REFLECTIONS ON SCIENCE AND THE MEDIA

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Abstract. Recently, one of my papers with Robert Scherrer and Thomas Kephart from the Vanderbilt Physics department has received widespread media attention. This was my first direct experience with the world of science news, and here I reflect on that experience. This is not intended as a research manuscript on science journalism.

Contents

1. Introduction

A paper that I recently published with Robert Scherrer and Thomas Kephart [15], based on some of my earlier work on relativistic viscous fluids [14], has received widespread media coverage. It featured in The Guardian [13], The New Statesman [51], The Huffington Post [57], Red Orbit [8], The Telegraph [21], Wired [50], and The Independent [27], to cite just a few examples (googling "disconzi big rip" can give one an idea of the magnitude of the coverage). Some background on the scientific content of [15] is given in section 2, with some of its technical aspects reviewed in section 3. Section 4 discusses specifically the media coverage. Section 5 has some final remarks.

This was my first direct experience with science journalists. The way that the media outlets jumped into the story caught me completely by surprise. The quality of the news coverage varied across the different outlets, and I believe that it is fair to say that while some of them made a genuine attempt to describe the ideas of our work, others seemed more interested in solely capturing the public's imagination. It is not my intention here, however, to pass judgment on science journalism. Rather, I am interested in trying to understand some of its dynamics, perhaps raise some questions

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for myself and those interested in this debate, and also reflect on what I could have done better to improve the news coverage. In doing so, it is important to stress that I am not a researcher on the complex topic of either the media in general or the science media in particular. Furthermore, my discussion here is guided primarily by my own experience and some anecdotal points, rather than by hard data. Ultimately, my main goal is to simply register my thoughts and reactions to these developments.

I hope that this text will be largely accessible to a general audience, although I punctuate the discussion with a few technical remarks. An exception is section 3, which is primarily technical.

Remark 1.1. I finish this introduction with a remark on how [15] fits within my research program. This remark is intended primarily for mathematicians and physicists. My two collaborators in [15] are physicists, the paper itself is a physics paper, but I am a mathematician. My research is on partial differential equations, geometric analysis, and mathematical physics. Most of it deals with mathematical properties of equations arising in physical theories, notably with Einstein's equations of general relativity, the equations of fluid dynamics, and, to a lesser extent, some equations that appear in string theory. While I do aspire to prove theorems that can be relevant to physical aspects of such theories, I am ultimately concerned with their mathematical structure. Thus, when I have to choose, as is so often the case, between obtaining a rigorous result under restrictive assumptions, and a potentially more interesting conclusion from a physics standpoint that is below accepted mathematical standards, I do not hesitate to choose the former. This does not mean that I have no interest in physics or the desire to advance its understanding when circumstances allow. In fact, I strive to prove theorems under physically reasonable assumptions, and I believe that establishing a result on, say, Einstein's equations under un-physical hypotheses would be of little interest. Thus, I am more than happy to write physics papers when the opportunity presents itself, which usually happens when I collaborate with physicists. That said, research in physics in a strict sense is a small fraction of my work[1]. Therefore, while I completely understand that physical theories capture the public's interest much more than research in mathematics, I cannot help but feel slightly puzzled by all the media attention drawn by what I consider to be a relatively small part of my work.

2. BACKGROUND

Everything started with my interest in relativistic viscous fluids. Viscosity is a measure of the degree to which a fluid under shear sticks to itself. A fluid is called inviscid when this property is absent. While realistic fluids are viscous, many times viscosity does not play an important role, and thus it can be neglected in the mathematical description of the corresponding fluid. Thus, inviscid fluids are largely an idealization, although idealizations of this sort are crucial in science. The reader can think, for instance, of honey as a very viscous fluid as opposed to water (water at standard conditions is well described by the equations of inviscid fluids [56]). The area of science dedicated to studying fluids is usually referred to as fluid dynamics or hydrodynamics.

Consider now a fluid that moves close to the speed of light or that is subject to a very strong gravitational interaction. In these situations, the equations describing the fluid have to be made compatible with Einstein's theory of general relativity (GR). This will be the situation, for instance,

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[1] To the extent that in my CV: `http://www.disconzi.net/Research/CV.pdf`, I explicitly separate publications in mathematics (which I refer as rigorous or proof-based), from physics (which I refer as applications).

in the study of self-gravitating fluid bodies, such as stars[2]. The resulting object then is a relativistic fluid[3], which is the subject of study in the field of relativistic hydrodynamics.

