Epistemic Injustice in Mathematics

Colin Jakob Rittberg, Fenner Stanley Tanswell, Jean Paul Van Bendegem

We investigate how *epistemic injustice* can manifest itself in mathematical practices. We do this as both a social epistemological and virtue-theoretic investigation of mathematical practices. We delineate the concept both positively – we show that a certain type of folk theorem can be a source of epistemic injustice in mathematics – and negatively by exploring cases where the obstacles to participation in a mathematical practice do not amount to epistemic injustice. Having explored what epistemic injustice in mathematics can amount to, we use the concept to highlight a potential danger of intellectual enculturation.

1. Introduction

Desirable mathematics extends beyond the confines of correct calculations. Desirable mathematics has to do with the behaviour of the building blocks of mathematics: we value *deep* theorems, *beautiful* proofs, *fruitful* theories, and so on.[1] Because mathematics is done by human agents, desirable mathematics also has to do with the ways in which mathematicians perform their craft: we value traits such as creativity, meticulousness or caution in mathematicians. In this paper, we focus on this human role in desirable mathematics.

Argumentation theorists have fruitfully applied the virtue terminology in their effort to understand the role of the human agent in successful arguments.[2] It has become clear that virtues and vices[3] allow them to access what is praiseworthy in an agent without demanding such traits or attitudes in terms of criteria to be fulfilled.[4] Virtue terminology thus allows for a normative assessment that is sensitive to the vagaries of our epistemic life.

Some philosophers of mathematics have already started using virtue terminology in their works.[5] These works amount to a preliminary exploration of virtues in mathematics. The aretaic turn currently seen in argumentation theory, epistemology, political theory, and other fields has thus far not reached the philosophy of mathematics. Part of the aim of this paper is to take a further step in this direction.

The role of the human agent in successful mathematical practices can be illuminated by exploring parts of mathematical practices that are dysfunctional in some way.[6] In this paper, we focus on parts of mathematical practices that are unjust along an epistemic dimension.

To investigate how epistemic injustice can manifest itself in mathematics, we first provide a brief overview of how the term has been used in the literature on epistemic injustice (Section 2). We then discuss a specific type of folk theorem we call “ghost theorems” and argue that they can be a source of epistemic injustice (Section 3). Having positively delineated what epistemic injustice can

[1] See e.g. Maddy (2011, 2017) for *depth* and *fruitfulness* and Rota (1997) for *beauty*.
[2] An excellent overview of the relevant literature is provided in (Aberdein 2014).
[3] For a study of epistemic vices, see e.g. (Cassam, 2016), (Kidd, 2016a).
[4] A similar point was made by Kuhn (1977), pp. 320 – 339.
[5] E.g. (Berry, 2016), (Decock, 2002), (Harris, 2015), (Lange, 2016), (Maddy, 2011), (Tanswell 2016).
[6] A point already made in Andrew Aberdein’s “Observations on Sick Mathematics”; (Aberdein, 2010).

amount to in a mathematical practice, we further our understanding of the concept by providing a negative account thereof: we present the case of Thomas Royen and argue that this is not a case of epistemic injustice (Section 4). We then discuss the case of Srinivasa Ramanujan to round out our understanding of epistemic injustice by contrasting it with the notion of epistemic awe (Section 5). In the Ramanujan case, the term "enculturation" already vibrates in the subtext. In a following section we bring out the term more explicitly and show how enculturation can lead to cases of epistemic injustice (Section 6).

2. Epistemic Injustice

The concept of *epistemic injustice*, explored in Miranda Fricker's 2007 book, picks out cases of injustice along a specifically epistemic dimension. Epistemic injustice can take on multiple different forms, of which Fricker herself concentrates on *testimonial* and *hermeneutical injustice*. However, recent work such as the excellent handbook on the topic (Kidd and Pohlhaus, 2017), has considerably expanded the scope of relevant cases and ways in which epistemic injustice can manifest itself across different situations. In this section, we briefly outline what epistemic injustice is and how it can manifest itself in joint epistemic endeavours, such as mathematics.

Epistemic injustice is a kind of injustice that is done to people in their capacity as epistemic agents and as knowing subjects. There are many ways that this can come about. For example, Fricker points out that a distributive injustice in access to education and other epistemic resources is an injustice that unfairly limits the knowledge of certain groups of people and might thus be called an epistemic injustice. While this is true, it is not particularly distinct from other distributive injustices, say of healthcare provisions or legal aid. The initially interesting cases, according to Fricker, are the cases where the injustice is of a distinctively epistemic variety. She focuses her book on two such distinctively epistemic forms of injustice:

- *Testimonial injustice*: "wherein a speaker receives an unfair deficit of credibility from a hearer owing to prejudice on the hearer's part" (Fricker 2007, p. 9).
- *Hermeneutical injustice*: "wherein someone has a significant area of their social experience obscured from understanding owing to prejudicial flaws in shared resources for social interpretation." (Fricker 2007, p. 148)

The first of these, *testimonial injustice*, picks out an imbalance in whom we choose to trust when they provide us with information via testimony; an imbalance due to prejudice by the hearer against the speaker because of the speaker's social identity. For example, if a doctor refuses to take seriously a female patient's medical complaints because of the prejudiced belief that women whinge more or have lower pain thresholds, then this is an instance of testimonial injustice.[7] It is important to ask, then, in what way this has been an injustice in a specifically *epistemic* sense; after all, the patient still retains the knowledge that they had going in to the doctor's surgery. The answer to this lies in Fricker's emphasis on the "economy of credibility". Recent moves towards *social*

[7] Indeed, this is a possible explanation for numerous cases where the diagnosis rates differ between sexes for some condition. For example, *cluster headaches* are far more commonly diagnosed for men than women, but there are indicators that the actual incidence rates are roughly equal, with female sufferers often diagnosed with migraines instead, which are much less severe. Testimonial injustice in healthcare generally is discussed in (Carel and Kidd 2017, p. 338).

epistemology have demonstrated that we must not ignore the social aspects of knowledge and the practices these relate to.[8] One sort of epistemic practice is about testimony, and how we assess the reliability of the information gained by others. The economy of credibility is about the distributions involved in whom we choose to trust or not. A fair distribution would trust others based on fairly assessing reasonable measures such as their expertise, their history of reliability, the particular information under discussion, and their access to it, etc. An unfair or unjust distribution may come about by undervaluing or overvaluing somebody's trustworthiness based on prejudice. The sense in which this is a distinctively epistemic injustice is therefore not in denying them knowledge, but in denying them the appropriate level of credibility in a social setting of knowledge exchange. For Fricker, knowledge is social in that it is a core part of being an epistemic agent that one can communicate one's knowledge, and so being disregarded is not merely incidental to being a knower.

Fricker's second kind of epistemic injustice, *hermeneutical injustice*, concerns the unfair lack of shared conceptual resources and how this can present a barrier to understanding social experience or being understood. For example, Fricker investigates the power of concepts such as *postpartum depression* and *sexual harassment* to identify and understand social experiences, where such concepts had been lacking due to prejudicial flaws in the earlier shared conceptual resources (Fricker 2007, pp. 147 – 152). For a case more closely connected to the current paper, recently Origgi and Ciranna (2017) have argued that another form of hermeneutical injustice is the lack of concepts in our online lives to identify our "predictive online statistical double", which is the online version of ourselves built by algorithmic data-profiling. Many uses of big data build in large amounts of prejudice and bias under the guise of mathematical objectivity, as is systematically demonstrated in (O'Neil 2016). There seems to be further scope for analysing this impact of mathematics in the real and digital worlds in terms of hermeneutical injustice, but the remainder of our paper will not deal with these analyses and so we shall set hermeneutical injustice aside.

In a response piece to Fricker, Hookway opens up the range of potential cases of epistemic injustice more broadly than Fricker's two main kinds (see Hookway 2010). He argues that both Fricker's cases fall primarily into an "informational perspective" on which we evaluate epistemic agents in terms of their ability to give and receive information, as emphasised by the economy of credibility. On that perspective, we primarily concentrate on epistemic agents' roles as givers and receivers of testimony and "we encounter people making assertions, recognizing (or failing to recognize) other people's authority to make assertions, and so on" (Hookway 2010, p. 156). However, epistemic practices are not limited to the informational perspective. Hookway gives the alternative "participant perspective", which is about whether an agent is able to be a proper participant in some epistemic endeavour, including the ability "to participate in activities such as discussion, inquiry, deliberation, and so on […] [For example,] asking questions, floating ideas, considering alternative possibilities, and so on." (Hookway 2010, p. 155). To illustrate, if a medical doctor gives a diagnosis that is not trusted without a second opinion due to a patient's prejudices, then this is an epistemic injustice on the informational perspective, as the patient undervalues the doctor's testimony. If that doctor is excluded from professional conferences or left off from authorship lists for similar reasons, then this is a participatory epistemic injustice, excluding the doctor from participating in an epistemic community.[9] Our lives as epistemic agents involve both testimonial

[8] Building on the growing literature in social epistemology; e.g. (Goldman 1999), (Lackey 2008), (Haddock et al., 2010).

