



# Einstein's second-biggest blunder: the mistake in the 1936 gravitational-wave manuscript of Albert Einstein and Nathan Rosen

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## Abstract

In a 1936 manuscript submitted to the *Physical Review*, Albert Einstein and Nathan Rosen famously claimed that gravitational waves do not exist. It has generally been assumed that there was a conceptual error underlying this fallacious claim. It will be shown, through a detailed study of the extant referee report, that this claim was probably only the result of a calculational error, the accidental use of a pathological coordinate transformation.

In July 1936, Albert Einstein and his assistant Nathan Rosen famously received a critical referee report for their paper “Do Gravitational Waves Exist?”, which they had submitted to the *Physical Review* several weeks earlier and in which they argued that the Einstein equations of general relativity did not allow for plane gravitational waves. This episode and the ensuing developments have been meticulously reconstructed by Daniel Kennefick, who also established the fact that the anonymous referee had been the American cosmologist H.P. Robertson (Kennefick 2007, pp. 79–104).

Einstein reacted quite strongly to the referee report and withdrew the paper. Indeed, not every paper submitted to the *Physical Review* was reviewed in those days (Lalli 2016), and it was the first time that Einstein had received such a report, despite having recently published two (not that uncontroversial) papers in the *Physical Review* (Einstein and Rosen 1935; Einstein et al. 1935). Einstein submitted the paper unchanged to the *Journal of the Franklin Institute*, but then—according to the recollections of Leopold Infeld, who succeeded Rosen as Einstein's assistant in the summer of 1936—discovered a mistake in the paper. Robertson, who was a colleague of Einstein's at Princeton, gave Einstein a decisive hint for fixing the mistake by “reinterpret[ing] [it]

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in a cylindrical polar coordinate system” (Rosen 1956, p. 172). Einstein then contacted the editor of the *Journal of the Franklin Institute* and presented him with a modified version of the paper in which the consequences drawn from the equations were fundamentally changed.<sup>1</sup> This modified version was then published and is the only extant version of the paper (Einstein and Rosen 1937). The argument here is *for* the existence of cylindrical gravitational waves; plane gravitational waves are not discussed.

In the meantime, Rosen had left for the Soviet Union, where he reacted to the publication of the modified version of the joint paper by writing a paper of his own, published in the *Physikalische Zeitschrift der Sowjetunion* (Rosen 1937b).<sup>2</sup> In this paper, Rosen argued *against* the existence of plane gravitational waves. Rosen’s paper was many years later identified as being mistaken by Bondi et al. (1959). When they presented their own plane gravitational-wave solutions to the Einstein equations in 1959, they pointed out that Rosen’s conclusions were erroneous, because he had demanded that there exist a *single*, singularity-free coordinate system covering the entire space-time manifold. Rosen’s mistake had been to impose too stringent requirements on the singularity structure of the metric.

Note that there are *two* mistakes here: the mistake that Einstein discovered and corrected and the mistake that Rosen made in his single-authored paper. The temptation is large to conflate the two. There is no need for the author to point fingers here, as he himself has, at least implicitly, assumed that Einstein’s original mistake was the same as Rosen’s and was thus only cleared up in the late 1950s, in the renaissance of general relativity (Blum et al. 2018, p. 535). It is the purpose of this paper to argue that the two mistakes are very much distinct. I will thus not have much more to say about Rosen’s mistake; it is quite explicitly spelled out by Bondi, Pirani, and Robinson, and its conceptual import will be analyzed in a forthcoming paper by Dennis Lehmkuhl and collaborators. Instead, in this paper I will try to reconstruct what the mistake was in the original paper submitted by Einstein and Rosen. This reconstruction will be based primarily on Robertson’s referee report, which, as opposed to the original manuscript, is extant and was in fact published in Kennefick (2007, Appendix A).<sup>3</sup> I will lay the foundations for identifying the mistake in Sect. 1, discuss the mistake itself in Sect. 2, and then end with some brief conclusions in Sect. 3.

## 1 The relations between the paper, the manuscript, and the report

The greatest difficulty in pinning down the mistake made by Einstein and Rosen in the manuscript submitted to the *Physical Review* (or originally submitted to the *Journal of the Franklin Institute*) is, of course, that this manuscript is no longer extant. Some assumptions about this document will necessarily have to be made, but I hope that the reader will find them all well founded. The central assumption is that the original

<sup>1</sup> Letter of 13 November 1936, Albert Einstein Papers, Hebrew University of Jerusalem, Document 20-217. I would like to thank Diana Buchwald of the Einstein Papers Project for providing me access to this letter.