It is well-known how to make the equations of inviscid fluids compatible with GR (see, e.g., the monographs [7, 11, 46]). However, there is not an accepted way of incorporating viscous fluids into GR (see, e.g., the monographs [11, 46, 54] or the discussion in section 3 for details). While the equations of relativistic inviscid hydrodynamics suffice for many applications (see, e.g., again the monographs [7, 11, 46]), the inclusion of viscosity is important in the study of neutron stars, supernovae, and in some models of the early universe [12, 16, 17, 18, 19, 22, 23, 33, 37, 38, 41, 42, 43, 44, 46, 47, 48, 54].

Over the years physicists have come up with different proposals for the equations of relativistic viscous fluids (see [46] for an overview). Such approaches, however, are plagued by the following drawback. Their equations have solutions that predict, under certain situations, the existence of faster-than-light signals [25, 26, 46]. Since it is well-known that nothing can propagate faster than the speed of light[4], this means that such equations cannot be completely correct, although somewhat satisfactory empirical models of relativistic viscous fluids have been constructed based on them (see references in the previous paragraph). In the physics jargon, we say that a set of equations is causal when all signals propagate at most at the speed of light, and non-causal otherwise. The current state of affairs in relativistic viscous fluids is that the most commonly used equations are causal under certain circumstances, but non-causal under the most general scenarios.

While investigating these matters, I came across a proposal by Lichnerowicz for the equations of relativistic viscous hydrodynamics [35]. Although his approach was put forward back in 1955, and it is, in my opinion, well-motivated, I found no study of those equations, except for a paper by Pichon [45] from 1965. In [14], I proved that, under certain assumptions, Lichnerowicz's approach leads to a sound theory of relativistic viscous fluids. By a sound theory I mean that the equations have solutions and the solutions are causal, i.e., they do not predict faster-than-light signals. See theorem 1.2 in [14] for a precise mathematical statement, and section 3 below for more details. It is important to stress, and I return to this point in section 4, that I am not claiming to have settled the problem of how to describe relativistic viscous fluids. First, because this is ultimately an empirical question, and, thus, beyond my area of expertise. Second, because I did not prove that the equations are causal under the most general assumptions relevant for physics (although I believe my theorem does not have any un-physical hypotheses, see remark 1.1).

When I gave a seminar about my result in our department, Robert Scherrer and Thomas Kephart, from the Physics department, attended. Robert and Thomas are both theoretical physicists. Robert works in cosmology, and Thomas is a particle physicists which, nowadays, is a field with many applications in cosmology. They asked me about the consequences of applying Lichnerowicz's equations in the context of cosmology. While I was aware of the existence of cosmological models with viscosity, it had not crossed my mind to study Lichnerowicz's equations in that context. That is how we three started working together. The result was the publication [15].

I should perhaps make some clarifications for the laymen. Cosmology is the area of physics concerned with the evolution and properties of the universe as a whole, and it is based, among other things, on GR. For instance, the study of the big bang falls under cosmology's umbrella. Many might be asking, what does that have to do with fluids? The short answer is that when one

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[2] To be more precise, other effects, such as electric and magnetic fields, have to be taken into account for an accurate description of stellar dynamics. Therefore, the relevant subject matter is not relativistic hydrodynamics but relativistic magneto-hydrodynamics. These technical distinctions, however, will not be important for the present discussion.

[3] One could distinguish between the cases where one uses special versus general relativity, but I will be concerned solely with general relativity here.

[4] The speed of light in vacuum, to be precise.

constructs cosmological models, the distribution of matter and energy across the universe is treated like a fluid. Once we do that, then it is perfectly legitimate to ask what type of fluid is used in the equations. For example, do we use the equations of inviscid or viscous fluids? Without getting into details, let me just point out that there are reasons to contemplate the case with viscosity (see, e.g., the discussion in [38] and references therein). Another important point is that in the context of cosmology, the viscosity that plays a role is a type of viscosity called bulk viscosity, which is the measure of a fluid's resistance to expansion or contraction (and hence the analogy with honey is not appropriate here; see, e.g., [54]).

As I mentioned, it is not known what the correct equations for relativistic viscous fluids are. Hence, different models of cosmology with viscosity are available, depending, among other things, on which particular choice is made for the equations. Our work was not the first one to investigate the effect of viscosity in cosmology. Indeed, cosmological models with viscosity date back to the seventies [52, 53], and many other works have been done since (see, e.g., some of the papers cited above when I mentioned neutron stars, supernovae, and models of the early universe). What is new in our work is the use of Lichnerowicz's equations, which, in light of my result [14], presents a likely viable alternative to earlier formulations of relativistic viscous fluids[5].