[9] Note that Fricker distinguishes between *incidental* injustices, which do not render the subject vulnerable to other kinds of injustices (legal, economic, political, sexual, etc.) and *systematic* injustices that do; cf. (Fricker 2007, 27). According to her, "The importance of systematicity is simply that if a testimonial injustice is not systematic, then it is not central from the point of view of an interest in the broad pattern of social justice" (ibid., 29). We will argue that a certain type of folk theorem is a potential source of epistemic injustice in mathematics. Because this injustice does not necessarily render its subjects vulnerable to other forms of injustice in the relevant way, it may be incidental in Fricker's sense. Whether or not these will then contribute to further injustices will depend on the

exchanges and many other participatory elements.

With the participant perspective in hand, it is clear that there can be other associated forms of epistemic injustice wherein our ability to be competent and respected participants in certain epistemic activities is undermined due to prejudice, unfairness or inequality. For example, unfair barriers to becoming a participant in an epistemic community, or the failure of others to treat you as a part of the community, can constitute an epistemic injustice. Indeed, Hookway says as much: "injustice can be manifested in obstacles to someone's ability to engage in practices that are constitutive of activities that are distinctively epistemic" (Hookway 2010, p. 155). While it may be that epistemic injustice makes it harder to be a proper participant in certain activities, Hookway also points out that from the participant perspective things take on a negative feedback loop by which, if you are not accepted by the epistemic community as a participant, it then undermines your epistemic confidence and thereby makes you less likely to be made part of the epistemic community later, leading to a form of *epistemic silencing*.

Such barriers to participation can come about from individual prejudices, systematic and structural inequalities, cultural problems, or some combination of these. The kind of barriers that can arise will differ depending on the kind of epistemic domain under consideration. In this paper, we will be looking at the case of mathematics and presenting certain mechanisms by which researchers and students may fail to be accepted as full members of a mathematical community. We will assess these to show how some of them are cases of epistemic injustice.

In her recent paper, Heidi Grasswick (2017) examines epistemic injustice in scientific practices and thereby provides a close parallel to our own investigation in several respects. For example, Grasswick emphasises that science is a prototypical case of cooperative inquiry, with well-known works in the sociology of science like (Latour and Woolgar 1979) mapping the dynamics of social interactions and how these affect scientific outcomes. Grasswick argues that "the centrality of such social elements to the core activities of science make participatory epistemic injustices highly relevant to understanding scientific practices and the ways in which oppression can be implicated in them" (Grasswick 2017, p. 316). Ethnographic work reminiscent of Latour and Woolgar has recently been carried out by Lorenzo Lane in (Lane 2017). Lane observes mathematical collaboration and gives insights into the deeply social nature of mathematical research. As such, we may expect that similar participatory injustices could be found in mathematical practices; indeed, in the next section, we show how a specific form of folk theorem can be a source of epistemic injustice in mathematics. Of course, there are similarities and differences between science and mathematics in practice. Both share an under-representation of women and minorities (especially historically) that can clearly be traced to systematic inequalities. Mathematics fortunately does not have a history of discriminatory human testing, which forms a main part of Grasswick's discussion about scientific practices. Nonetheless, our conclusion for mathematics will echo Grasswick's that "Members of underrepresented groups who have managed to 'make it' into the formal scientific community are not necessarily always treated with the same respect and granted the same cognitive authority as other similarly talented members of the community" (Grasswick 2017, p. 317).

Before we end the discussion here of epistemic injustice, it is important to note that the work described above is part of the literature on *virtue epistemology*.[10] Epistemic injustice is treated by Fricker as an epistemic vice, also explored in detail in (Battaly 2017). Fricker's suggestion is to

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specific cases. For example, they may contribute to systematic discriminatory practices in mathematics, or they may be incidental. Nonetheless, we submit, understanding the kind of injustices that may arise from folk theorems is relevant to an understanding and betterment of mathematical practices. Our interest in a specific nexus of epistemic practices (mathematics) is mirrored in the interest in epistemic injustices as they occur in other scientific practices as discussed in (Kidd and Pohlhaus, 2017). We are indebted to Alessandra Tanesini for pushing us on this point.

[10] See (Sosa 1980) for beginnings, see (Zagzebski 1996) for a fundamental contribution.

combat epistemic injustice with the epistemic virtue of epistemic justice, which involves a critical self-reflection on the possibility for biases and prejudice. We see this paper as part of a broader assessment of virtues and vices in mathematical practices and believe that identifying and combating cases of epistemic injustice is a direct advantage of applying the virtue framework to mathematics and mathematical practices.

3. Ghost Theorems

There are mathematical results which are taken as accepted in a mathematical community, relied upon in talks, discussion, and proving further results, but which cannot be traced to a concrete proof in the literature. These results are part of the expert knowledge one is expected to have in certain communities and we will present examples below. What name to give to such results?

The name such results are given in the examples we will present below is "folk theorem". Harel (1980) identifies three properties of folklore results in mathematics: popularity, anonymous authorship, and age.[11] He writes

> It is rarely the case that no one knows with whom the theorem, or its rigorously stated various versions, originated. Rather, what tends to happen is that the effort involved in tracing back such a theorem is far greater than that involved in reproving it… The roots of a folk theorem might be buried in some obscure lecture notes, in a letter to an editor, or worse, in "private communication". (Harel 1980, 380)

For mathematicians, the common expression of this is that a theorem is a folk theorem if the author of the original proof is lost in history.

Our focus in this section is on mathematical results whose concrete proofs cannot be traced back to the literature. In light of the above, not all folk theorems are of this nature. Nonetheless, the mathematicians we will meet when discussing the indicative story of Olivia Caramello call these non-traceable results folk theorems - notice the alignment with Harel's considerations of the buried roots of some folk theorems. The kind of result we wish to discuss, i.e. the kind of result whose proof cannot be traced in the literature, thus seems to be a special kind of folk theorem for which we propose the term "ghost theorem"; these theorems are immaterial in the sense that they are not proven in the literature yet they "haunt" parts of daily mathematical life.

We will argue that ghost theorems can lead to epistemic injustice. To do this we will present the indicative story of Olivia Caramello. Two methodological issues deserve to be stressed here.[12] First, we do not pass judgment on whether Caramello suffered epistemic injustice. As we explain in more detail below, we refrain from doing so because there may be relevant information for such a judgement which is inaccessible to us. Second, the Caramello case is anecdotal evidence and thus cannot reveal a systematic failure in mathematical practices. It can, however, function as an indicative story: it raises our awareness for how epistemic injustice can manifest itself in

[11] Besides Harel (1980), there are few case studies of folk theorems in the literature. We are thankful to an anonymous referee for pointing out Lassez et. al (1982) as a further example.
[12] We thank an anonymous referee for encouraging us to be clearer on these matters.

mathematical practices, thereby guiding our critical thinking.[13] Thus, the specifics of the Caramello case indicate what can be relevant to consider when tracking epistemic injustice in mathematics. This will be helpful in operationalising the relevant concepts for further rigorous empirical study of the phenomena. However, our argument that ghost theorems are a potential source of epistemic injustice in mathematics does not rely on how one judges the Caramello case; our argument rests on purely normative grounds.

We start our discussion by considering some features that are good about ghost theorems. We get at this from some more general considerations about mathematical proof.

Proofs can have desirable features such as depth, elegance or simplicity. Some proofs employ new methods that are applicable in other cases as well. Some proofs are short and concise. Some proofs have all these features. Such proofs are worthy to be written down and published because they embody some of the virtues of mathematical proofs.

Other proofs embody some of the vices of mathematical proofs. They may be straightforward, lengthy, employ complicated notation or rely on well-known methods that are used in a standard way. None of these vices alone suffices as an argument against publication. Many proofs in the literature are straightforward but short enough for publication.[14] Some proofs are long yet very deep and elegant.[15] Some proofs require complicated notation. And many (if not most) proofs employ well-known methods in standard ways; this is what makes these methods so well-known.

Proofs can also fail the values of their target audience. A proof of a theorem in topology may very well be rejected by a journal dedicated to functional analysis. A publisher of teaching material may judge the straightforwardness of a proof differently than a jury of experts.

When a proof embodies few or none of these virtues, fails to meet the values of its target audience, and embodies many of the vices, we may reasonably question whether it is worthy of publication.[16] Assume that the proof has the vice of being straightforward (for its target audience) and is also lengthy. Should it still be given in the literature? Perhaps we can reasonably claim at this point that the proof is “too easy” or simply “too boring” to be given. Both writing and reading it would be time consuming with minimal epistemic pay-off. In this case it may be reasonable to omit the proof and simply state that the theorem, for which only a non-virtuous proof could be supplied, is true.[17]

The journey of this (fictional) theorem with the omitted non-virtuous proof now continues. The experts of the relevant field take notice of the theorem and use it in their own work. Some may even write the proof down for themselves but because the proof fails to have the virtues one expects of publication-worthy proofs whilst having some of the vices, the proof remains unpublished. The theorem has turned into a ghost theorem.

Some ghost theorems can serve as good exercises for students. The experts may deem a proof straightforward but in giving it to students the target audience changes; for them the degree of complexity may be just right. Students need to learn how to wield lengthy proofs, complicated notations, and so on. Students not only need to learn about the virtues of their field but also of its vices to function properly in the expert community they aspire to join.