<sup>2</sup> There is also a somewhat shorter version of this paper in Ukrainian (Rosen 1937a).

<sup>3</sup> I received this information and further valuable inspirations and hint at a reading group hosted by the Lichtenberg Group for History and Philosophy of Physics at the University of Bonn. I would like to thank all participants of this reading group, in particular, Dennis Lehmkuhl, Daniel Kennefick, and Christian Rößen.

manuscript used a certain ansatz for the metric, namely

$$ds^2 = A(dx_4^2 - dx_1^2) - Bdx_2^2 - Cdx_3^2, \quad (1)$$

where, following the conventions of the time,  $x_4$  is used for the time coordinate. Furthermore, all coefficients are taken to be positive, i.e., the signature of the metric is the one with predominantly minuses.

This metric ansatz is used in the published Einstein–Rosen paper (Eqs. 14 and 15) and in the Rosen paper (Eq. 4). It also appears in the referee report. This report is structured into several parts. Parts (a)–(c) concern typos and missing references. Part (d) discusses what I consider to be the central mistake. Part (e) discusses the relation of the paper to work by Weyl; it will also become important. Parts (f) and (g) discuss additional points that are irrelevant to my discussion here. Robertson gives the above metric in his part (d), as his first equation, and identifies it as the ansatz used in the original manuscript. It is thus safe to say that this ansatz, with this notation, was used by Einstein and Rosen throughout their entire work. It arises from assuming that the metric does not depend on  $x_2$  and  $x_3$ , implying

$$g_{23} = g_{12} = g_{13} = g_{24} = g_{34} = 0. \quad (2)$$

The intuition here, in all cases, was that we have a gravitational wave propagating in  $x_1$  direction, so that the metric depends only on  $x_1$  and  $x_4$ . This is supplemented by a coordinate condition ensuring  $g_{14} = 0$  and  $g_{11} = -g_{44}$ .

While this metric ansatz may be used in all of the sources, they differ in their interpretation of the coordinates, that is, in particular in the ranges assumed for them. In the Rosen paper and in the referee report, all four coordinates are taken to run from  $-\infty$  to  $\infty$ . Following the terminology of Robertson's report, we shall refer to such coordinates as "Cartesian"; this is only a statement about the values the coordinates can take, without any implications for the physical properties of the spacetimes involved.<sup>4</sup> In the Einstein–Rosen paper, on the other hand, the spatial coordinates are taken to be cylindrical, with  $x_1$ , the radial coordinate, running from 0 to  $\infty$  just like  $x_2$  the axial coordinate,<sup>5</sup> while the angular coordinate  $x_3$  runs from 0 to  $2\pi$ . Toward the end of the Einstein–Rosen paper, the authors perform a coordinate transformation that is well defined only if the coordinates are taken to be cylindrical. It is the central thesis of this paper that the mistake in the original manuscript was that Einstein and Rosen had taken their coordinates to be Cartesian, but still performed this coordinate transformation, which was then pathological. I will take the rest of this paper to argue for this thesis.

To this end, let us first look at this coordinate transformation in the published Einstein–Rosen paper, where it is mathematically unobjectionable. The first step was to introduce new metric variables to simplify the field equations.<sup>6</sup> This is just a re-labelling; it is not the coordinate change in question, nor is it even a coordinate change.

<sup>4</sup> Rosen instead used the term "quasi-Cartesian."

<sup>5</sup> It is not clear why Einstein let the axial coordinate take only positive values, so that he was effectively dealing only with one half of space. But that is what he did and it does not make a difference for what follows.

<sup>6</sup> These same new variables were introduced in the Rosen paper, but were not followed up by the critical coordinate transformation.

The new variables were

$$\begin{aligned}\alpha &= \ln A \\ \beta &= \frac{1}{2} \ln (B/C) \\ \gamma &= \frac{1}{2} \ln (BC).\end{aligned}\tag{3}$$

