'A REMARKABLE ARTIFICE': LAPLACE, POISSON AND MATHEMATICAL PURITY

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Abstract. In the early nineteenth century, a series of articles by Laplace and Poisson discussed the importance of 'directness' in mathematical methodology. In this thesis, we argue that their conception of a 'direct' proof is similar to the more widely contemplated notion of a 'pure' proof. More rigorous definitions of mathematical purity were proposed in recent publications by Arana and Detlefsen, as well as by Kahle and Pulcini: we compare Laplace and Poisson's writings with these modern definitions of purity and show how the modern definitions fail to grasp some more nuanced aspects.

§1. Introduction. At the onset of the nineteenth century, two leading French mathematicians argued over proper methods of proof through a series of publications: In 1809 the back-and-forth was kicked off by Pierre-Simon Laplace (1749–1827), whose initial article prompted responses from Siméon-Denis Poisson (1781–1840) over several years.

The particular discussion between these two leading mathematicians which we will investigate started when Laplace published his *Mémoire sur divers points d'analyse* [22], wherein he employed a novel method to solve a particular problem. In response to this publication by Laplace, Poisson [30] criticised the novel method used by Laplace and proposed an alternative way to solve the same problem. More responses followed from both sides discussing the novel method employed by Laplace.

In his 1809 article [22], Laplace computed a specific class of improper integrals, namely

$$\int_0^\infty x^{-\alpha}\cos(x)dx \quad \text{and} \quad \int_0^\infty x^{-\alpha}\sin(x)dx,$$

wherein $0 < \alpha < 1$ is a constant. Laplace was interested in these integrals because they were useful to the study of a physical system. After his investigation of these integrals, he 'applied his results to the study of an elastic lamina wound on itself in the form of a spiral' [7, p. 96] in the second half of the article, which we won't discuss.

In his article from 1811, Poisson highlighted the part of Laplace's paper which concerns the integrals, and he provided an alternative proof for the results that Laplace had derived. In Laplace's article, a crucial step in his solution of the integrals was a complex-valued substitution. This step of introducing complex numbers to solve a real-valued integral is what bothered Poisson: he did not believe that we are 'allowed'

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to prove statements in real analysis by using complex numbers—why would something external to our notion of real numbers, specifically the use of complex numbers, be able to teach us anything about strictly real integrals? Poisson's response to Laplace's article attacked this step. Instead, Poisson proposed a way to avoid it through what he called 'direct' methods [30, p. 243]. Laplace himself was hesitant to use the complex substitution, but he was still convinced of the truth of his findings.

The purpose of this paper is to investigate Laplace and Poisson's thoughts on this methodological issue of 'directness' and to compare our findings to recent work on similar issues. Specifically, we will argue that Poisson's 'directness' is similar to what we today call 'mathematical purity': the importance of mathematical purity, wherein a proof is considered pure if the methods used in it are in some sense intrinsic to the problem at hand, has been pondered by various philosophers and mathematicians throughout history.

In fact, sentiments similar to Poisson's were expressed by thinkers as far back as Aristotle. Aristotle was one of the earliest proponents of purity of proof methods, stating that 'we cannot in demonstrating pass from one genus to another. We cannot, for instance, prove geometrical truths by arithmetic' [4, p. 10]. This aspiration to avoid 'crossing from one genus to another', or *metabasis eis allo genos* in Aristotle's words, is one way to characterise the ideal of purity. Aristotle's reasons for promoting purity appeal to his view of how knowledge is developed: knowledge pertaining to a certain topic or subject should be inferred from the 'essence' of the subject. It seems that arithmetic truths are not part of the essence of geometry, and can thus not teach us the causes of geometrical truths.