[13] For a discussion of the intertwined nature of case studies and our philosophical views, see also (Chang 2011).
[14] (De Cezaro and Johansson 2012) includes an example.
[15] See the seminal (Inglis and Aberdein 2014) for an argument that beauty in mathematics is not simplicity.
[16] There are other reasons why a proof may not be published. For example, Thurston (1994) suggests that time-constraints on the mathematician may interfere with write-up and hence publication. This point will be discussed below.
[17] This happens for example in (Robertson et al. 1997).

Ghost theorems foster the cohesion amongst experts in a field. These experts know the ghost theorems of their trade. The ghost theorems unite them by forming part of the knowledge of the community. This is not unique to ghost theorems; all algebraists know the fundamental theorem of algebra. The theorem and its proof can be learned from any introductory book on algebra. This is not true for ghost theorems. A ghost theorem might be learned either by discovering it oneself, by being told about it by someone or by observing a reference to it. It is in many cases more difficult to know a ghost theorem than to know a "normal" theorem. Ghost theorems can thus function as a certain standard: those who know them are in, those who do not are out. This conjures up the image of an exclusive club whose members fiercely guard their secret knowledge. We will explore this thought below. Note here that ghost theorems do not function as a clear demarcation between experts and non-experts; one does not have to know all the ghost theorems of descriptive set theory to be a descriptive set theorist. There will be shades and hues. Those mathematicians with the appropriate shading can rely on each other to have the kind of unpublished and thus somewhat hidden knowledge ghost theorems transport. It can facilitate communication and form an additional layer on top of the common (because published) knowledge of the field. And this establishes cohesion.

Ghost theorems then are not a sign of laziness of mathematical communities. There are reasons why their proofs are omitted and not published. These proofs tend to be boring and reading and writing them might be a waste of time. Proofs of folk theorems in general, and ghost theorems in particular, can also be incorrect; see figure 1.

Abstract

For about twenty five years it was a kind of folk theorem that complex vector-fields defined on $\Omega \times \mathbb{R}_t$ (with Ω open set in $\mathbb{R}^n$) by

$$L_j = \frac{\partial}{\partial t_j} + i \frac{\partial \varphi}{\partial t_j}(\mathbf{t}) \, \frac{\partial}{\partial x} \ , \ j = 1, \ldots, n \ , \ \mathbf{t} \in \Omega, x \in \mathbb{R} \ ,$$

with φ analytic, were subelliptic as soon as they were hypoelliptic. This was the case when $n = 1$ but in the case $n > 1$, an inaccurate reading of the proof given by Maire (see also Trèves) of the hypoellipticity of such systems, under the condition that φ does not admit any local maximum or minimum (through a non standard subelliptic estimate), was supporting the belief for this folk theorem. Quite recently, J.L. Journé and J.M.Trépreau show by examples that there are very simple systems (with polynomial φ's) which were hypoelliptic but not subelliptic in the standard L^2-sense. So it is natural to ana-

Figure 1: Abstract on Ghost Theorems[18]

Mathematicians acquire a sense for what can and cannot be proved. This sense is grounded in experience and a familiarity with the field of study. It is, however, not infallible. Mathematicians can be led astray when evaluating the straightforwardness of a non-written proof. They may forget about important and difficult to deal with case distinctions, assume that they can rely on another theorem without verifying its premises, and so on. Writing down the proof can help because at this point the proof-writer may spot the mistake and can aim to correct it. A standard vetting procedure (colleagues, referees, etc.) is in place which serves as a safety-net for mistakes that get through the

[18] From (Derridj and Helffer 2010).

writer's self-checking.[19] This safety-net is not perfect; there are many known cases of published but incorrect proofs. Nonetheless, it is sufficiently reliable for mathematics as an epistemic practice.[20]

Now consider a ghost theorem again. Its proof may never have been written down. Even if it has been, the safety-net of the standard vetting process is missing because the proof was deemed unworthy of publication. Perhaps the ghost theorem was mentioned as a folk theorem in a paper and some other experts have, rather than saving time and skipping its proof as suggested by the term “folk theorem”, checked the proof for themselves – perhaps on paper, perhaps in their heads. This provides something like the safety-net of the standard vetting process, but it is much more loosely knit because fewer mathematicians have checked for errors.

The practice of ghost theorems comes with vices other than the weaker epistemic status relative to standard mathematical results. An omitted proof for a ghost theorem lacks what Easwaran has called *transferability*: “a proof must be such that a relevant expert will become convinced of the truth of the conclusion of the proof just by consideration of each of the steps in the proof.”[21] If there is no proof at all, then even experts cannot follow its steps. Transferability is needed in part, so Easwaran argues, to avoid reliance on authority. In the case of ghost theorems there may not be such reliance: the idea is that an expert could simply come up with a proof herself. Note however that the style of writing suggests not to do this. Flagging something as “straightforward but lengthy” does not inspire one to go through the steps oneself. Whilst experts need not rely on authority, the writing style aims to persuade them to do so. This problem gets worse when considering mathematicians working in different areas than the ghost theorem. They may have to trust the author because, even though they are expert mathematicians, they are not experts in the relevant field. These mathematicians must believe the ghost theorem based on testimony (or invest considerable time and effort to acquire the relevant capacities). Testimonial knowledge is not abnormal for mathematics: mathematicians rely on theorems whose proof they have not gone through themselves, as Geist et al. (2010) show, and mathematical papers are not reviewed line-by-line; see (Andersen, 2017). Whilst the lived reality of mathematical practices introduces a testimonial basis for mathematical knowledge here, such a basis could, in principle, be avoided because the relevant knowledge is written down (just not read). This is not so for the omitted proofs of ghost theorems. Easwaran argues that a non-testimonial basis for mathematical knowledge is desirable for mathematics. In light of Geist et al. and Andersen we may refine Easwaran’s considerations to a desirability of a principally avoidable testimonial basis for mathematical knowledge. Because ghost theorems score poorly on these points, the lack of transferability of their omitted proofs is a vice of the practice of using ghost theorems.

Omitting proofs for ghost theorems also lacks the virtues associated with the proving activity such as transparency, rigour, diligence or meticulousness. The proving activity going on in print in the case of ghost theorems is reduced to dismissive words such as “lengthy” or, as if to sum up, “folkloric”. Such arguments display argumentative vices such as quietism, intellectual callousness or intellectual rashness. Especially intellectual callousness, i.e., being insensitive to and disregarding of others, will play a role in what follows.

Ghost theorems can act as a barrier to participation in a mathematical practice because if something is considered to be widely known it may not be considered original enough to warrant publication. We discuss one such case in some detail: the case of Olivia Caramello.

[19] See (De Millo et. al, 1979).
[20] See (Andersen 2017).
[21] (Easwaran 2009, 343). Indeed, Easwaran calls this a *social virtue* in mathematics, which we endorse as part of a virtue-based perspective on mathematics.

Caramello specialises in topos theory. She started and is pursuing a unification programme which, as we will see in what follows, struggled to have the necessary traction with some of the senior experts in topos theory for the smooth birth of a mathematical programme. Caramello finished her post-doctoral work in 2017 and is now an assistant professor at the University of Insubria in Como (Italy). Some of her work and the results she obtained have been called "well-known" or "folklore".[22] She writes about these experiences in her blog.[23] This makes her side of the story accessible to our philosophical investigation. The other side of the story, the side of the other experts of the field, is not accessible in this way. Caramello has published some of their letters on her web page. To our knowledge there is no further written account of the events that took place or arguments as to why Caramello's work has been dismissed as non-original. This makes our account of the events incomplete. Our evidence does not allow us to move away from a one-sided story-telling which is insufficient to argue that the Caramello case is a case of epistemic injustice.[24] Nonetheless, the Caramello case strongly suggests that epistemic injustice can occur in mathematics; it can serve as an indicative story. To identify how epistemic injustice can manifest itself in a mathematical practice is our aim. Whether or not Caramello actually suffered epistemic injustice is a secondary question which will find no answer in this paper.

Caramello writes:

> […] it has emerged that indeed the word had been spread around that I prove "well-known" or "folklore" results (with all the imaginable negative consequences that this naturally entails), without nevertheless there being any proof whatsoever supporting such claims. Indeed, **none of the contacted experts was able to provide a single reference containing a proof or a statement of a result that I attributed to myself but which had been proved before, nor anyone showed that any of my results could be deduced from previously existing results in an essentially straightforward way.**[25]

Note that the emphasised part in this quotation clearly marks the kind of folk theorem Caramello is talking about as the kind we have labelled "ghost theorem".

If Caramello is right, then epistemic injustice manifests itself here in the obstacles to her ability to engage in a specifically epistemic practice; namely, contributing to the body of mathematical knowledge. We start our investigation of the matter by engaging with an objection to the claim that injustice manifested in the Caramello case.

It is part of any academic practice that there are obstacles on the road to dissemination of one's work. These obstacles can be some academic standards, the character of a journal, the whims of conference organisers or otherwise. Obstacles on the road to dissemination are simply part of the game. And this is also true for mathematical practices. Any journal or conference is in principle allowed to reject any contribution. Because there are enough journals and conferences out there, this is not a problem. The argument then is that Caramello did not suffer epistemic injustice. Rather, she played the game and lost.