This was then followed by the actual coordinate transformation. The new coordinate system left the symmetries that went into the metric ansatz untouched, and consequently, the coordinates  $x_2$  and  $x_3$  are unaffected and the metric retains the form of Eq. 2. The coordinates  $x_1$  and  $x_4$  are transformed to new coordinates  $\bar{x}_1$  and  $\bar{x}_4$ , such that the new coordinate  $\bar{x}_1$  is related to the components of the metric as

$$e^\gamma = a\bar{x}_1,\tag{4}$$

where  $a$  is some proportionality constant.<sup>7</sup> We are thus dealing with a transformation to a metric-dependent coordinate system, where one of the spatial coordinates is fixed to be proportional to a function of the metric variables. In the new coordinate system, the field equations then take the form<sup>8</sup>

$$\beta_{11} - \beta_{44} + \frac{\beta_1}{\bar{x}_1} = 0\tag{5}$$

$$\alpha_1 = \frac{1}{2}x_1(\beta_1^2 + \beta_4^2) - \frac{1}{2\bar{x}_1}\tag{6}$$

$$\alpha_4 = \bar{x}_1\beta_1\beta_4,\tag{7}$$

where the indices indicate partial derivatives with respect to the indicated coordinate. One observes the utility of the coordinate transformation: there are now only two independent variables, because  $\gamma$  can be eliminated in favor of an explicit dependence on the coordinate  $\bar{x}_1$ .

I will now argue, based primarily on the referee report, that this same coordinate transformation was used in the original Einstein–Rosen manuscript, but with Cartesian coordinates. I will then show that the coordinate transformation is indeed pathological in that case and argue that this was the central mistake pointed out in the referee report. The first obstacle is that neither the coordinate transformation itself nor the field equations in the new coordinate system are reproduced in the referee report. But thankfully, the referee report delivers quite a lot of other information about the original manuscript. It is, one might say, a rather loquacious referee report. Indeed, this is the reason why Einstein and Rosen’s central mistake, which Robertson did point

<sup>7</sup> The proportionality constant is irrelevant, because the field equations only contain derivatives of  $\gamma$ , not  $\gamma$  itself, so  $a$  does not show up in the field equations.

<sup>8</sup> Note that in the published Einstein–Rosen paper, the overlines on the new coordinates are dropped immediately. We will keep them, because the overlines were kept in the referee report and we will thus be able to compare equations more easily.

out explicitly, is somewhat buried in erudition and has not been clearly worked out until now.

However, the verbosity of the referee report is *also* a boon to the historian, as it makes it possible to make a strong case that the above coordinate transformation was indeed also performed in the original paper. Robertson, somewhat gratuitously, points out that something he refers to as the “complete wave solution” in the original manuscript is “*mathematically* equivalent to the Weyl–Levi–Civita axially symmetric static field” (Kennefick 2007, p. 284), for which he cites Hermann Weyl’s book “Raum-Zeit-Materie” (Weyl 1923, p. 266). He then goes on to spell out this mathematical equivalence, by showing how the coordinates of Weyl are related to the coordinates of the Einstein–Rosen manuscript, providing us with a Rosetta Stone:<sup>9</sup>

$$r = \bar{x}_1, z = i\bar{x}_4, \theta = \bar{x}_2, t = i\bar{x}_3. \quad (8)$$

The coordinates of the Einstein–Rosen manuscript are denoted by  $\bar{x}_\mu$ . This is the first indication that the “complete wave solution” of the original manuscript is in fact obtained only after a coordinate transformation, which I claim is the same coordinate transformation as in the published paper. To bolster this claim, I will now argue that Weyl’s metric, translated according to Robertson’s prescription, in fact gives the metric ansatz of Eq. 2 in the overlined coordinates as used in the published paper. The metric in the overlined coordinates (and the new metric variables of Eq. 3) is not explicitly given in the published Einstein–Rosen paper, but the calculation is straightforward, giving

$$ds^2 = e^\alpha (d\bar{x}_4^2 - d\bar{x}_1^2) - a\bar{x}_1 (e^\beta d\bar{x}_2^2 + e^{-\beta} d\bar{x}_3^2). \quad (9)$$

The reason why I need to “argue” that this is equivalent to the Weyl metric using Robertson’s instructions and cannot simply “show” it, is that the translation of the coordinates (Eq. 8) is insufficient. We also need a translation for the metric variables. The Einstein–Rosen metric in the overlined coordinates these are  $\alpha$  and  $\beta$ . The Weyl (and Levi–Civita) metric reads<sup>10</sup>

$$ds^2 = e^{2\phi} dt^2 - e^{2\gamma_W - 2\phi} (dr^2 + dz^2) - r^2 e^{-2\phi} d\theta^2 \quad (10)$$

with the metric variables  $\gamma_W$  and  $\phi$ . I have added the index  $W$  to avoid confusion with the  $\gamma$  of Einstein and Rosen (Robertson explicitly referred to this function as “Weyl’s  $\gamma$ ”). We can then directly perform the unambiguous relabelling of the coordinates using Eq. 8, obtaining

$$ds^2 = -e^{2\phi} d\bar{x}_3^2 - e^{2\gamma_W - 2\phi} (d\bar{x}_1^2 - d\bar{x}_4^2) - \bar{x}_1^2 e^{-2\phi} d\bar{x}_2^2. \quad (11)$$

But now, we also need to translate the metric variables. For this, Robertson gave the following instructions:

<sup>9</sup> For some reason, Robertson here uses  $x_0$ , but I have replaced that with  $x_4$  for notational consistency.