Similarly to Aristotle, Poisson objected to the application of some mathematical techniques to specific problems. In particular he warned against the use of complex numbers in real analysis, or freely using Aristotle's terms, the crossing between the genera of real and complex analysis: the crossing from one genus to another mentioned by Aristotle is paralleled by a 'passage from the real to the imaginary' mentioned by Poisson and Laplace. Unfortunately it is not clear how exactly a 'genus' would be defined here since Aristotle did not know complex numbers. To Aristotle, geometry and arithmetic could easily be viewed as separate fields, as geometry was then based on geometric construction and was largely thought of without resorting to notions from arithmetic. Today, it is arguably harder to divide mathematical disciplines into separate 'genera', when for example complex and real analysis can be connected easily.[1] As such, we can't definitively view Poisson and Laplace's discussion through Aristotle's lens, as it is not immediately apparent whether Laplace's complex substitution constitutes a 'crossing from one genus to another'.

The familiar reader may notice that Aristotle's phrasing when arguing against the crossing of genera is reminiscent of a different, though related topic in the philosophy of mathematics, that of mathematical *explanation*. Philosophers of mathematics have noted an opposition between proofs that are explanatory and those that are not—this is based on the notion that a proof of a given theorem may convince the reader of its truth, while not giving a conceptual *explanation* of why it is true. Attempts to formalise our understanding of mathematical explanation have been proposed by

[1] Attempts at dividing up all of mathematics into separate fields have been made in spite of the perceived difficulty of the endeavour, for example, by Bourbaki in [8].

for example Steiner [31] and more recently by Pincock [29].[2] The quote above from Aristotle contests whether arithmetic truths can teach us anything about the *causes* of geometrical truths, which may seem to tie the issue of purity to that of explanation. An in-depth discussion of how Aristotle views the relation between the two concepts can be found in a recent article by Arana [3, pp. 26–27].

This same article also provides some contemporary insight into how purity relates to explanation. Arana shows that pure proofs are not necessarily explanatory, and vice versa. Our case study fits well with Arana's analysis of mathematical explanation as a concept distinct from that of mathematical purity. We shall see that Laplace and Poisson are concerned with the formal question of whether a calculation involves complex numbers or not. They find it desirable to eliminate dependence on complex numbers as an end in itself, with no regard for whether the alternative proofs so obtained are any more psychologically illuminating.

Bernard Bolzano (1781–1848), a contemporary of Laplace and Poisson, also stressed the importance of pure proofs. He was particularly concerned about the circularity of proofs in real analysis which appealed to geometric results, since he believed geometry to be a more specialised, applied field that was in the end derived from the principles of real analysis. As a consequence, mixed-field proofs could be circular when an analytic result is 'proven' using geometric reasoning which relies on the truth of the very analytic result we set out to prove: he cited contemporary proofs of the intermediate value theorem as an example, because these proofs would often "borrow a truth from geometry" [12, p. 183]. Talking thus about impurity, Bolzano stated that it was 'an intolerable offense against correct method to derive truths of pure[3] (or general) mathematics from considerations which belong to a merely applied (or special) part, namely, geometry. Indeed, have we not felt and recognized for a long time the incongruity of such *metabasis eis allo genos*?' [6, p. 228], quoting Aristotle.

By arguing that geometry is derived from real analysis, Bolzano argues that impure proofs (at least analytic proofs that rely on geometry) can lead to circular reasoning, which means that proofs thus obtained are epistemically worthless. Bolzano's argument relies heavily on his view that geometry is a more specialised subject, *derived from* analysis: circular reasoning of the kind that Bolzano warns us about only occurs if we use results from 'more specialised' areas to derive more general results. It's not entirely obvious that geometry is a specialised branch of real analysis (Aristotle would disagree), so Bolzano's argument is not obviously right.

Bolzano's ideas don't translate directly to those of Poisson and Laplace either. Bolzano warns against the use of more 'specialised' knowledge to prove more general statements, but from this understanding it's unclear what he would think about the use of complex numbers in real analysis. It's unclear whether complex analysis is a derived branch of real analysis—the case can be made that the opposite is true—so Bolzano's argument does not answer our question of what exactly constitutes a pure proof in our context, nor does it tell us why such proof methods are important in general cases, where the circularity Bolzano warns against is not apparent.

