew out of dispersion theory. Jähnert cites our paper a few times[12] but not as providing a

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[10]As Jähnert points out (p. 4, note 1), this work has not been published yet.

[11]Think, for instance, of his famous analogy with how we learn to identify water foul (Kuhn, 1977, pp. 473–482).

[12]He criticizes a few relatively minor points in the paper. I think these criticisms are largely based on misreadings of our paper, but since the prize-worthiness of his thesis certainly does not depend on *those* points I will not argue them here.

source (resulting from the same larger research effort that his work is part of!) for an account along the same lines as his own alternative to his "Kuhnian" account.

Given what he ended up doing in this thesis, Jähnert should definitely have included "On the verge of *Umdeutung*" in his overview of the existing literature in sec. 1.1 (*Forschungsstand*). In this section, Jähnert focuses on the literature on the correspondence principle, which he characterizes very nicely indeed.[13] An important weakness of "On the verge of *Umdeutung*" (first pointed out by Rynasiewicz) is that it does not do justice to the development of the correspondence principle between 1913 and the early 1920s. Duncan and I focus on the way correspondence-principle techniques were used in 1924–1925 by Van Vleck, Kramers, Born, Heisenberg, and others and do not discuss or even mention earlier and different versions of the correspondence principle. Jähnert is much more careful on this score. For instance, he is right to point out—following Bokulich (2008) rather than Duncan and Janssen (2007)—that Bohr and other practitioners did not see the correspondence principle as limited to the regime of high quantum numbers. He also recognizes the limits of Bokulich's position, which is based largely on the exegesis of Bohr's writings, and emphasizes the importance of looking at how the principle was used in actual practice.[14] Now it is precisely the use of correspondence-principle arguments in actual practice in precisely the period that Jähnert is most interested in, the period leading up to *Umdeutung*, that is central to "On the verge of *Umdeutung*". So, Jähnert, in situating his work in the existing literature, seems to have mistaken an epicycle (admittedly the largest one) of his overall project (the historical debate over the correspondence principle) with its deferent (the debate over the transition from the old quantum theory to matrix mechanics).

Since Jähnert is ultimately interested in the latter rather than the former debate, he can be forgiven, I think, for not citing two key sources for the debate over the correspondence principle: Bokulich's 2008 book (he only cites a 2009 preprint) and an important paper by Fedak and Prentis (2002) on which Bokulich relies for the technical details of her argument. Personally, I think Jähnert would have written a stronger thesis, had he conceived of it more as a contribution to the literature on the correspondence principle. He has some great material for that: the notion of

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[13]I particularly like his characterization of the differences between Darrigol, Tanona, and Bokulich: "Zwischen diesen drei Autoren bestehen jedoch große Unterschiede in der Frage, ob das Prinzip ein Satz über die Analogie von Quantentheorie und klassischer Elektrodynamik (Darrigol ...), über die Relation von empirisch beobachtbaren Spektren (Tanona ...) oder eine Gesetzmäßigkeit der Quantentheorie (Bokulich ...) darstellt" (p. 11, note 9).

[14]He writes: "Mit wenigen Ausnahmen wurde die Analyse des Korrespondenzprinzips von der hermeneutischen Frage geleitet, was Bohr mit dem Korrespondenzprinzip meinte ... Anstelle der Frage, was haben Physiker gemeint, wenn sie über das Korrespondenzprinzip reflektierten oder es in ihren Argumenten benutzten, wird meine Analyse sich auf die Frage stützen, wie Physiker das Korrespondenzprinzip innerhalb physikalischer Argumente anwandten, um es produktiv werden zu lassen" (p. 12).

intermediate orbits (passim); work on intensities by Stern and Voelmer (p. 49 ff.); early Kramers–Reiche correspondence about dispersion (p. 57 ff.); Frank Hoyt's work on the correspondence principle (p. 65 ff.). None of this material, as far as I know, has been dealt with before in the historical literature. As it stands, however, his discussion of this material is subordinated to the overall goal of providing a new account of the emergence of matrix mechanics. The thesis, I think, therefore has be judged on the merits of that new account and Jähnert's arguments for it.