<sup>10</sup> Robertson, in his referee report, pointed out a number of typos in Weyl’s book, which I have tacitly corrected for this paper, so as not to add a further layer of confusion.

$$\phi = -\frac{1}{4}\beta - \frac{1}{2} \ln \bar{x}_1 \quad (12)$$

$$\gamma_W = -\frac{1}{4}\beta + \frac{1}{2} \ln \bar{x}_1 + \frac{1}{2}\delta. \quad (13)$$

In order for this to transform the Weyl metric into the Einstein–Rosen metric, we need to make a number of smaller hypotheses concerning conventions and notation, none of which, I think, are overly contrived. They are ultimately justified by the full consistency of the translation, and I in fact originally arrived at them in the attempt to make this translation work. The assumptions are:

1. First of all, Robertson, at a different point in the report (Kennefick 2007, p. 283), explicitly refers to the variable  $\gamma$  used in the original paper and identifies it with  $\ln(BC)$ . This differs from the expression for  $\gamma$  in the published paper by a factor of 2. I will assume that  $\beta$  was also defined without the factor of 1/2 in the original manuscript, so that the  $\beta$  in Robertson's instructions needs to be replaced by  $2\beta$ .
2. I will assume that the coordinate system used in the original manuscript was essentially the *same* one as in the published paper, independent of the conventions used in defining  $\gamma$ . The only difference I assume is that  $a$  was simply taken to be equal to 1 in the original manuscript, since there is no constant that could be identified with  $a$  anywhere in Robertson's report. This would mean that in the original manuscript, the decisive coordinate transformation was determined through the relation

$$e^{\gamma_{\text{old}}/2} = \bar{x}_1, \quad (14)$$

where  $\gamma_{\text{old}}$  is the function  $\gamma$  as defined in the original manuscript (and in Robertson's report). In the published paper, Einstein then decided to include the factor of 1/2 in the definition of  $\gamma$ , instead of in the coordinate transformation. We will thus leave  $\bar{x}_1$  in Robertson's instructions as is and compare it with the Einstein–Rosen metric with  $a$  set equal to 1.

3. Somewhat surprisingly, Robertson calls the functions in the complete wave solution  $\beta$  (as was to be expected, though recall that I am assuming that this is  $2\beta$  in the convention of the published paper) and  $\delta$  (which was not to be expected). I have simply assumed that Robertson's  $\delta$  is the  $\alpha$  of the published Einstein–Rosen paper, and this assumption has turned out to be consistent.
4. Robertson's translation expressions also include a gap in the expression for  $\phi$ . Clearly, a symbol appears to be missing here. Indeed, in Robertson's typewritten report, many of the mathematical symbols, including plus and minus, had to be put in by hand, and at this point he appears to have forgotten to do so. I have played around with different possibilities, and only the assumption that a plus sign is missing here leads to consistent results, so I will proceed on this assumption.

Using these three assumptions, Robertson's translation instructions now read

$$\phi = -\frac{1}{2}\beta + \frac{1}{2} \ln \bar{x}_1 \quad (15)$$

$$\gamma_W = -\frac{1}{2}\beta + \frac{1}{2}\ln \bar{x}_1 + \frac{1}{2}\alpha. \quad (16)$$

Inserting this into the Weyl metric (Eq. 11) directly gives the Einstein–Rosen metric in the overlined coordinates (Eq. 9), if the proportionality constant  $a$  is set equal to one. This result can be made even more plausible by also translating the field equations. Robertson claims that, using his translation, the field equations as given by Weyl<sup>11</sup> deliver a set of equations from the Einstein–Rosen manuscript. And if we translate Weyl's field equations using the above instructions, we indeed obtain the field equations in the coordinate system where  $\bar{x}_1 = e^\gamma$ , as given in Eq. 5 (and in the published Einstein–Rosen paper).<sup>12</sup> I hope to have thereby convinced the reader that in all probability, the original manuscript used the same coordinate transformation as in the published paper.