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2 A good overview of the history of mathematical explanation in the philosophical literature (up until 2008) can be found in [26].

3 Bolzano uses the word 'pure' here in the sense of dividing mathematics into pure and applied mathematics, which is a different issue entirely. An interesting account of the pure/applied dichotomy arising in the German-speaking area around 1800 can be found in [14].

Mathematical purity was not just important to people hundreds of years ago. Michael Detlefsen has argued that the 1950 Fields medal being awarded to the Norwegian mathematician Atle Selberg was partly due to his elementary (i.e., pure) proof of the prime number theorem:[4] 'that Aristotelian purity continues to function as an ideal among contemporary mathematicians is also suggested by the notice that was taken of Selberg's and Erdös' proofs, notice which resulted in Selberg's being awarded the Fields medal in 1950' [12, p. 190]. It is debatable whether the medal was awarded for this proof and whether it was the proof's purity that attracted Fields medal attention.[5] However, it's still worth noting that Selberg was looking for an elementary proof in the first place: if not expressed in the Fields medal, at least we see that twentieth century mathematicians were still interested in pure proof methods to some extent.

One particularly pointed statement of what a pure proof entails was given by David Hilbert (1862–1943) in his lectures on the foundations of geometry.[6] Speaking on the impurity of the spatial proof of the planar Desargues theorem, he stated that 'we are for the first time in a position to put into practice a critique of means of proof. In modern mathematics such criticism is raised very often, where the aim is to preserve the purity of method [die Reinheit der Methode], i.e., to prove theorems if possible using means that are suggested by [nahe gelegt] the content [Inhalt] of the theorem' [17, pp. 315–316]. Discussing this quote from Hilbert, Arana argued that 'what is critical for a proof's being pure or not, according to Hilbert, is whether the means it draws upon are "suggested by the contents of the theorem" being proved' [3, p. 29].

Hilbert has also been the subject of some investigation in relation to *simplicity of proof*, because of some notes of his on a '24th problem' which he considered to include in his list of 23 problems addressed to the International Congress of Mathematicians in Paris in 1900 [18]. The 24th problem asked for a criterion for a proof's simplicity, and for a way to find the most simple version of a proof. The question of proof simplicity has been considered in relation to purity, for example, in another article by Arana which challenges the claim that impure proofs are simpler than pure ones [2]. Our Laplace–Poisson case study will also show a mismatch in simplicity of pure and impure proofs, in favor of impure proofs sometimes being simpler.

The purpose of this article is to investigate Laplace and Poisson's *querelle* through the lens of contemporary definitions of purity, starting with a *topical* definition of purity formulated by Arana and Detlefsen in their 2011 article *Purity of Methods* [13]. We will also consider another definition by Kahle and Pulcini, which they call *operational* purity [19]. This operational definition was formulated as an alternative

4 The prime number theorem had already been proven by impure means in 1896 by Hadamard and de la Vallée Poussin. Both of their proofs use complex analysis, which qualifies them as not elementary, or impure. The case for impurity here is more easy to make than in the case of Laplace's complex substitution, as the connections between arithmetic and complex analysis are (at least intuitively) less tight than the connections between real and complex analyses. As such, the use of complex numbers in proving the prime number theorem attracted some protest as complex numbers seem very foreign to the study of primes. An overview of different proofs of the prime number theorem given by Hadamard and de la Vallée Poussin, Selberg and others is given in Dawson's *Why Prove it Again?* [11, pp. 111–147]. In his book, Dawson provides several case studies where mathematicians provided alternatives to existing proofs, also discussing their reasons for doing so—purity of proof methods is discussed as one reason for mathematicians to re-prove theorems.

5 For a more nuanced account of the attribution of this medal, see [5, p. 386].

6 A more complete account of Hilbert's views on purity of methods can be found in [16].

to topical purity, which the authors argue is flawed. Laplace and Poisson provide us with an interesting case study to investigate the differences between these two recent definitions, as well as with an opportunity to reveal their respective limits: we will see that both definitions fall short when confronted with the Laplace–Poisson case.