Before I give my assessment of the thesis on these terms, I need to draw attention to another problem with it. Jähnert's command of the mathematics of his subject leaves much to be desired. One could argue that this is largely because he is tackling an extremely demanding subject, which I certainly would not feel comfortable writing about without the help of a professional physicist. However, with the help of such a professional physicist, I have written a paper in which much of the technical apparatus needed to understand the source material in this area is laid out in great detail. In fact, this was one of the explicit goals of "On the verge of *Umdeutung*" (and one of the reasons we had trouble getting it published!).[15] Jähnert, for reasons that, frankly, I don't understand, chose not to avail himself of our assistance. Especially since that assistance was directed and tailored to the very group of researchers that he is a member of, I'd say he did so at his own peril. And, unfortunately, it is clear at various points in his thesis that he made the wrong choice.

There is telling sentence on p. 66 revealing that he completely misunderstands how the derivation of the Kramers dispersion formula works, a derivation that is just as central to his account as it is to the one in "On the verge of *Umdeutung*." He writes:

> In seinen Arbeiten zur Dispersionstheorie leitete Kramers aus der Theorie mehrfachperiodischer Systeme zunächst eine klassische Dispersionsformel für den "Ersatzoszillator" her, im Anschluss übersetzte er das Ergebnis der klassischen Überlegungen in eine quantentheoretische Version.

This is nonsense. *Ersatz* or virtual oscillators only come into play *after* the classical formula for the dispersion *by some classical multiply-periodic system* has been derived. The notion of a virtual oscillator provides a representation of sorts of

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[15] "[W]e give an elementary and self-contained presentation, drawing on [papers by Van Vleck] of the technical results on which our narrative . . . rests. In particular, we use canonical perturbation theory in action-angle variables to derive a classical formula for the dispersion of radiation by a charged harmonic oscillator and apply the correspondence principle to that formula to obtain the Kramers dispersion formula for this special case. This fills an important pedagogical gap in the historical literature. Given the central importance of the Kramers dispersion formula for the development of quantum mechanics, it is to be lamented that there is no explicit easy-to-follow derivation of this result in the extensive literature on the subject . . . [we also give] the extension . . . to arbitrary non-degenerate multiply-periodic systems" (Duncan and Janssen, 2007, p. 560).

what happens in dispersion according to the old quantum theory, but the classical theory of multiply periodic systems is never applied to virtual oscillators.

This is not just an isolated slip. Once the reader starts paying closer attention, it will become clear to him or her pretty quickly that the author is not always on top of the relevant mathematics. One tip-off is that he has a tendency not to explain what the symbols in his equations stand for. And there are other give-aways. On p. 75, for instance, he writes: "Auf der Grundlage der Hamilton-Jacobi-Theorie ließ sich die Frequenz einfach durch $\nu_k = \partial H_0/\partial J_k$ ersetzen." This relation has nothing to do with Hamilton-Jacobi theory. It is just half of Hamilton's equations, $\dot{q}_k = \partial H/\partial p_k$, for special coordinates q_k and conjugate momenta p_k for a given Hamiltonian H that are known as action-angle variables and that are typically denoted by J_k and $w_k = \nu_k t$, respectively.

I turn to Jähnert's 'transformation through application' account of the transition from the old quantum theory to matrix mechanics. He introduces two new notions in developing this account, the 'statistical-mechanical hybrid' (p. 7) and 'the state in between', the latter instantiated initially (in Kramers' dissertation) as intermediate orbits (*Zwischenbahnen*) and later (in the dispersion theory of Ladenburg, Reiche, and Kramers as well as in BKS) as virtual oscillators (p. 39). Both notions are presented in the language of Minsky's 'mental models' (p. 6[16]).[17]

The "statistical-mechanical hybrid" is best seen, I think, as a new label for the way in which a statistical element starts to rear its head in the old quantum theory through Einstein's A and B coefficients (and its connection to Fourier amplitudes in the work of Kramers and others [including, importantly, Van Vleck]) and BKS. Labels are important and this one is certainly interesting. However, it does not translate into any new insights in the transition under consideration that aren't contained already in "On the verge of *Umdeutung*" and the sources it built on.