## 2 The mistake

This finally brings us to the mistake in the original Einstein–Rosen manuscript and to the question: what is wrong with this coordinate transformation? Or rather: what is wrong with this coordinate transformation in Cartesian coordinates—for recall that Robertson explicitly mentions that the original manuscript used Cartesian coordinates. The referee report is actually rather explicit about what is wrong, but it is only mentioned briefly at the beginning of the “logical” (as opposed to “typographical”) comments and then is not revisited, obscuring the fact that this is the only veritable mistake pointed out in the entire report. Robertson begins by stating that the class of solutions studied by Einstein and Rosen should include flat spacetime (Kennefick 2007, p. 283). After all, flat spacetime is consistent with all of the symmetry properties they assume. Put differently: flat spacetime is just a gravitational wave with zero amplitude. Now, if the coordinates are taken to be Cartesian, this implies that, using the metric ansatz of Eq. 2,  $A = B = C = 1$  should be a possible solution. This is, however, problematic, as pointed out explicitly by Robertson:

But the normalization which they adopt (eq. (23a) and that below eq. (30b)) preclude this, for in each case they have in essence used  $\gamma = \ln(BC)$  as one of the space-time variables (Kennefick 2007, p. 283).

I take it that “variable” here means “independent variable”, so that Robertson is here really just referring to the use of  $\gamma$  as a space-time coordinate. And that is indeed “essentially” what happens in the decisive coordinate transformation, which—as I have argued at length—was probably used in the original manuscript as well. But in flat spacetime and Cartesian coordinates, we have  $B = C = 1$ , which implies that  $\gamma = (\frac{1}{2})\log(BC)$  is identically equal to zero. And this is of course in blatant contradiction with the assumption that we can find a coordinate system in which

<sup>11</sup> Weyl's field equations are given as a total differential of  $\gamma_W$  (bottom of p. 266), which yields two partial differential equations, and the “potential equation” for  $\phi$  (top of p. 267).

<sup>12</sup> Note that for this equivalence, no use need be made of the assumption  $a = 1$ .

$a\bar{x}_1 = e^\gamma$ . This would imply that  $\bar{x}_1$  is a constant. This contradiction also explains why Robertson chose the word “variable” and even underlined it.

As Robertson pointed out, this difficulty is easily mended by—from the start—taking the coordinates to be cylindrical rather than Cartesian.<sup>13</sup> When we identify  $x_1$  with the radius,  $x_2$  with the polar angle, and  $x_3$  with the coordinate along the cylindrical axis, we get as a flat space-time solution  $A = 1$ ,  $B = x_1^2$ , and  $C = 1$ .<sup>14</sup> The contradiction then immediately disappears. Indeed, in flat spacetime, the equation  $\bar{x}_1 = e^\gamma$  turns into  $\bar{x}_1 = x_1$ , so that for flat spacetime in cylindrical coordinates, the decisive coordinate transformation is simply the identity mapping. The coordinate system used thus almost *implies* cylindrical coordinates, as Robertson indeed seems to suggest (the coordinate “ $x_2$  should be interpreted as an azimuth instead of a Cartesian coordinate” (Kennefick 2007, p. 283), my emphasis). As Robertson further points out, the field equations in this coordinate system can be immediately interpreted as leading to gravitational waves, since the first one of them (our Eq. 5, apparently Eq. 31 in the original paper) is the equation for cylindrical waves.

After Einstein realized that the coordinate system was problematic, he appears to have immediately fallen in line. Indeed, he could keep most of the paper. All he needed to do was point out from the start that he was working in cylindrical coordinates, and then adjust the final conclusions, i.e., the last two pages after performing the decisive coordinate transformation. This is made further plausible by the fact that while in the rest of the paper equations are frequently numbered, even if they are not referred to later on, equation numbering stops entirely right where the interpretation of the field equations (now in cylindrical coordinates) begins (p. 53)—implying that these last two pages may have been typeset at a later point. As Robertson had done in his referee report (and probably also in personal conversation with Einstein), the published paper highlights the fact that one of the field equations in the new coordinate system is the wave equation in cylindrical coordinates and constructs cylindrical waves from it. We thus see that there was no deep conceptual misunderstanding at play here, but merely a bad choice of coordinate system. If anything, we see here how challenging the notion of general coordinate invariance still was 20 years after the formulation of GR. In any case, it seems very plausible that Einstein noticed this mistake by himself, as Infeld would later claim.