One of the consequences of our work [15] is that it favors a scenario called the "big rip." In order to understand what this means, it is important to first recall something that is currently known about our universe: that it is expanding at an accelerated rate, i.e., not only is the universe expanding in size, but the rate of expansion is increasing. This is a fact established by observations [55]. Its discovery was awarded the Nobel Prize in Physics in 2011 [1]. This accelerated expansion pushes galaxies far apart. The idea of the big rip is that the rate of expansion becomes so large that not only galaxies, but solar systems, planets, and eventually atoms are pushed apart from each other[6]. Mathematically, this means that the rate of expansion becomes infinite, and this is a point I will return to in section 4. The origin of the accelerated expansion of the universe is usually credited to the so-called "dark energy," a mysterious form of energy that we know nearly nothing about other than the fact that it is "out there," and whose understanding is one of the great challenges of contemporary physics.

The idea of the big rip dates back to 2002-2003 [9, 10], and it does not require the introduction of viscosity in the equations. What our paper shows is that it is possible to construct models of the accelerated expansion of the universe that not only allow the big rip to occur, but includes viscosity as one of the key elements driving the universe toward such an ultimate fate.

3. Some technical discussion

Here I shall review some of the technical points underlying the discussion of relativistic viscous fluids. The reader (in particular, the layman) can probably skip this section without compromising the reading of the remaining parts of the text. The reader is also referred to [14, 15]. This section is intended primarily for mathematicians and physicists who want to know more about the technical aspects of relativistic viscous fluids. A thorough and up-to-date discussion of relativistic viscous fluids can be found in [46] (although it does not include Lichnerowicz's formulation, which can be found in [35]).

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[5] The application of the result of [14] to the equations studied in [15] is not, however, straightforward. See the discussion in section 3.

[6] I will spare the lay reader attempts to describe this (e.g., the usual depiction of the the universe as an inflating balloon where all points are stretched far away from each other), since he or she can probably find these popular accounts in many books or on the internet (and such other accounts can probably do a better job than I at explaining it in colloquial terms).

3.1. **Viscosity in relativity.**

As already mentioned, we lack a satisfactory formulation of viscous phenomena within Einstein's theory of general relativity (GR). One of the main reasons for this is the lack of a variational formulation of the classical (non-relativistic) Navier-Stokes equations. In the absence of a variational description of the equations of motion, one does not have a principle that uniquely defines the stress-energy tensor $T_{\alpha\beta}$ in the context of GR. As a consequence, there have been different proposals for what the correct $T_{\alpha\beta}$ should be when viscosity is present. We refer the reader to [38] and references therein for a brief history of different attempts to formulate a viscous relativistic theory.

A natural candidate for a viscous stress-energy tensor for viscous fluids is

$$T^N_{\alpha\beta} = (p+\varrho)u_\alpha u_\beta + p g_{\alpha\beta} - \kappa \pi_{\alpha\beta}\nabla_\mu u^\mu - \vartheta \pi^\rho_\alpha \pi^\mu_\beta (\nabla_\rho u_\mu + \nabla_\mu u_\rho) + q_\alpha u_\beta + q_\beta u_\alpha, \tag{3.1}$$

where p and ϱ are respectively the pressure and density of the fluid, u is its four-velocity, κ and ϑ the coefficients of bulk and shear viscosity[7], g is a Lorentzian metric (with the convention $-+++$), q is the heat-flux or heat conduction, and $\pi_{\alpha\beta} = g_{\alpha\beta} + u_\alpha u_\beta$. p and ϱ are related by an equation known as the equation of state, the choice of which depends on the nature of the fluid and has to be given in order to close the system of the equations of motion. We say that the choice $T^N_{\alpha\beta}$ is natural because it is a straightforward covariant generalization of the stress-energy tensor of a viscous non-relativistic fluid[8], and it reduces to the stress energy tensor of an inviscid fluid[9] when $\kappa = \vartheta = 0$.

The first theories of relativistic viscous fluids were based on (3.1) [20, 34]. Unfortunately, these theories are non-causal [25]. In fact, it is possible to show that the corresponding equations are parabolic if $p + \varrho \gg 1$ [45].

One way of overcoming the lack of causality in such models consists in extending the variables of the theory. This leads to what is known as relativistic extended irreversible thermodynamics [32, 40]. In these approaches, it is possible to show that, under certain circumstances, the equations of motion fall into known classes of hyperbolic equations, exhibiting, as a consequence, finite propagation speed. It is not at all clear, however, that the equations remain hyperbolic under all physically realistic scenarios, and some instances of this approach have in fact been shown to be non-causal [46].