The truly new element is the "the state in between" and the connection made through it between intermediate orbits and virtual oscillators. Unfortunately, this connection is also the least convincing element of the thesis. Statements of the connection abound, almost to the point where the repetition makes it easy to forget that no textual evidence is adduced for it. A selection:

- "Dabei gelangte er zu einer neuen Dispersionstheorie, die maßgeblich auf einem neuem Modell des "state in between" basierte. Dieses Modell war nicht mehr die "Zwischenbahn" der Kramer'schen Dissertation, statt dessen operierte Kramers mit einem "Ersatzoszillator", den Ladenburg und Reiche zuvor als erweiterte Illustration des statistischen Ansatzes entwickelten" (p. 57)

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[16]Mental models have been used extensively by Renn, Jähnert's advisor. See, for instance, the paper by Renn and Sauer (2007), parts of which were presented at HGR6 in 2002 in Amsterdam.

[17]There are brief references to other historiographical machinery, such as Galison's 'trading zones' (p. 6), 'Actor-network theory' (p. 7) and Foucault (p. 12), but that equipment is not used in the actual analysis of the thesis.

- "In diesem Punkt sind die "virtuellen Oszillatoren" demnach in nichts von den "Zwischenbahnen" verschieden und stellen eine weitere Form des "state in between" dar." (p. 63)
- "Der statistisch-mechanische Hybrid und der "state in between" wurden nun nicht mehr durch das Modell der "Zwischenbahn" beschrieben, sondern fanden ihren Ausdruck in "virtuellen Oszillatoren"." (p. 64)
- "Die virtuellen Oszillatoren als Terminologie, wie sie Kramers beschreibt, werden dabei von ihm nicht als neuer "state in between" erkannt, argumentativ sind sie es dennoch." (p. 69)
- "Wie Kramers verband Born dazu die Strahlungsfrequenz eines Quantenübergangs mit der klassischen Strahlungsfrequenz des "state in between", der wie gesehen die Form eines virtuellen Oszillators angenommen hatte ... Während jedoch Kramers aus diesem Argument mit der Zwischenbahn ein neues Modell entwickelt hatte, um Fourier-Koeffizienten zu bestimmen, konnte Born auf den neuen virtuellen Oszillator als "state in between" aufbauen" (p. 74)

Jähnert certainly deserves credit for having drawn attention to the notion of 'intermediate orbits'. Van Vleck, the main protagonist of "On the verge of *Umdeutung*," also talked about intermediate orbits. Yet in our paper, Duncan and I essentially drew Mark Twain's famous curtain of charity over the relevant scenes. Jähnert convinced me (in a talk I saw him give on his work in July 2011 and again in this thesis) that this was a mistake on our part.

As the phrase suggests, intermediate orbits are orbits in between those allowed by the old quantum theory. They can be used to make one of the old quantum theory's most radical departures from classical theory more palatable: the severing of the relation between orbital frequencies and the frequencies of radiation emitted upon transitions between orbits. Such transition frequencies, though different from the mechanical frequencies of both the initial and the final orbit, can always be written as a weighted average over the mechanical frequencies of the intermediate orbits. Related to this idea, as Jähnert indicates (and is technically done more cleanly in the paper by Fedak and Prentis (2002) mentioned above, which was then used by Bokulich (2008)), are attempts to connect Fourier components of orbital motion to radiation frequencies.[18]