What the analysis given here cannot reconstruct is how the original argument against gravitational waves went. It must have started from the field equations in the overlined coordinate system interpreted with Cartesian coordinates and somehow concluded that this could not contain gravitational waves. Whatever this argument was, Robertson did

<sup>13</sup> Indeed, the ease with which Robertson moves to this solution in the referee report may well have obscured the fact that he was here pointing out the central mistake of the manuscript. He even remarks in this context that he does “not consider this as particularly serious”, but this relates more generally to the issue of how to deal with metric-dependent coordinate transformations (such as the one in question here) in the linearized approximation. In the framework of Einstein and Rosen (the published paper), the comparison with the linearized approximation was always necessary to decide which gravitational waves were truly physical (cf. the discussion at the bottom of p. 51 of that paper).

<sup>14</sup> Note that in the published paper, Einstein assigns  $x_2$  to the cylindrical axis and  $x_3$  to the polar angle. Given the symmetry between  $x_2$  and  $x_3$  in the original metric of Eq. 1, this is equally viable and would be what we ended up with if, instead of following Robertson’s convention, we had switched  $x_2$  and  $x_3$  in the translation relations of Eq. 8.



not explicitly criticize it in the report, and there is no immediate reason to believe that it made erroneous use of singularity arguments. Given the pathology of the coordinate system used, it may well be that the original arguments against plane gravitational waves themselves did not make use of erroneous argumentation—they were simply based on false premises.

While this clarifies, I believe, the story behind the first mistake, there is still the question of the Rosen paper. Rosen does not use the  $\bar{x}_1 = e^\gamma$  coordinate system, so the paper indeed avoids the problem pointed out in the referee report. Rosen's argument must thus be different from the one used in the original paper. It may, of course, have been similar in spirit (especially with regards to the attitude toward singularities). However, it may well have been entirely different. In general, I would now assume, given the above analysis, that Rosen's paper differed more strongly from the original Einstein–Rosen manuscript than did the published Einstein–Rosen paper. Rosen's slipshod argument (the “second” mistake) would then have been an ill-conceived attempt to save the conclusions of the original paper, rather than, as Einstein had done, radically altering the conclusions while keeping most of the mathematical apparatus, including the decisive coordinate transformation. It would have been a conceptual rather than a calculational error.<sup>15</sup> Answering this question requires a more detailed study of attitudes towards singularities in the 1930s, to see whether Rosen's criterion was indeed, as Bondi, Pirani, and Robinson would later claim, usual at the time. I will not provide such a detailed study in this paper, and thus simply end with a few brief remarks.

### 3 Conclusions

I have argued that in the well-known story of the withdrawn Einstein–Rosen manuscript on gravitational waves there are really two mistakes at play: one in the original manuscript (which was then corrected by Einstein and avoided by Rosen) and one in the later paper by Rosen. The original mistake in the Einstein–Rosen manuscript turned out to be a bona fide calculational error, and not the result of some deep conceptual misunderstanding of general covariance and gravitational waves.<sup>16</sup>

There is no universal historical lesson here. Some years back, I wrote a paper on how a supposed calculational mistake was really a conceptual shortcoming (Blum 2015); now, I am presenting a paper where things go the other way. Concerning the specific historical situation and the history of gravitational waves in particular, my results do warrant some re-evaluation of the degree of confusion (especially on Einstein's side) concerning the existence of gravitational waves. However, for such a re-evaluation to be meaningful, the results presented here would have to be supplemented by a more detailed study of the second mistake made in the Rosen paper. As regards Einstein's

<sup>15</sup> Pirani, in a letter to Alfred Schild dated 11 May 1957 (Alfred Schild Papers, Dolph Briscoe Center for American History, The University of Texas at Austin), described Rosen's paper as “wrong (physically, even if there are no actual mistakes in it).”

<sup>16</sup> The reader should note that Einstein's “biggest blunder” alluded to in the title of this paper was neither a calculational error nor a conceptual misunderstanding. The quote refers to Einstein's introduction of the cosmological constant into his field equations, of which George Gamow would later claim that Einstein referred to it as his biggest blunder. See O’Raifeartaigh and Mitton (2018).

understanding, this paper highlights how difficult more complicated coordinate transformations (such as the metric-dependent one of the Einstein–Rosen paper) remained more than 20 years after the construction of general relativity.

The primary result of this paper is probably that it clarifies an essential detail in one of the most famous stories about Einstein in America. In addition, there may be a methodological lesson for the history of science here: sometimes historical insights are hidden deep within the calculations.

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