[22] Such rejections occur not only in Caramello's case. Consider Harel (1980), writing for a mathematical audience: "We would like the reader to recall the last time he received a referee's report in which the referee dismissed his latest achievement as 'a piece of folklore that has been around for ten years'" (p. 379).

[23] http://www.oliviacaramello.com/

[24] Caramello read a draft version of this section and offered some constructive criticism in private communication. According to her, the accessible evidence for the case under discussion is not incomplete: those voices that are not present on Caramello's web page have chosen to remain silent.

[25] http://www.oliviacaramello.com/Unication/InitiativeOfClaricationResults.html; accessed January 2018, (emphasis in original)

The above argument assumes that journals and conferences may reject submissions for any reason. This is not so. We would be appalled if a journal would reject a submission due to its author's colour of skin or gender. Some obstacles to dissemination are part of a well-functioning mathematical practice, others are not.[26] The question becomes whether the obstacles Caramello encountered were of the first or of the second nature.

Caramello is a woman and the thought that she might have been a victim of discrimination on the basis of her gender arises. The evidence we have examined for this paper does not immediately suggest that this is the case, nor have we been looking for evidence that would support such claims. Further study may reveal that there is evidence for such claims.[27] In this paper, however, we will put the gender-question aside. There is another dimension on which Caramello may have suffered epistemic injustice. This dimension is our focus.

Caramello's work is satisfactory on a technical level; her work is not rejected because it contains logical flaws. The presentation of her work meets the standards of the community. Peter Johnstone, an expert in topos theory and Caramello's former PhD supervisor,[28] considers her to be an "extremely talented mathematician"[29] and attests when asked about Caramello's contribution to a conference "I don't think you made any scientific mistakes."[30] It seems that Caramello plays by the most obvious rules of the game, yet still her work is rejected.

Work may be rejected because it is not of sufficient interest to the mathematical community. Caramello's results contribute to her unification project and this lends importance to her results for all who value this programme. The unification programme offers a view on the field which differs from more established views. Mathematicians committed to these more established views may find Caramello's results of less interest because the results "only" add to a project these mathematicians judge unlikely to succeed; what counts as interesting enough to publish might be a matter of perspective. However, notice that Caramello's results were not dismissed because they are uninteresting but because they are non-original.

The demand for originality is good academic practice because it promotes progress and tames stagnation. The originality of a work must be judged on the thoughts contained in the work; changing some xs to ys in a mathematical paper does not make for original work. These thoughts must be measured against the existing body of knowledge of the relevant field(s). This measuring is done by human agents and they can do so both virtuously and viciously.

There is a question as to what the "existing body of knowledge of a field" amounts to. Some may consider a theorem which is a special case of a well-known more general theorem as part of the existing body of knowledge. This is warranted because the special case theorem does not add to the knowledge of the experts of the field. Applying the special case theorem in a surprising and fruitful fashion however merits publication. What counts as the "existing body of knowledge of a field" is thus partly dependent on the value judgements of the agent in question.

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[26] This theme will be engaged with in more detail in the following sections.

[27] In private communication, Caramello attests that she did not feel gender was at the root of the hostility to her work.

[28] As Caramello reports, the relationship between her and Johnstone has deteriorated: "I was a very respectful Ph.D. student who greatly admired her Ph.D. supervisor… I still cannot understand why he has chosen to betray the trust that I had in him"; http://www.oliviacaramello.com/Unification/InitiativeOfClarificationResults.html#AttitudeTwoCambridgeProfessors

[29] http://www.oliviacaramello.com/Unification/InitiativeOfClarificationResults.html#AttitudeTwoCambridgeProfessors

[30] http://www.oliviacaramello.com/Unification/JohnstoneResponses.html , the conference was the *International Category Theory Conference 2010*.

Steve Awodey, a veteran champion of topos theory, tells us about the problem of existing knowledge in his field:

> some of the experts know a lot more than they have published. Also, there are some people in the field who will easily know almost anything that you can come up with, even if they have not thought of it before, and so you will not get the credit or due respect for even previously unpublished results.[31]

This starts to sound like the exclusive club of secret knowledge keepers mentioned above. Here we have a group of experts who are in positions to shape to a large degree what the "existing body of knowledge" is. They do so based on what they know. Part of what they know is unpublished, the "secret knowledge" that the members of the exclusive club are guarding in the metaphor. Other parts of what the experts attribute to the "existing body of knowledge" are, according to Awodey, pieces they have never thought of but (theoretically?) could have. We get a picture in which the "existing body of knowledge" contains both secret knowledge and possible knowledge.

Caramello's conception of what the "existing body of knowledge" of topos theory is, is different from that of her referees. She probably has some of the secret knowledge (she knows some ghost theorems), but she does not have all of it (otherwise she would not be surprised when her work is judged folkloric). Caramello cannot include the secret and possible knowledge of the more senior experts in her conception of the "existing body of knowledge" of topos theory because she has no access to this kind of knowledge.

Some of Caramello's work is also neither comprised of straightforward deductions of a shared body of knowledge nor are these deductions trivial. Here is Andre Joyal to tell us this:

> Caramello's duality theorem is a result in the theory of classifying toposes. It states that the quotients of a geometric theory are in bijection with the subtoposes of the classifying topos of this theory. Although considered "folkloric" by some experts, the result does not appear in the literature. I had believed that one could directly deduce it from the theory of classifying toposes of Makkai and Reyes. It is only recently, in the context of a discussion with Caramello, Johnstone and Lafforgue, that the latter attracted my attention to an aspect of Caramello's proof which I had missed, namely that the category underlying the syntactic site of a geometric theory varies if one changes the axioms of the theory. Surprised by this observation, I tried to exhibit the "folkloric" proof that I thought I had of this theorem. With my great astonishment, it took me a night of work to construct a proof based on my knowledge of the subject, and the proof depended only partially on Makkai-Reyes' theory! It is true that Johnstone subsequently sketched a simpler proof based on a paper by Tierney. Nonetheless, I draw from this experience the following conclusions: (1) that I had misread Caramello's paper; (2) that the duality theorem may falsely appear straightforward; (3) that the result is non-trivial; (4) that the result is original, since it does not appear in the literature.[32]

To judge the originality of Caramello's work on the basis of a conception of the "existing body of knowledge" which comprises both secret and possible knowledge is intellectually callous. It disregards what Caramello could have reasonably known and thus applies unreasonable standards to her. Indeed, the claim that her work was unoriginal and folkloric (i.e., ghost-like) was incorrect, as

[31] From http://www.oliviacaramello.com/Unification/AwodeyResponse.html. Compare also Thurston (1994): "It was hard to find the time to write to keep up with what I could prove, and I built up a backlog", p. 173.

[32] From http://www.oliviacaramello.com/Unification/JoyalLetter.html

the proof was far more substantial than the folkloric conception of it. These unreasonable standards are obstacles to Caramello's participation in an epistemic practice. And that is to say that if Caramello's work was judged on the basis of secret and possible knowledge, then epistemic injustice manifests itself in the Caramello case. We engage with this premise in more detail below.[33]

Possible knowledge is a vicious standard for assessing the originality of a mathematical work. The assessor is in an epistemically advantageous position over the writer with no means for the writer to remedy this fact. Even if the assessor is not intellectually dishonest – it is easy to claim, "I could have done this myself" - the writer is still in a position in which her work is assessed according to standards which she cannot know. Such an assessor stresses the superiority of his epistemic status and the importance of his (possible) knowledge. This is the vice of intellectual arrogance. The assessor may also misidentify himself as a reliable authority. Recall that Joyal, after some consideration, realised that Caramello's proof was neither straightforward nor trivial. Misidentification of authorities as reliable is another epistemic vice.[34] Holding a writer to the standards of one's possible knowledge also displays a reluctance to accept something without protest.

Secret knowledge is a similarly vicious standard of assessment of originality. Much of what we said about possible knowledge as a standard of assessment also holds for secret knowledge as such a standard. Unlike with possible knowledge however, with secret knowledge the writer could, at least in principle, level the playing field and lift herself up to an epistemic position which is equal (in the relevant respects) to that of her assessor by obtaining the relevant secret knowledge.

Could Caramello have obtained the relevant secret knowledge? Our available evidence does not deliver conclusive answers here. We are no experts in topos theory and cannot judge whether results such as Caramello's duality theorem are sufficiently widely known that Caramello could have obtained them from somewhere else. Besides what Caramello has published on her web page, the experts of the field have not written on this matter (to our knowledge). Thus, we now depart from the question whether the specifics of the Caramello case instantiate epistemic injustice. To reiterate, we do not pass judgement on whether epistemic injustice manifests itself in the specific case of Olivia Caramello because, as elaborated above, we deem our available evidence as too weak to support such claims. The Caramello case has served us as an indicative story, as a means to access how epistemic injustice can manifest in a mathematical practice. We highlighted how the existing body of knowledge comprises both secret and possible knowledge in close connection to the Caramello case. We now depart form the specifics of the Caramello case and discuss secret knowledge more generally. Our claim is that secret knowledge is a source of epistemic injustice in mathematics.[35]

One can reasonably expect mathematicians to know some of the secret knowledge of their field. This will be the case for ghost theorems that have been mentioned or relied upon in the well-known literature of the relevant domain. To be ignorant about such secret knowledge reveals a lack of diligence on the part of the mathematician.