However, and this is the biggest problem I have with his Master's thesis, Jähnert does not provide a good argument for a genetic link between intermediate orbits and virtual oscillators. I can see that, conceptually there is a connection between the two. Trivially, both are defined with respect to a pair of orbits or states. More interestingly, as Jähnert points out (if I understand him correctly), the ideas involving intermediate orbits, Fourier components, and radiation that crystallize

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[18]Coming to think of it, the real problem with Jähnert's account may be the connection between intermediate orbits and Fourier components. The connection between Fourier components and virtual oscillators sounds like a much more promising starting point to me!

in Kramers' dissertation describe essentially the same complex of phenomena as the *Ersatz* or virtual oscillators in the dispersion theory of Ladenburg, Reiche, and Kramers and in BKS, namely the totality of radiation emitted from an atom at a whole range of frequencies. However, Jähnert cannot point to any textual evidence for a genetic link between the two concepts, i.e., he cannot point to a single physicist of the time explicitly making the connection between the two.

In a 1926 book on the old quantum theory, for instance, Van Vleck talks about both intermediate orbits and virtual oscillators, yet makes no connection between them (Van Vleck, 1926, pp. 23–24, p. 159 ff.). One example of a text in which the connection is *not* made is, of course, no evidence that there was no connection. But the burden of proof, it seems to me, is on Jähnert to provide clear evidence for something that gets that much emphasis in his thesis. I would also expect him to provide evidence *against* the standard account (amplified in "On the verge of *Umdeutung*") that *Ersatz* or virtual oscillators originate in the oscillators of the classical dispersion theory of Helmholtz, Lorentz, and Drude.[19]

Here is why I think this is such a serious problem for Jähnert's overall account. Let's bracket for a moment the unproven claim about the genetic relation between 'intermediate orbits' and 'virtual oscillators', tied together, in Minsky's language, as two subsequent *settings* of the *frame* 'state in between'. And let's excise it from Jähnert's account for a moment. It turns out that this makes very little difference to his story of what happened in 1921–1925! It is easy to see why: the virtual oscillators play their part regardless of whether or not they are a reincarnation of intermediate orbits. But without this 'reincarnation' element, the account offered by Jähnert *basically* (the qualification will be explained below) turns into another version of the one given in "On the verge of *Umdeutung*" (and older work that our paper was based on). Both in Jähnert's and in these other accounts, it is noted that the transition to matrix mechanics was brought about by applying the translation scheme inspired by the construction (with the help of the correspondence principle) of the Kramers dispersion formula not to one isolated formula but to the basic laws of classical mechanics. That is what Heisenberg meant by *Umdeutung*.

On p. 92, for instance, Jähnert says:

> In ihrer Beschäftigung mit der Dispersionstheorie und *dem "state in between" in seiner neuen Form des virtuellen Oszillators* entwick-elten Born, Kramers und Heisenberg zunächst eine allgemeine Vor-schrift, um die Differentialquotienten der klassischen Mechanik in Differenzenquotienten der Quantenmechanik zu übersetzen. Diese Übersetzungsvorschrift wies für Born und Heisenberg über einzelne Modelle hinaus und wurde zum wichtigsten Werkzeug bei der Suche nach einer neuen "Quantenmechanik". Als solches trat die formale Übersetzungsvorschrift auch an entscheidender Stelle in Heisenbergs

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[19]"[I]n the quantum dispersion theory of the 1920s, the oscillators of the classical theory were grafted onto the Bohr model" (Duncan and Janssen, 2007, p. 580).

> Umdeutung auf und ermöglichte die Transformation der alten Quan-
> tenbedingung in eine neue quantenmechanische Form (my empha-
> sis).