One formulation based on ideas of extended irreversible thermodynamics that has been widely used to study relativistic viscous fluids is the Mueller-Israel-Stewart (MIS) theory [28, 29, 30, 31, 39, 49]. The linearization about equilibrium states of the MIS theory has been shown to be causal [24]. The non-linear theory, however, is also plagued with non-causality behavior [26]. To be fair, such loss of causality is known to happen under extreme physical conditions unlikely to be met by most realistic systems. More precisely, in [26] the authors investigate the relatively simple case where only heat conduction is present, so that the bulk and shear viscosity are zero, and under the assumption of planar symmetry. In particular, the stress energy tensor in this case does not include bulk or shear viscosity, whereas a general feature of viscous theories is that they involve a stress-energy tensor where such terms are present. Under these assumptions, it is shown in [26] that the equations of motion are causal under a restriction on the values of the heat-flux, and non-causal if such a restriction is violated.

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[7] In the simplest situations they are non-negative constants, but more realistic descriptions take them to be a known functions of the other variables, most notably a power of the density ϱ.

[8] We remind the reader that the stress-energy tensor for a non-relativistic viscous fluid is known, despite the absence of a variational formulation of the classical Navier-Stokes equations. It is constructed by exploring the conservation of mass, energy, and momentum of the problem. A similar procedure becomes ambiguous in the setting of GR, see [54].

[9] Which is derived from a variational approach.

In passing, one should note that the MIS is sometimes referred to as "causal dissipative relativistic theory," but strictly speaking that is, in view of what has been said, a misnomer.

It is fair to say that despite considerable progress since the original work of Eckart [20], the description of relativistic viscous phenomena still presents many challenges. Rezzolla and Zanotti conclude their detailed discussion of relativistic viscous fluids pointing out that "the construction of a formulation that is cast in a divergence-type [which amounts to a refinement of the aforementioned extended theories] is not, per se, sufficient to guarantee hyperbolicity" [46].

The stress-energy tensor for relativistic fluids introduced by Lichnerowicz [35] is

$$T_{\alpha\beta} = (p+\varrho)u_\alpha u_\beta + p g_{\alpha\beta} - \kappa \pi_{\alpha\beta}\nabla_\mu C^\mu - \vartheta \pi_\alpha^\rho \pi_\beta^\mu (\nabla_\rho C_\mu + \nabla_\mu C_\rho) + q_\alpha C_\beta + q_\beta C_\alpha, \tag{3.2}$$

where $C^\alpha = \frac{p+\varrho}{r}u^\alpha$ is the dynamic velocity [36] or enthalpy current. r is the rest-mass (or baryon number) of the fluid, satisfying

$$\nabla_\mu (r u^\mu) = 0.$$

The quantity $F = \frac{p+\varrho}{r}$ is the (relativistic) enthalpy of the fluid. The reader is referred to [14, 15] for a discussion of the motivations behind the introduction of C^α.

The (relativistic) vorticity of a fluid is the two form Ω given by $\Omega = dC$, where C is the one form given component-wise by $C_\alpha = g_{\alpha\beta}C^\beta$. The fluid is irrotational when $\Omega = 0$. See [36] or [46] for details.

I can now come back to [14]. There, I consider (3.2) with no heat-flux and no bulk viscosity. I then proved Einstein's equations coupled to (3.2) are well-posed and causal if the fluid is stiff and irrotational. See [14] for a precise statement. The reader can now understand what I meant above when I said that I have not established causality under the most general assumptions relevant for physics. That is because while stiff and irrotational fluids are certainly an important class of fluids, they are far from the most general ones.

I finish this section with some comments on the differences between approaches based on (3.2) and the MIS formulation. These remarks are not intended to claim that Lichnerowicz's proposal is better than the more studied MIS theory, but rather to highlight how little is known about viscosity in GR.

The first thing to notice is that the instability results of [25], which cover not only models based on (3.1) but a large class of theories, do not apply to (3.2). Thus, the simple modification of considering the dynamic velocity instead of the velocity into the viscous terms of the stress-energy tensor suffice to avoid the assumptions of [25].

As said above, known results of causality of the MIS theory are restricted to its linearization, and the case where shear and bulk viscosity are absent, planar symmetry is imposed, and upon a restriction on the heat flux. The result in [14], in contrast, makes no symmetry or near-equilibrium assumption, and treats the full non-linear system, albeit it assumes stiffness and irrotationality. While [14] treats solely the case of shear viscosity, it is important to notice that, from the point of view of causality, this is precisely the most challenging scenario, as it includes the term

$$\pi_\alpha^\rho \pi_\beta^\mu (\nabla_\rho C_\mu + \nabla_\mu C_\rho).$$

This is because this term leads to multiple characteristic in the equations $\nabla^\alpha T_{\alpha\beta} = 0$ due to the presence of the projections $\pi_\alpha^\rho \pi_\beta^\mu$.