Other kinds of secret knowledge are not accessible by diligent work alone. Some ghost theorems are neither mentioned nor relied upon in the well-known literature. Generally, an agent cannot actively

[33] Recall here that in this paper we do not pass judgement on the Caramello case.
[34] See Aberdein (2016).
[35] Addendum to the Caramello case: in private communication Caramello stressed how difficult it was for her to find an academic position. She was evaluated by the very mathematicians who had judged her work as unoriginal and who Caramello had decided to fight. Caramello furthermore attested that this episode in her life was very stressful to her and that she could have published "twice as much" otherwise. These points harken back to the negative feedback loop and epistemic silencing mentioned in Section 2.

search for such knowledge because the agent does not know what she is searching for. The agent has to hope for lucky finds in conversations, presentations, publications or else. To obtain this kind of knowledge thus requires epistemic luck.

To judge the epistemically unlucky as unworthy is vicious.[36] It harms the rejuvenation of the practice – the old had more time to be lucky than the young. Here is Awodey telling Caramello such hindrances for young researchers.

> I have also felt that the field of topos theory is not a good area for new researchers, in part because some of the experts know a lot more than they have published. […] My own response to this problem was to look for other areas than topos theory to work in, preferably ones in which those experts were not also working.[37]

Standards of evaluation which demand the kind of epistemic luck we are investigating here also do not promote the virtues of academic research. A diligent, meticulous, careful, and creative researcher may still see her work rejected by the members of the exclusive club of secret knowledge keepers simply because she was not epistemically lucky in the right kind of way. Such a virtuous researcher may reasonably question the worth of her virtues. To assess worthiness based on epistemic luck is thus corrupting.

The case of Caramello's duality theorem reveals another dimension of the kind of secret knowledge discussed here. The theorem can neither be learned from the well-known literature (cf. Joyal) nor is Caramello reliant on epistemic luck in the sense discussed above: she had a theorem and could thus have checked whether the theorem was already known; she knew what she was searching for. Such a search would require, besides much time and effort, trust in the contacted experts. To come up with a good theorem to prove is hard. To then go around telling members of the community about it without having it published requires a trust in them that they will not publish it first. To demand this is to demand that the agent puts herself into an epistemically volatile situation. Such demands thus display the vice of recklessness: they promote potentially dangerous behaviour.

Epistemically lucky agents may not know that they have been epistemically lucky. They may misjudge their lucky find as something ordinary. This blurs the distinction between the kind of secret knowledge which an agent can be expected to have in a community of experts and those kinds of secret knowledge which should not be thus expected: agents will judge what can be expected differently. This introduces epistemic luck into the whole process. Those who are judged need to be lucky enough to have judges with a virtuous understanding of what can be expected in order to be judged worthy.

Standards of worthiness based on secret knowledge are vicious. They harm the rejuvenation of the epistemic practice in question; they fail to promote virtues; they display a reckless disregard for the other. They are unjust obstacles on the road to participation in a specifically epistemic practice. Thus, they are a source of epistemic injustice in mathematics.

Michael Barr writes:

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[36] Other instances of epistemic luck in mathematical practices include simultaneous discoveries with unevenly distributed credit; see Whitty (2017). A discussion of these matters is beyond the scope of this paper.

[37] http://www.oliviacaramello.com/Unification/AwodeyResponse.html. Compare this also to Thurston's (1994) "I heard from a number of mathematicians that they were giving or receiving advice not to go into foliations— they were saying that Thurston was cleaning it out. People told me (not as a complaint, but as a compliment) that I was killing the field"; p. 173.

> In general I would say that any result that is "well-known folklore" should be published if for no other reason than that then it can be cited.[38]

We have given another reason. The special kind of folk theorems we called ghost theorems promote secret knowledge and secret knowledge is a source of epistemic injustice in mathematics. To publish well-known ghost results would help to dry out this source.

4. Thomas Royen and the Gauss Correlation Inequality

In Section 2, we gave a brief introduction to the idea of epistemic injustice. In Section 3, we sharpened our understanding of the term by providing a detailed case for the specific form of epistemic injustice in mathematics that can arise from ghost theorems. Part of that argument was that participation in a mathematical practice can be unjustly obstructed. However, as we already mentioned, not all obstacles in one's way to participation in a mathematical practice are a source of epistemic injustice. In this section, we engage with this issue in more detail by providing an analysis of a case where there were obstacles to participation which, even though they may seem harmful, did not bring about epistemic injustice. This allows us to draw out differences between cases of epistemic injustice and mere cases of obstacles to participation, which deepens our understanding of epistemic injustice as a concept. We discuss the case of Thomas Royen's proof of the Gauss Correlation Inequality.

The Gauss Correlation Inequality is found across both multivariate statistics and convex geometry, and (roughly) relates the probability of some item falling in the overlap of two shapes to the probability of falling into those two shapes individually. Originally conjectured in the 1950s, it had become a well-known conjecture which had stumped many researchers who had tried to prove it for decades. Despite this, when Thomas Royen provided an elementary proof of it in (Royen 2014), it received little response from the mathematics community. Only after an additional article, made available on the ArXiv in December 2015, bringing attention to the proof (published as Latała and Matlak 2017), a presentation of the result by Franck Barthe at the Institut Henri Poincaré[39], and media attention in *Quanta Magazine*[40] and various other newspapers[41] has this proof come to be well-known. A major mathematical result received little attention or recognition for nearly three years. Was Royen denied his rightful credit?

Royen worked as a statistics professor from 1985 – 2010 at the Fachhochschule Bingen[42] in Germany. He is now retired. In 2014 Royen had the idea for his proof of the Gauss Correlation Inequality. The proof is simple and uses only classical techniques; "any graduate student in statistics could follow the arguments, experts say".[43] Instead of using LaTeX, the standard word processor amongst mathematicians, Royen typed his proof in Microsoft Word and posted his paper on the mathematical database ArXiv. Royen also sent his proof to his American colleague Donald Richard (Pennsylvania State University), who helped Royen re-type his manuscript in LaTeX and circulated

[38] http://www.oliviacaramello.com/Unification/BarrResponse.html
[39] Available online: https://www.youtube.com/watch?v=lWk5qeeb_0A
[40] https://www.quantamagazine.org/statistician-proves-gaussian-correlation-inequality-20170328/
[41] Examples include the German *Frankfurter Allgemeine Zeitung* (FAZ), the *Spiegel* and the English *Independent.* We will refer to the FAZ article found here: http://plus.faz.net/feuilleton/2017-04-07/der-beweis/338154.html/ accessed 26/01/2018.
[42] "Polytechnic Bingen" (our translation).
[43] https://www.quantamagazine.org/statistician-proves-gaussian-correlation-inequality-20170328/

Royen's paper within the community. One of the experts they contacted was Bo'az Klartag of the Weizmann Institute of Science and Tel Aviv University. Royen's manuscript arrived together with two other purported proofs of the Gauss Correlation Inequality. Klartag checked one of them, which contained a mistake, and then put the others to the side "for lack of time".[44] Royen also published his proof in the little-known pay-to-publish Indian journal *Far East Journal of Theoretical Statistics*, of which Royen had become an editor one year prior. In December 2015 Latała and Matlak made available a paper which begins as follows: "The aim of this note is to present in a self contained way the beautiful proof of the Gaussian correlation inequality, due to Thomas Royen"; (Latała and Matlak 2015). Their paper was well received and word about Royen's proof began to spread. However, when Quanta Magazine published its article about Royen's proof in March 2017, they wrote: "The statistician Alan Izenman, of Temple University in Philadelphia, still hadn't heard about the proof when asked for comment last month".

The *FAZ* article implicitly suggests that Royen's is a case of epistemic injustice by describing the story as an example "über die Schwächen und Tücken des Wissenschaftssystems"[45]. The idea would be that the slow dissemination of Royen's work is due to participatory barriers to the epistemic community of mathematicians, similar to the case of ghost theorems above. Royen was not accepted as a member of the mathematical community, so mathematicians did not feel that they had to pay proper attention to his contributions and accorded him a credibility deficit in assessing his claims to have proven the conjecture.

Although we agree that it is a shame that it took time for such a result to be picked up and acknowledged, we argue that this is not a case of epistemic injustice.

Royen chose to publish his work in the pay-to-publish *Far East Journal of Theoretical Statistics* rather than going through the arduous peer-review process of a high-level mathematics journal. Royen also was a member of the journal's editorial team, which would further undermine the confidence in the journal's ability to provide objective evaluation for the paper. In the *FAZ* interview he even describes how he was too demanding as a referee for the journal in the past, so they stopped sending him refereeing requests. Royen believes that he was bad for business. This suggests that he could know in advance that this publication would add nothing to the credentials and credibility of his proof. It may have even diminished the credibility by making it appear more like so-called "crank mathematics". Indeed, the Gauss Inequality Conjecture sat alongside infamous mathematics like Gödel's Incompleteness Theorems, *P=NP*, Cantor's diagonal argument, Goldbach's Conjecture etc. for attracting crank results. Mathematicians are especially wary of purported proofs or refutations of such statements. These proofs rightly have low levels of credibility in the mathematics community, so if one presents one's work in this way it is not a credibility deficit that one encounters. Such proofs are largely ignored in favour of mathematics by well-respected colleagues, who are far more likely to be doing rigorous and interesting work.