Here is what Duncan and I say:

> Rather than using classical mechanics to analyze features of electron
> orbits and translating the end result into a quantum formula, as
> Kramers and others had done ... Heisenberg translated the Fourier
> series for the position of an electron that forms the starting point of
> such classical calculations into a quantum expression. He replaced
> the amplitudes and frequencies by two-index quantities, referring to
> the initial and final state of a quantum transition, respectively, and
> thus replaced classical position by an array of numbers associated
> with transitions between states. Reinterpreting rather than replac-
> ing the old theory, he assumed that these new quantities would sat-
> isfy all the familiar relations of Newtonian mechanics (Duncan and
> Janssen, 2007, sec. 3.5, p. 593).[20]

It is only in a later section that we discuss the (well-documented) connection with
virtual oscillators. As we point out, there is "ample evidence" for the claim by
McKinnon (in a paper Jähnert also used) that

> "[t]he virtual oscillator model played an essential role in the process
> of reasoning that led Heisenberg to the development of quantum me-
> chanics" ... In fact, this thesis is not nearly as controversial as Mac-
> Kinnon makes it sound. In the entry on Kramers for the *Dictionary
> of Scientific Biography*, the sober-minded Dutch physicist Casimir
> states matter-of-factly: "The notion of virtual oscillators was the
> starting point of Heisenberg's quantum mechanics—the virtual os-
> cillators became the matrix elements of the coordinates" (Duncan
> and Janssen, 2007, p. 616).

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[20]Here is how we characterize the translation "scheme of the dispersion theory" (as Heisenberg
calls it in his interview for the *Archive for History of Quantum Physics*):

> We now translate this classical formula into a quantum formula. The idea is
> to construct a quantum formula that merges with the classical formula in the
> limit of high quantum numbers. This is done in three steps. For high values
> of the quantum number i, the derivatives $\partial/\partial J_i$ can be replaced by difference
> quotients, the square of the amplitudes $A_{\tau_i}(J_l)$ by transition probabilities $A_{i \to j}$
> (where $|i - j|$ is small compared to i), and orbital frequencies ν_i by transition
> frequencies $\nu_{i \to j}$. We then take the leap of faith that the resulting formula holds
> for all quantum numbers (Duncan and Janssen, 2007, p. 593).

Jähnert discusses these same three steps, spending, as Duncan and I do, much more time on the
first two, which were only developed in the early 1920s, than on the first one which goes all the
way back to the embryonic version of the correspondence principle in Bohr's 1913 paper.

If the italicized clause in the passage from Jähnert's thesis quoted above ("dem "state in between" in seiner neuen Form des virtuellen Oszillators") is replaced simply by "virtuellen Oszillatoren"—i.e., if the bit about virtual oscillators instantiating the "state in between" is dropped—he is *basically* saying the same thing Duncan and I are saying. This illustrates the point I made above, namely that the bit about "states in between" instantiated first as *Zwischenbahnen* and then as virtual oscillators does not make that big of a difference in the end. In the period of interest, virtual oscillators take center stage, irrespective of their relation to intermediate orbits.

Finally, let me explain the qualifier "basically" used twice above. Even if the notion of the "state in between" is bracketed, (at least) one substantial disagreement between Jähnert and Duncan and Janssen (2007) remains. We argue that pre-*Umdeutung* the correspondence-principle technique (coupled with the visualization provided by virtual oscillators) did not produce any important results besides the Kramers dispersion formula and related formulae for emission and absorption (the latter found by Van Vleck). In particular, commenting on a testy exchange between Born and Van Vleck, we note that Born did not produce such results. Here is the relevant passage from our paper:

> Born had written:
>> I am sending you my paper On Quantum Mechanics, which pursues a goal similar to yours. While you limit yourself to the correspondence with high quantum numbers, I conversely aim for rigorous laws for arbitrary quantum numbers.[21]
>
> To which Van Vleck replied:
>> I have read with great interest your important, comprehensive article. There is, as you say, considerable similarity in the subject matter in your article and mine, especially as regards to dispersion ... As noted in your letter you mention more explicitly than do I the fact that formulas of the quantum theory result from those of the classical theory by replacing a derivative by a difference quotient. I have stressed the asymptotic connection of the two theories but I think it is clear in the content of my article that in the problems considered the classical and quantum formulas are connected as are derivatives and difference quotients.[22]
>
> ... Van Vleck used the correspondence principle—in particular, the replacement of derivatives by difference quotients—to *check* that