Finally, to the best of my knowledge, the aforementioned causality and well-posedness results of the MIS theory [24, 26] do not include coupling to Einstein's equations, i.e., they consider the fluid equations in a fixed background, whereas [14] does treat the full Einstein-fluid system.

3.2. **Applications to cosmology.** In light of what was said above, I believe we were justified in taking (3.2) as an interesting and viable candidate for a model of viscous cosmology. An immediate objection, however, seems to arise. Only shear viscosity is present in [14], whereas Friedmann-Robertson-Walker (FRW) cosmological models can only allow a bulk type of viscosity [55], so that the stress-energy tensor used in [15] is

$$T_{\alpha\beta} = (p + \varrho) u_\alpha u_\beta + p g_{\alpha\beta} - \kappa \pi_{\alpha\beta} \nabla_\mu C^\mu. \tag{3.3}$$

How are we justified in invoking the theorems of [14] to motivate the study of models based on (3.3)? The answer is as follows.

As already pointed out, the shear term is the most problematic one when it comes to establishing some type of hyperbolicity for the equations of motion[10]. As the equations of motion are causal in the presence of the shear term (under the assumptions discussed above), it is not completely unreasonable to conjecture that they will also be causal under the simpler situation where only bulk viscosity is present. While sensible people can certainly disagree on this, a reasonable conjecture of this sort is all we need, I believe, in order to be justified in investigating the physical consequences of adopting (3.3). Moreover, finding that (3.3) can lead to interesting physics can all but spur further study of its causality properties.

In passing, I should perhaps mention that we do know how to establish the causality of Einstein's equations coupled to (3.3). This will appear in a future work. In fact, in a forthcoming paper we will study not only the causality and well-posedness of the equations of motion associated with (3.3), but many general properties (e.g., equilibrium states, entropy production, etc.) of fluids described by (3.3), without restriction to the FRW formalism as in [15].

Once we have adopted (3.3), the work [15] consists basically of a standard analysis of the corresponding FRW equations. It is assumed that the coefficient of bulk viscosity depends on the density via a power law $\kappa = \kappa_0 \varrho^\alpha$, and different values of α are explored. This analysis shows that for a large class of α-values a future-time singularity is reached (i.e., the big rip). The reader is referred to [15] for details.

4. Media coverage

Like most universities, Vanderbilt has an office of research news [2] responsible, among other things, for disseminating the research findings of Vanderbilt faculty to the general public. David Salisbury [3] contacted us to write a story about our paper. I thought that this was a good idea. While, as I mentioned earlier, I have very little knowledge about the world of science news, I am genuinely interested in questions of popularization of science, scientific literacy, public views on science, etc. Thus, I thought this could be an opportunity to engage a little bit with science journalism, although I had never thought that it would generate the sort of media frenzy that it did.

The Vanderbilt news article, i.e., David's story, was published on June 30 [4]. Among all the news stories covering our work, David's was the only one where we had substantial control over the end product. We met with David on a few occasions. He ran drafts of the story by us asking for feedback, which we provided in multiple instances. Therefore, we have only ourselves to blame for any inaccuracy or lack of context that the news article might have.

Keeping in mind that this was a short news story for the general public, hence not intended as a thorough introduction of our research to the laymen, I was satisfied with the version of the text that eventually got published (David would not publish it before we gave him a green light). In hindsight, however, I see a few points that I think we could have explained better.

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[10] We recall that concepts of hyperbolicity and causality are closely linked.

We should, perhaps, have made it very clear that the big rip is not a original idea, but rather dates back to 2002-2003 [9, 10]. I suppose we assumed that this was common knowledge and, thus, outside the context of a scientific paper where one cites the corresponding works. I am not saying that we deliberately said something like, "let us not include any prior mention of the big rip because it is common knowledge." The issue simply did not even crossed our minds, which of course does not mean that we were intentionally trying to sell the idea that the big rip was something original.

Another thing that we could probably have done better was to emphasize the very hypothetical nature of our results. We came up with the idea of constructing a cosmological model based on my earlier results on relativistic viscous fluids which, in turn, were based on long forgotten work by Lichnerowicz. We did that, and found that it favors this big rip scenario. I do think that our results are interesting and call for more study, but of course that does not mean they are correct. Physicists come up with new models all the time, and they cannot be all correct. People come up with new models because they are trying to understand something not yet understood; that is why we call it research. However, I now realize that when the layman reads about a new idea, he or she probably does not realize that this is simply scientists doing usual business, i.e., their day-by-day research. Thus, the layman, upon reading such a news story, might think that the new idea in question is tantamount to Einstein's discovery of general relativity.