Royen had little contact with the wider mathematical community. From the interview with Royen in the *FAZ* article we learn that he doesn't attend many mathematics conferences, and that he believed his academic position was not well-respected in the field: "In Germany, when one is not at a [research] university, then one is considered a nobody".[46] While prestige bias is a relevant issue in

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[44] Ibid.

[45] Roughly translated to "the weakness and perils of the academic system".

[46] "Anfänglich besuchte Royen noch ab und zu Konferenzen, doch wirklich engen Kontakt hatte Royen nicht zu Kollegen. In Deutschland hatte er mit dem niedrigen Ansehen der Fachhochschulen zu kämpfen. „Wenn man in Deutschland nicht an einer Universität ist, dann gilt man ja nichts." Er lacht." (Anderl 2017). Our translation: "At first Royen still attended conferences from time to time, but he did not have close contact with colleagues. In Germany he struggled with the low reputation of polytechnics. 'In Germany, when one is not at a university, then one is considered a nobody. He laughs."

academia that needs addressing, it is important to note that epistemic communities are social and therefore make demands on the individual too. If one chooses not to interact with others, then the rest of the community is not blameworthy when they are slow to come to appreciate one's work.

In Royen's case there are no unfair participatory barriers to the epistemic community, but rather an unwillingness to join and become a full participant. Royen chose to ignore certain norms of the mathematical culture he aspired to join; the presentation style did not meet contemporary standards and the journal chosen for publication is not well-respected. The cultural norms that kept Royen from receiving his well-deserved credit are not to blame because Royen chose to ignore them.

Royen's case may actually be a success story of how mathematical knowledge gets disseminated. While Royen's proof was not picked up immediately, the time that it took (and still takes) for the proof to come to be known and accepted is not particularly long by academic standards. Despite the tarnished credibility caused by Royen's choice of where to publish the theorem, it has still found its place and Royen has now rightly received the credit for his marvellous breakthrough.

An interesting counterfactual to consider is whether the proof would have been accepted had it been less elegant and simple. The proof as it stands is surprisingly straightforward for such a major theorem, so it is easy to verify for an expert in the field. Had the proof required substantially more mathematical machinery, we can imagine experts refusing to put the time in to understand it carefully and thereby the proof being missed by the mathematics community. Nonetheless, there are standard ways to go about disseminating such works, such as lecturing across different departments and conferences, or through correspondence with experts in the field. Through these kinds of activities, one partakes in the epistemic community and will gain the credibility needed for experts to willingly review your work. Undoubtedly, some mathematicians won't participate in these activities, and there are surely some of these counterfactual Royens whose important results are missed. But the mechanisms of mathematical communities also provide the pathways to having one's work accepted.

The lesson, then, is that while there can be epistemic injustice manifested in barriers to entry to a mathematical community (as with ghost theorems), that does not mean that all barriers to participation are epistemically harmful. It is not the mere existence of prerequisites for joining the mathematical community that are the source of epistemic injustice. Rather, it is when these barriers to the epistemic community are opaque, unfair, or biased, that epistemic injustice occurs.

5. The case of Ramanujan

Barriers to entry in a mathematical community may (but need not) be a source of epistemic injustice in mathematics (cf. sections 3 and 4). Some such barriers may be constitutive of the relevant practice; one needs to know some number theory to participate in number-theoretic practices. Can such constitutive barriers also lead to instances of epistemic injustice? We discuss the singular case of the Indian mathematician Srinivasa Ramanujan (1887-1920) to discuss injustices resulting from the obligation to provide proofs, which will lead us to consider oracles and their access. A second injustice then appears in terms of this accessibility.

Although it might seem that not much needs to be said about Ramanujan – he is definitely part of the gallery of mathematical heroes, witness the film *The Man Who Knew Infinity* (2016), directed by Matt Brown, itself rather freely based on Kanigel (1991) – nevertheless when it comes down to the mathematical work itself many questions remain unclear. Although it seems clear that he did not produce proofs (in the standard sense) for his mathematically bold and surprising statements, it is less clear how he arrived at his results. Or, to be more precise, he did not write the proofs down, the lack of (expensive) paper being an important reason. In other words, the notebooks only mention the results. Did he use some kind of induction (not mathematical induction but rather the capability to see a general pattern in a finite and limited set of examples)? Did he make use of subtle algorithmic manipulations or complex calculations? Or did he have 'arguments' (here understood as an informal step in a reasoning) of some kind, enough to justify the results but indicating a lesser degree of rigour than is standardly demanded? This was a point of discussion between his Cambridge mentor G.H. Hardy and Ramanujan later on (see (Berndt and Rankin 1995)):

> Dear Sir,
> I was exceedingly interested by your letter and by the theorems which you state. You will however understand that, before I can judge properly of the value of what you have done, it is essential that I should see proofs of some of your assertions. (46)

and

> But I want particularly to see your proofs of your assertions here. You will understand that, in this theory, everything depends on rigorous exactitude of proof. (47)

This in itself is quite interesting: Hardy insists on proofs because the domain – in this case, analytic number theory – requires, according to Hardy, the highest degree of rigour. In fact, in the introduction to *Ramanujan's Collected Papers* (Hardy et al. (1927)), Hardy writes:

> There are regions of mathematics in which the precepts of modern rigour may be disregarded with comparative safety, but the Analytic Theory of Numbers is not one of them, … (xxiv)

Not all the results produced by Ramanujan were correct. In fact, when he sent his results for the first time to Hardy, roughly one third was original (and proved to be correct), one third were known results, and one third was wrong. This implies that an authority argument – 'A' is true because the authority says so – is not applicable here. And, even if it were so, the question remains whether this is acceptable in mathematical practices. There are at least two arguments that suggest otherwise:

1. We do not expect mathematical results to be dependent on a particular mathematician. It is precisely one of the defining characteristics of mathematics that the author disappears into the background. Fortunately, one need not know Euclid's biography to understand the *Elements*. (But note that it is a different thing to understand the Greek culture to which Euclid belonged to understand certain practices, as this does not involve a particular individual but a more global cultural setting. See Netz (1999, 2009) for an intriguing approach to Greek mathematics and Greek culture.) In the case of an authority, we do need specific properties of the individual to be able to judge whether or not his or her claims can be accepted.[47]

[47] It is interesting to make the comparison with the accepted practice in mathematics of attributing important theorems to specific mathematicians (often, but certainly not always the actual author). We all know the examples: the Cauchy-Schwarz inequality, Brouwer's fixed-point theorem, the Radon-Nikodym theorem, Fermat's Last Theorem, … This is not to be seen as a case of authority acknowledgement but rather a recognition of the fact that that mathematician was the first one to prove the theorem. Apart from this "property claim", nothing needs to be known about his or her life and times.

2. It follows from the previous point that access to the authority is important. They who do not have such access will not know about the results produced by the authority. One could, of course, publish these results but then the more serious problem remains: the missing proofs need to be produced. Apart from the fact that this requires genuine mathematical talent (of the level of Hardy and Littlewood in the case of Ramanujan), once the proof has been found the original source becomes irrelevant for now there is a proof and therefore the authority argument no longer holds sway.

From the epistemic point of view, one might think it is a valid option to do away with authorities altogether (in addition, to avoid possible epistemic injustices). Or so it seems at first sight. Would it not follow from the above considerations that Hardy and Littlewood should not have invited Ramanujan to come to Cambridge? The suggestion we would like to make is that, instead of ‘authority’, a term such as ‘oracle’ explains the situation better and will help us to identify an important source of epistemic injustice. There are at least two conditions that need to be satisfied to qualify as an oracle:

1. An oracle needs to produce results that are *interesting*. This is necessary to justify the time needed to prove the results. Seen this way, there is a similarity with the ‘open problem’ phenomenon. When a mathematician at the end of a paper lists a set of open problems, these problems will only be picked up by other mathematicians if, for whatever reasons, they seem interesting to pursue. Perhaps the most well-known case is Paul Erdös who expressed the importance and degree of interest in pecuniary terms (see Hoffman (1998) for some nice examples).

2. An oracle cannot be a random source of results. Suppose it were, then it would become necessary to have a set of filters to select the interesting results from the trivial, silly, wrong or impossible ones. But if not random, then one must be convinced of the fact that some ‘mechanism’ (or coordinated set of mechanisms) is at work that gives a sufficient support to the idea that the results are indeed interesting and worthy of the time of investigation. This goes together with the mechanism not being knowable because, if so, the oracle would lose its special status. There is an element of unavoidable ‘mystery’ present here and, from the epistemic point of view, this mystery reinforces the power of the oracle. This could perhaps be described as a form of ‘epistemic awe’, a form of epistemic virtue. In general terms, awe is related to reverence and admiration and often it is associated with a combination of wonder and fear. It is not associated with *blind* admiration because one can produce good reasons and arguments for being in a state of awe. What it does recognize is the special status of the source of awe. That in its turn implies that one is knowledgeable of what it means to attribute such a status to someone and that means one must have some idea of the social structure of the mathematical community to which one belongs. In this sense two mathematicians within the same community, standing in awe of the same person, thereby confirm that they recognize and accept each other as members and thus the expression of awe also serves as an expression of social cohesion.