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[21]Born to Van Vleck, October 24, 1924 (AHQP).
[22]Van Vleck to Born, November 30, 1924 (AHQP).

quantum formulae merge with classical formulae in the limit of high quantum numbers, whereas Born wanted to use the principle to *construct* quantum formulae out of their classical counterparts. We sympathize with Van Vleck's point in response to Born that the difference between the two approaches should not be exaggerated . . . Van Vleck knew perfectly well how to construct quantum formulae on the basis of correspondence considerations when he had to. And while it is true that Born put more emphasis on the constructive use of the correspondence principle, this did not lead Born to additional results of any consequence for subsequent developments. It was left to Heisenberg to show how one could use the correspondence principle as a guide not just to a few new formulae but to a whole new theory (Duncan and Janssen, 2007, p. 638)

Jähnert quotes this exact same exchange (and should have referred to our discussion of it) but sympathizes with Born rather than with Van Vleck:

> With the same letter I am sending my paper On Quantum Mechanics to you, which has an aim similar to yours. While you restrict yourself to the correspondence for high quantum numbers, I am on the contrary aiming to get exact laws for arbitrary quantum numbers. [Born an Van Vleck 24 October, 1924 (AHQP 49,9) Hervorhebung im Original]

Van Vleck auf der anderen Seite ließ bei gleichem Resultat keinerlei Prioritätsansprüche für Borns Herleitung gelten:

> As noted in your letter you mention more explicitly than do I the fact that formulae of the quantum theory result from those of the classical theory by replacing a derivative by a different quotient [sic!] [our transcription is incorrect here, MJ]. I have stressed the asymptotic connection of the two theories but I think it is clear in the content of my article that in the problems considered the classical and quantum formulae are connected as are derivatives and different quotients. [Van Vleck an Born 13.11.1924 (AHQP 49,9)]

Während also Van Vleck nur eine marginale Differenz zwischen sich und Born ausmachen konnte, war Born davon überzeugt, dass die Differenz-Differential-Transformation in ihrer allgemeingültigen Form den Kern des Korrespondenzprinzips ausmachte und sah darin "einen Anfang einer vernnftigen Quantenmechanik [. . .]". [Heisenberg an Pauli 8.06.1924 (Pauli, 1979, 155)]

Let me emphasize that I think of this as an issue where competent commentators can reasonably disagree with one another. I mention it mainly to give a sense of similarities and differences between Jähnert's account and ours.

In summary, I feel there are too many problems with Jähnert's thesis to award it the Hanneke Janssen Prize. Jähnert clearly has some original ideas (though not as many and not as revolutionary as he makes it sound in places), but (a) he does not do justice to the existing literature (or, more charitably, situates his project in the wrong literature), (b) has limited command of the (admittedly difficult) mathematics and physics he needs (and failed to use a paper coming out of the Berlin quantum project to educate himself), and (c) fails to produce a convincing argument for a claim central to his account of the transition from the old quantum theory to matrix mechanics, namely that 'virtual oscillators' succeeded 'intermediate orbits' as the *setting* of the *frame* 'state in between' (though a slightly modified version of the claim may well be salvageable [see note 18]).

3. Recommendation

Given the serious reservations about the submissions of both Acuña and Jähnert that I registered above, I feel strongly that it would send the wrong message to young scholars if either of these submissions were awarded the Hanneke Janssen Prize. I understand that there is another candidate, Matt Gorski, a student of Don Howard, who submitted a paper on Griffith's consistent-histories interpretation of quantum mechanics. Regardless of the merits of Griffith's approach or lack thereof, this might well be a worthy, if not particularly exciting, winner of this year's Hanneke Janssen Prize. My recommendation is that the jury look into this possibility or not award the prize at all this year.

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