A third point that could have been made clearer in the Vanderbilt news article is the relation between fluids and cosmology, as I mentioned in section 2, and the relation between the works [14] and [15]. In particular, I wish we had given more context to the problem of incorporating viscosity in relativity, how [14] is concerned primarily with the mathematical structure of the equations (by no means giving a definitive answer to the question of how to formulate relativistic viscous fluids), whereas [15] is concerned with questions of another nature, namely, the construction of a new cosmological model with viscosity. I also wish we had emphasized more how Lichnerowicz's was the one who originally came up with the formulation of relativistic viscous fluids that we employed, even though he has done little more than simply writing down the equations. As mentioned, it took about 60 years until I showed in [14] that his formulation leads to a viable candidate for a theory of relativistic viscous fluids.

As the saying goes, it is always easier to be smarter in hindsight. I am sure that over time I will realize other points that could probably be improved in the original Vanderbilt news article. One problem with considering such improvements is that they would probably make the text too long for the type of news article it is supposed to be.

There are other points that I do not think had to be included in the Vanderbilt news story per se, but that I wish they would have been included had I known that other news outlets would base their story on [4].

The first of these points is that I do not think of our group as having a leading author. In mathematics, papers are published in alphabetical order (by last name). This practice is sometimes adopted in other fields as well [5]. While I know that different fields have different conventions regarding authorship, and ours is a physics paper, we stuck to the mathematicians standard. Furthermore, conventions aside, I certainly do not think that my contribution was more or less important that those of Robert and Thomas. This was another of those things that had not even occurred to me, and I only thought about it when I started seeing the news stories referring to me as the person who "led the Vanderbilt team," and the only reason I can imagine why a journalist would think so is that my name appeared first in the paper. In fact, David's story explicitly says that our model was developed by myself "in collaboration with physics professors Thomas Kephart and Robert Scherrer," making no mention of leading authorship. My colleagues, fortunately, do not seem to mind it. One of them was, in fact, pleased that the media considered me as the group leader, so that he did not have to deal with all the emails from journalists requesting comments. However, I can certainly imagine a situation where different co-authors would react in a less understanding fashion, potentially generating some unnecessary friction.

In any case, I was deemed the leading author by the science journalists interested in covering the story, and, therefore, I started receiving requests to answer questions, provide comments, etc.

The very first thing that surprised[11] me about the interaction with science journalists is the very short time-line for the publication of a story. For instance, I would receive an email early in the morning asking to answer some questions by late morning, and by early afternoon the story would be already out. This example pretty much summarizes the time-scale of the news processing for basically all the media outlets that contact me regarding our work.

Another thing that struck me is that I was never given a chance to see the text before it was published. This is, I imagine, a natural consequence of the short time-line I referred to above. These two features, the fast communication and publication without my prior reading, are to be contrasted with the process I described above about the creation of the Vanderbilt news story. Even though I did not expect coverage by the traditional media to be similar to that produced by our own university office, I was still taken by surprise by such a difference.

While I understand that it is natural for the news cycle to function at a very fast pace (nobody wants to read yesterday's news), for some reason I was under the impression that science articles had a different time-scale. It did not take me long, however, to realize that this was an admittedly naive assumption (I will come back to this in section 5). Science journalists operate within the same framework as the rest of the press, where one of the highest prizes consists in breaking the story first.

As the output of news stories progressed, I gained a better idea of what journalists were likely or unlikely to include in their stories, which assumptions I should or should not make, and things that I should probably explain better or emphasize. For instance, as I mentioned, we should probably have mentioned in [4] that the big rip was not an original idea. However, one of the journalists I spoke with (see remark 4.1 below) did point out the earlier origins of the big rip, even though I had not told him so nor had it appeared in the Vanderbilt news story. This gave me the wrong impression that other journalists would act in a similar way. Only after subsequent news stories, written by journalists I had also talked to, appeared, did I realize that this was a bad call, and I should always inform them that the notion of the big rip is more than a decade old.

Remark 4.1. I am purposely avoiding reference to specific news stories in this discussion. Thus, when I say, for instance, that one journal wrote X, whereas another one did not, I trust that the interested reader can quickly find out, from the reference I mentioned and/or from a quick internet search, which news stories did what. I am doing so because, as I said in the introduction, it is not my intention to pass judgment, classifying this or that story as good or bad. If anything, this short interaction with the media has reminded me of how distant academia and the press are, and how little I know about the particular hurdles of the journalistic profession. Thus, I am probably not the right person to make strong statements about the quality of news coverage of scientific research.