These conditions were certainly satisfied in the case of Ramanujan. As to 1, the fact that he also reproduced known results was an important element in judging the unknown results as interesting, in addition, of course, to their puzzling nature, inducing even a form of doubt (in the sense of ‘Could this really be true?’). After all, a result can be too ‘nice’ to be true![48] As to 2, the best

[48] A nice example are formulas that express the number of ways, p(n), into which a natural number n can be partitioned. If anybody would come up with a simple formula for p(n), that would be too ‘nice’ to be true. An alternative example would be any statement that talks about objects in a particular domain without any exceptions. So instead of “For all functions f, satisfying conditions C1, C2, …, Cn, X holds”, one boldly states: “For all functions f, X holds”. This, by the way, corresponds nicely to one of the ten signs a claimed mathematical breakthrough can be wrong, summed up by Scott Aaronson on his blog https://www.scottaaronson.com/blog/?p=304 (consulted on May 15 2018). The third sign is that “the approach seems to yield something much stronger and maybe even false (but the authors never discuss that).”

illustration of the presence of epistemic awe is the fact that there was a discussion whether or not Ramanujan should 'learn' how to produce proofs out of fear that it would 'contaminate' the original mechanisms of his thought. As far as we can tell, for Ramanujan the source of his inspiration was divine. He is claimed to have stated that 'An equation for me has no meaning unless it expresses a thought of God.' (see Kanigel (1991), 7). In more mundane terms, one is, of course, inclined to think of mathematical intuition and the words of Reuben Hersh in his (1997) illustrate what it is we are trying to state here:

> The word intuition, as mathematicians use it, carries a heavy load of mystery and ambiguity. Sometimes it's a dangerous, illegitimate substitute for rigorous proof. Sometimes it's a flash of insight that tells the happy few what others learn with great effort. (61)

It is worth mentioning that, as a matter of fact, Ramanujan produced proofs, but apparently his proof style was clearly idiosyncratic and easily recognisable or, in more vulgar terms, 'weird'. And in some cases not even that: witness the one-page paper *Congruence Properties of Partitions* (in Hardy et al. (1927)), where Ramanujan simply states that:

> I have since found another method which enables me to prove all these properties and a variety of others, … (230)

without any clarification as to what that method could be.

However, the question remains whether, from the epistemic point of view, oracles are permissible in mathematical research, whether mystery and epistemic awe should have a place, and whether epistemic injustice can manifest itself. To start, we argue that some form of epistemic luck is involved here too. For, as long as the oracle speaks, the results will not be published for the simple reason that proofs are lacking. (This, by the way, leads to an interesting comparison with ghost theorems where the opposite phenomenon is at work: it is not published because it is deemed uninteresting to do so.) Thus, direct access to the oracle or – to remain within the analogy – to the high priests assigned to the oracle, is essential to know what the results are. This therefore leads to the unavoidability of epistemic luck. To make a comparison: imagine that David Hilbert had spoken his famous 1900 lecture, listing his famous 23 problems, at home for a selected group of mathematicians. Being part of that group would have meant insights into the future of mathematics, in terms of what is interesting and worth the time investment[49]. Add to this the fact that the high priests are the ones who will have the first opportunity to search for a proof, and it is clear that thereby they are provided with an epistemic advantage over others, leading to epistemic injustice.

At the same time, it should be clear that, epistemically speaking, the existence of oracles, as already indicated, does seem to have its virtues. In its mildest form we know it, as said, as open problems in journal papers, in a stronger form as an agenda for future research and, in a special form, as claims that certain results are true without proofs provided[50]. It is an odd observation to make, but this latter strategy reminds one of late Renaissance mathematicians, who often wrote letters claiming to have a proof of a statement, though they had no such complete proofs readily available but rather a

[49] At least that surely was Hilbert's intention, witness the opening statement of the 1900 lecture: "Who of us would not be glad to lift the veil behind which the future lies hidden; to cast a glance at the next advances of our science and at the secrets of its development during future centuries?" (Hilbert 1902, 407).

[50] It is important to remark here that oracles are not intended to replace mathematical proofs and proof search. This has been made quite clear by Yehuda Rav (see Rav (1999), 5 – 6): the existence of a perfectly reliable yes-no oracle – called PYTHIAGORA, by the way – would eliminate the need for proofs, thus marking the end of mathematics as such. The main argument is that lack of insight would prevent us from thinking up new conjectures and ideas worth pursuing.

proof idea or outline, in the hope that the other party would struggle to provide a complete proof[51]. Many statements were proven using this somewhat provocative procedure, so it clearly made mathematics go forward.

6. Enculturation

Hardy wanted Ramanujan to write proofs. Proofs, so Hardy claimed, are part of the mathematical culture Hardy wanted Ramanujan to join: the analytic theory of numbers. In the last section, we offered some thoughts on this particular case and contrasted it with our notion of epistemic awe. In this section, we draw out more generally how enculturation can lead to a form of epistemic injustice.

Unlike the prior sections, this section does not deal with research mathematics. Instead, we will be discussing epistemic injustice in mathematics as it happens in the classroom. There are two reasons for this. First, the school setting is an obvious place to look at in order to learn about potential injustices with the enculturation into mathematical cultures. Second, mathematics extends beyond the practices of research mathematicians. By providing a discussion of the classroom setting we provide a richer description of how epistemic injustice can manifest itself in mathematical practices.

Imagine a school class learning about fractions. One student hands in homework in which he wrote that ½ + ½ = 2/4. When asked about this by his teacher, the student reasons as follows: "suppose there is a car park with two cars in it, one of which is yellow. Then one out of two, i.e., ½, cars in this car park are yellow. Now imagine another car park with two cars in it, again one of which is yellow. Then, when you put the two car parks together, you have a car park with four cars, two of which are yellow".

What should we say to such a student? We might focus on the formalism used, ½ + ½ = 2/4 (= ½), and point out that it is incorrect, that a half plus a half does not equal a half but equals one. We might try to make the student see this, for example by reasoning about pizza slices. In this case, we would aim to help the student to acquire some cognitive cultural practices. Richard Menary (2015) calls this enculturation: "Enculturation rests in the acquisition of cultural practices that are cognitive in nature" (p. 4).

We will argue that aiming to enculturate the student to fractions can be justified. We say "can be" rather than "is" justified here because there are dangers with enculturation as conceived by Menary. In this paper we focus on one particular danger: the kind of epistemic injustice that can occur in cases of enculturation.

Menary is a cognitive scientist. His focus on the enculturated agent is thus unsurprising.[52] A fuller picture also needs to consider that which is doing the enculturating. Often, this will be a human agent; its avatar is the teacher. Enculturation may also happen through artefacts such as books. Our focus is on enculturating done by human agents.

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[51] A famous example is no doubt Pierre de Fermat. In Mahoney (1994) we read that: "His notion of how to share mathematical results had not, however, changed much since the early 1640's. He still preferred to let his correspondents wrestle with the problems before revealing his solutions and methods of solution. If he did not like controversy, he did enjoy intellectual combat." (62 – 63)

[52] Menary tells us that he "seeks to outline the phylogenetic and ontogenetic conditions for the process of enculturation" (Menary, 2015), p. 20.

To enculturate somebody does not mean to make this person cultured. To say somebody is cultured is a term of praise. It is fundamentally different from saying that somebody is part of a certain culture, which is an assessment of a situation rather than a term of praise. Anthropologists know about this difference[53] and it is important to keep it in mind when thinking about enculturation.

If becoming enculturated does not mean to become cultured and thus praiseworthy, but instead simply means to become part of a certain culture group, then we can see why the to-be-enculturated agent may want to resist enculturation; we deem it praiseworthy to resist enculturation into the cognitive cultural practices of the Ku Klux Klan.

Enculturation can be coerced. “Believe us or die” has stood on the swords of various religious groups throughout the centuries. Academics may publish on certain topics simply because these topics are trendy and allow for quick (and needed) publications. The teacher’s guiding hand may turn into a controlling whip that impels the student down a certain path.

Coerced enculturation is not in itself unjust. In contemporary Europe we are coerced to enculturate into the cognitive cultural practices associated with literacy for example. This coercion is done via threats of becoming a dysfunctional member of society should we resist enculturation, point systems in schools, parental guilt or other means necessary to make children literate. Whilst the methods used to coerce the children into literacy may at times be blameworthy (e.g., parents hitting their children), the goal to ensure literacy is itself not blameworthy.

There is an important difference between the literacy case and the other examples of coerced enculturation we gave. In the literacy case there is a single intellectual practice on offer for which enculturation needs to be considered. In the other examples, there are multiple such practices available, but the to-be-enculturated agent is coerced into “the one”; be it through the “believe us or die” rhetoric of some fundamentalist groups, through the “do this or fail” rhetoric found in teaching environments or else. In these cases, there is no argument why the particular epistemic practice is chosen; perhaps it was not even acknowledged that a choice has occurred. In these cases, the threats or mechanisms of force that make up the coercion exercised by the enculturating agent allow this agent to sidestep a debate with the to-be-enculturated agent about which epistemic practice is most suitable for the given situation. This, we argue, is a form of epistemic injustice.