I steadily learned that I should point out that the concept of a big rip was not our creation; that our model was one among many, and physicists come up with models all the time; that there were two aspects behind the genesis of our our paper, the part concerned with fluids in relativity, and its application to cosmology; that it misleading to consider me the person who "led the team;" that the formulation of relativistic fluids we used was not "my theory," but rather a new cosmological model based on erlier work done by Lichnerowicz's; that while the big rip is mathematically described by

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[11] I stress something that I have already mentioned in the introduction, namely, that the description that follows is my own experience. Naturally, other people might have had entirely different interactions with science journalists.

the rate of expansion becoming infinite, the appearance of infinities in the equations is usually a hint that we are missing a part of the puzzle. Also, when someone asked if it was possible to prove that "my theory" was right, I was quick to point out that in science we never "really prove things," that this was definitely the type of question beyond my area of expertise, and that an experimental physicists would be able to provide a better answer.

In other words, I did not have a scripted version of what to say to the media and had to learn as things progressed. Here, again, I learned from the advantage of hindsight, as I could fine tune my explanations after seeing previous news stories that I thought could be improved. In any case, as I mentioned, I had ultimately no control over the texts written by the journalists who covered our paper, having no access to a draft of the text prior to its publication. This had some interesting consequences. For instance, while some news outlets correctly quoted me saying that

> "My result by no means settles the question of what the correct formulation of relativistic viscous fluids is. What it shows is that, under some assumptions, the equations put forward by Lichnerowicz have solutions and the solutions do not predict faster-than-light signals. But we still dont know if these results remain valid under the most general situations relevant to physics,"

others have chosen to ignore this and similar statements that I made intended to highlight how little we know about some of the things addressed in our paper and, therefore, the necessity of more research. I can only speculate about how science journalists are making their decisions. I share some of these speculations in section 5.

I have so far described what our work was about and how the media coverage took place. I have not yet address a basic question, namely, why was our work given so much media attention?

The honest answer is that I do not know. As I mentioned, I do think that our result is interesting, but as I also tried to convey in the above text, I would be pretentious to claim that it is revolutionary or that it answers longstanding and deep unknown questions. Certainly the appearance of the Vanderbilt news article kicked off the avalanche of media coverage. But it cannot be a sufficient condition, as many other science news stories also also appear in research news on Vanderbilt's website without the same impact on the media. I suspect that the coverage of redorbit.com may also have played a major role, and I suppose that Redorbit picked up the story from the Vanderbilt website. Although I confess that I had never heard of Redorbit until their journalist contacted me, it is my impression, after some internet search, that it is a major news outlet for science. Thus, it is natural that other journals and magazines found out about the story. But this also, in itself, does not completely explain the widespread media interest, since Redorbit carries other science news. Then, there is also the conclusion of the big rip, which seems to be something that captures the public's imagination. However, as people who check arXiv [6] daily know, papers with this type of fascinating conclusion appear quite often, so general interest about the big rip cannot, by itself, be the cause of the wide media attention.

What is it, then? It is likely that it is a combination of all the above and other factors that I am not even aware of. And, finally, there is also an element of luck. I do not like the word "luck," and generally I would rather use something like "random," but I think it will not make a difference here, and I hope that the reader understands that I am not subscribing to any kind of superstitious thinking.

5. Some final thoughts

Here I would like to share some ideas about science news. They are not so much related to the specific coverage of our paper, but rather they are general questions that I raised guided by my recent experience with science journalists.

I often hear complaints from colleagues that science journalism is usually of poor quality. This may or may not be true, and I honestly do not know the answer (see remark 4.1). In fact, I do not even know what type of criteria one would use to make such judgments. Instead, let me come to some related, but simpler problem, which does not explicitly state that media coverage of science is good or bad, but rather focuses on specific flaws found in the science news, flaws that could potentially be mitigated.

Every so often I hear colleagues in academia asking things like "why did magazine X not seek a second opinion about that paper?" or "why has the author not explained Y to the journalist who wrote the piece?" when it comes to the press coverage of science.

I do not know the answer to these questions. But I would like to point out that, if the description I gave above of how the science media covered our work is typical of the science news press, then I think it is fair to say that the time pressure puts tremendous constraints on how much an article can be revised, shown to third parties, etc., before it is finally published. It also puts constraints on the length of such news stories, making it hard for long and detailed articles can be produced in a timely fashion.