We are tracking the thought that epistemic injustice can occur if an agent coerces another agent into one epistemic practice when two (or more) such practices offer themselves in the given situation. There is a blocking from participation in the non-selected epistemic practice here, which aligns this form of epistemic injustice with the Hookwayian *participant perspective* discussed earlier in this paper. However, the unjustly treated agent is not excluded from participation in all distinctly epistemic practices under consideration. Rather, he is coerced into a particular epistemic practice.

Coercion into one epistemic practice rather than another is epistemically unjust. The coercing agent displays an unwillingness to engage in argument why the selected epistemic practice is preferable in the given situation to the non-selected one. She, the coercing agent, is thus non-communicative. She also fails to display the intellectual courage needed to engage in such a debate. Thus, she seems to display a lack of faith in her reasons for her choice (assuming there were any in the first place). This amounts to a lack of fairmindedness and intellectual empathy needed for open debate on her part. Instead, she falls back onto a reasoning from authority. These are argumentative vices.[54]

[53] See e.g. (Kroeber and Kluckhohn, 1952).
[54] Cf. (Aberdein, 2016).

She, the coercing agent, also robs him, the coerced agent, of the ability to display his argumentative virtues. She encroaches on his intellectual autonomy to choose which epistemic practice is most suitable for the given situation. She acts in a way that threatens his intellectual courage to explore ways of thinking for his own and she threatens his faith in reason by not allowing his arguments their rightful place.

The coerced agent is thus in a situation in which he is victim to the argumentative vices of the coercing agent. This renders him unable to manifest some of his argumentative virtues. Importantly, his abilities to escape this situation are hampered by threats to his well-being or mechanisms in play that force him to be submissive to the coercing agent. The coercing agent has thus put the coerced agent in a near inescapable situation which is epistemically vicious. This is unjust.

We argue that this form of epistemic injustice can occur in mathematics. Mathematics is not a black and white world of right and wrong answers. To think that $\frac{1}{2} + \frac{1}{2} = x$ allows only for one value of x is to assume that there is only one way to think about the problem. This would be misguided. The student we introduced at the beginning of this section offered another way of thinking. He reasoned on parked cars and their colour. This reasoning was sound. What is more, it suggests a generalisation to a mathematics of proportions.

Using the formalism the student provided we might define:

$$G^* = \{a/b : 0<a<b \ \& \ a,b \text{ natural numbers}\}$$

and an addition function + from G^* to G^* such that

$$a/b + a'/b' = a+a'/b+b'$$ [55]

Mathematically speaking, $(G^*, +)$ is a magma, a group-like object without much of the structure found in groups. Adding a 0-element to G^* gives us G. Now define $a/b+0=0+a/b=a/b$. Then $(G,+)$ is a monoid. In terms of structure, monoids are between magmas and groups. Monoids (perhaps unlike magmas) have enough structure to be mathematically interesting and have been extensively studied.

The student has thus taken first steps into exploring a mathematically fruitful structure. We might call such an exploration bottom-up since it starts from examples (cars in this case) which can be lifted to more general structures (e.g., magmas and monoids). Such explorations are part of successful mathematical practices and therefore deserve to be nurtured.

It is easy to imagine how a teacher who, charged with enculturing the student into the theory of fractions, upon reading the student's $\frac{1}{2} + \frac{1}{2} = 2/4$, awards 0 points. We can even imagine the student asking about this and explaining his reasoning, only to be shut down by a version of the argument that in mathematics, you are either right or wrong, and 2/4 is the wrong answer here. Such a teacher would use the point system employed in schools to coerce her student into one epistemic practice (fractions) over the other (bottom-up exploration). Such a teacher would act epistemically unjustly.

It is similarly easy to imagine a teacher who engages with the reasoning the student provides. A teacher who argues with the student, pointing out the values of bottom-up explorations but also stressing the necessity to master fractions to become a successful mathematician. Such a teacher would guide rather than coerce the student into one epistemic practice over another, thereby avoiding the epistemic injustice that coercion entails.

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[55] Hardy and Wright (1975) call a+a'/b+b' the "mediant" (p. 23); we are indebted to Peter Cameron for drawing our attention to this. Notice that there is a slight abuse of notation in our presentation: on the left-hand side the + sign denotes the newly defined function, on the right-hand side the + sign has its usual meaning. This need not bother us as it is clear from the context what + is intended to denote.

The idea that taking the epistemic framework of an agent seriously whilst also guiding him into a different framework is more epistemically just resonates with the work that has been done on epistemic injustice in healthcare. Carel and Györffy (2014) write:

> Not only children's testimonies, but also their interpretative frameworks are at risk of rejection by adults, who, with few exceptions, cease to readily understand the child's world. When the two interpretative frameworks clash, the adult interpretation usually trumps the child's. (p. 1256)

Carel and Györffy give the example of sexually abused children interpreting and reporting their pain as "tummy ache" instead of "abdominal pain". Carel and Györffy argue that healthcare professionals must be able to reach and understand their young patients and the authors provide a list of gruesome examples in which these communication skills have failed.

Epistemic injustice in healthcare may not be limited to young patients. Carel and Kidd (2014) argue that the gap in hermeneutical resources available to, on the one hand, the epistemically privileged healthcare professionals - the doctors with all their knowledge about the human body and the illness that may befall it – and, on the other hand, the ill - who may lack such knowledge - can lead to epistemic injustice. To bridge this gap, the epistemically privileged need to reach and understand the epistemically disadvantaged, because the epistemically disadvantaged lack the hermeneutical resources to reach and understand the epistemically privileged; it is the doctor who must understand the girl's "tummy ache" as abdominal pain and not the girl who needs to understand the doctor. Nonetheless, it may be beneficial for the patient to let the doctor change her interpretative framework. Carel and Györffy mention a 5-year-old girl who reports double vision. A CT scan is avoided when the doctor realises that what the girl meant was blurred vision: she needed glasses. To guide the girl's interpretative framework from "double vision" to "blurred vision" provides her with an explanation of what is happening to her and epistemic injustice is averted.

In this section, we challenged the view that enculturation into an epistemic practice is always an epistemic good. We showed how coercion into an epistemic practice can lead to epistemic injustice and suggested that guiding rather than coercing can help avoid this kind of injustice.

7. Conclusion

Epistemic injustice can occur in mathematics. We showed how ghost theorems can be a source of secret knowledge that influences what mathematicians perceive as the existing body of knowledge. This perception of what the existing body of knowledge is influences judgment on the originality (and hence publication-worthiness) of a mathematical work. As such, ghost theorems can function as unjust barriers to mathematical publication and thus to participation in a distinctly epistemic practice. This makes ghost theorems a potential source of epistemic injustice of the kind Hookway has called the "participant perspective"; Section 3.

Mathematical cultures and practices come with their own norms and values that shape what it means to be a successful member. These norms and values are barriers to participation, yet they need not be sources of epistemic injustice. We presented the case of Thomas Royen to make this visible. This sharpened our understanding of the concept of epistemic injustice by making us more sensitive to what it is not; Section 4.

To provide proofs for one's theorems is part of what it means to be a research mathematician, yet one of the best-known mathematicians, Ramanujan, did not always submit to this norm. We suggested the notion of an oracle and its associated virtue of "epistemic awe" to make visible how even seemingly entrenched and defining norms of mathematical cultures can be overcome in the pursuit of epistemic gain; Section 5.

The norms that shape what it means to be part of a mathematical culture can act as barriers to participation in ways that are epistemically unjust. We can also be coerced to submit to these norms. We showed how such coercion can lead to cases of epistemic injustice in enculturation in the school setting and suggested guiding rather than coercing into a culture as a way to avoid such cases of injustice; Section 6.

Our paper reveals a social dimension of mathematical knowing. This paper has shown that the norms and social features of mathematical cultures and practices shape what we consider the existing body of knowledge in the relevant field to be. They can also be sources of epistemic injustice in mathematics. Only once these injustices have become visible can we aim to overcome them. Philosophy can thus aim to further shape mathematics into a desirable form by drawing our attention to cases of injustices that occur in mathematical practices. The virtue terminology, so our paper suggests, is suitable for such a task. It is hence time for an aretaic turn in the philosophy of mathematics.

Acknowledgements

The authors are indebted to Ladislav Kvasz for a very fruitful discussion about folk theorems. We thank Olivia Caramello for her comments and discussion on the folk theorem section and Katherine Hawley for her helpful comments on section 2. We would also like to thank Andrew Aberdein, Neil Barton, Catarina Dutilh-Novaes, Alessandra Tanesini, Benedikt Löwe, Josh Habgood-Coote, and the audiences at St Andrews, Munich, Hamburg and the CLWF in Brussels for their helpful criticisms on presentations of drafts of this paper. Furthermore, we thank two anonymous referees for their very helpful remarks. Research for this paper by the first author has been funded by the Research Foundation - Flanders (FWO), project G056716N. Research for this paper by the second author was supported by the EPSRC grant for the project 'The Social Machine of Mathematics' led by Prof. Ursula Martin [EP/K040251/2].

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