It could be argued that these are self-imposed constraints, as in principle the science journalist could take the time to write a long, well-explained piece, requesting feedback from a group of specialists before finally publishing the final story. There are, after all, long and detailed news pieces on scientific research that probably follow some of these criteria. One should keep in mind that a story about a recent scientific discovery is different than, say, press coverage of the recent U.S. Supreme Court decision on gay marriage, which should be reported soon after the fact. So why the rush and the demand for concision in science news?

These are, of course, hard questions that ought to be addressed by those who study the dynamics of science media. Stressing, once more, that I am not a specialist on media, I hope I would still be welcome to share my thoughts based on my recent experience.

Perhaps the demand for speed and concision is self-imposed, but, if so, I believe that it is an editorial decision rather than an individual one. I imagine a situation where a journalist of magazine X decides to write a long and detailed piece on a newly announced scientific discovery. He or she spends a few weeks writing it, talking to a wide range of specialists, etc. Meanwhile, magazine Y, direct competitor of X, immediately publishes a short, and perhaps inaccurate account of the discovery in question. Would we be surprise in hearing that the journalist of magazine X got fired for missing the story, while Y broke it? The journalist of magazine X would be shielded if a conscious editorial decision had been made stating that X would not engage in this type of fast paced and ultimately superficial news coverage of science. In fact, I believe that it is because of such types of decisions that we can find the sort of long and detailed articles I previously referred to. However, it is my impression (and this is indeed just an impression; it would be interesting to have some hard data on this) that such detailed science news articles usually address some ongoing research topic rather than a recently announced breakthrough. Think, for example, of the difference between a well-elaborated news story featuring the long battle to fight cancer versus a story covering some announcement that a definitive cure for cancer has been found.

We should not lose sight of the fact that those making editorial decisions about science journalism are usually subject to the same pressures that regular news organizations are. So there is a reward for breaking the story first and a punishment for being blindsided. This, of course, raises the question of how much the demand for speed and concision could be classified as "self-imposed," even at the level of editorial decisions. In the age of the internet, we ought to ask whether it is possible for newspapers and magazines to avoid, without the risk of being driven out of business, this sort of instant and fast pace news coverage even when it comes to science.

Another point to keep in mind is what the public is looking for in science coverage. Sure, scientists themselves probably would like to see long and detailed articles in the science section of their favorite newspaper. But is that what the rest of the public wants? I am not going to recite the mantra of some journalists who write sensationalist stories, i.e., that they simply write “what the public wants.” Given the powerful tools of marketing that we are aware of, I think it is naive to ignore the mechanism by which news outlets “shape” their readers. On the other hand, I also think it is just too easy to explain away the supposed lack of deep science news coverage by putting all the blame on greedy editors that only want sensationalist stories. The dynamics between all these elements is complex, and one that I admittedly do not understand. Being aware of it, however, is important to avoid simplistic explanations.

I would also like to point out what seems to be an implicit assumption among many scientists, namely, that science journalism has to depict scientific discoveries as accurately as possible. I certainly would like to believe that when science is described to the general public one has to strive to make it as precise as possible. The problem of how to walk the fine line between scientific accuracy and public understanding is another complex one that I am not prepared to discuss. Instead, I would like to call the assumption that science journalism has to depict scientific discoveries as accurately as possible into question.

I started thinking about this after one of the journalists I spoke with told me that they wanted to ask me about our paper because they thought this would be a good story to “spice up the imagination.” That kept me thinking. It was not mentioned that they wanted to educate the public, or make people understand what our work was about. They wanted, simply put, to spice up the imagination. What if that is what science journalists are primarily trying to do? And if so, is that “wrong”?

One could argue that it is more important to fascinate the public about scientific discoveries, even if they are not portrayed accurately, as this will raise interest to science. But one could also argue that this will misrepresent the rigor of the scientific process, ultimately diminishing the public’s trust in science. This is, of course, an old and long debate to which I would have very little to add. But I am curious as to how much data there is addressing these questions, since they are, ultimately, empirical questions. Of course, it would be impossible to run a controlled experiment to sort this out empirically, and I am not sure how one would measure, for instance, public support for science. But this is also true for most matters in society, and scientist still try their best to collect (imperfect) data. While I am not myself aware of any hard data addressing these questions, I am very curious about them.

To finalize, let me point out that my personal view is that science journalism should do (much) more than just spice up the public’s imagination. But I also think that scientists should be aware that not everyone agrees, and that perhaps much of the confusion that occurs between scientists and journalists is due to a mutual lack of understanding of each other’s goals and implicit assumptions.

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