In the next few sections we will investigate Laplace and Poisson's writings, first analysing the mathematical contents of their initial articles after which we will delve into their own comments on 'directness'. We shall then confront this historical case study with the concept of purity as defined by Detlefsen and Arana as well as Kahle and Pulcini.

§2. Laplace's *Mémoire sur divers points d'analyse*. In his *Mémoire sur divers points d'analyse*, Laplace employed what he called a 'remarkable artifice' for solving real integrals through a 'passage from the real to the imaginary' [22, p. 193]. By modern standards Laplace's use of complex numbers is in some places unrigorous and arguably ill-defined.

In order to find values for improper integrals involving sines and cosines later on, Laplace began by considering the improper complex integral

$$\int_0^\infty x^{-\alpha} e^{ix} dx, \tag{1}$$

wherein $0 < \alpha < 1$. By Euler's identity, we have

$$\int_0^\infty x^{-\alpha} e^{ix} dx = \int_0^\infty x^{-\alpha} \cos(x) dx + i \int_0^\infty x^{-\alpha} \sin(x) dx,$$

so to evaluate the complex-valued integral (1) it suffices to evaluate two real-valued integrals involving sines and cosines. In fact, Laplace was interested in these two real integrals rather than the complex one: he only used the complex integral (1) in order to distinguish its real and imaginary parts later on to find his desired values.

As inefficient as this may seem, these integrals cannot easily be solved with more elementary methods. If we look at the integral $\int_0^\infty x^{-\alpha} \sin(x) dx$, we might consider using integration by parts to eliminate the $x^{-\alpha}$ term. This won't work because α is not an integer; the derivatives and primitives of $x^{-\alpha}$ will always leave some power of x, leaving us with yet another difficult integral. The term $\sin(x)$ won't cancel out either, as its derivatives are simply more sines and cosines.

Another approach which Laplace could have considered and which was commonplace at the time, would be to rewrite the $\sin(x)$ or $\cos(x)$ parts of the integrals as series expansions, in order to then invert the sum and integral symbols. Attempting this for the integral $\int_0^\infty x^{-\alpha} \sin(x) dx$ does not immediately work:

$$\begin{aligned} \int_0^\infty x^{-\alpha} \sin(x) dx &= \int_0^\infty x^{-\alpha} \left(\sum_{n=0}^{\infty} (-1)^n \frac{x^{2n+1}}{(2n+1)!} \right) dx \\ &= \sum_{n=0}^{\infty} \frac{(-1)^n}{(2n+1)!} \int_0^\infty x^{-\alpha} x^{2n+1} dx. \end{aligned}$$

As each of these individual integrals diverges, this first straightforward attempt doesn't work. We might continue our investigation by trying to approximate the desired result, for example, by integrating up to some large number instead of up to infinity, to

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then combine the individual integrals in order to obtain an approximate expression that could be generalised. Alternatively, we might look for a clever way to recombine individual terms from the series of diverging integrals to somehow get a series of converging terms. Attempting a proof along these lines was a popular approach at the time, and it turns out that other mathematicians had already tried to solve the integrals from this article in this way. The Italian mathematician Lorenzo Mascheroni in particular had already 'found' values for such an integral using an approach by series expansion [27], but Laplace would show later on in this paper that Mascheroni's findings were incorrect.

As such, finding these integrals is a non-trivial task. This could explain why Laplace bothered with using complex numbers at all; these integrals had not been solved using the standard methodology, prompting him to try some less orthodox methods.

His first step was to make the complex substitution $x := it^{\frac{1}{1-\alpha}}$, which gives $dx = \frac{1}{1-\alpha} it^{\frac{\alpha}{1-\alpha}} dt$. We then have

$$\begin{aligned} x^{-\alpha} e^{ix} dx &= (it^{\frac{1}{1-\alpha}})^{-\alpha} e^{i(it^{\frac{1}{1-\alpha}})} \frac{1}{1-\alpha} it^{\frac{\alpha}{1-\alpha}} dt \\ &= i^{-\alpha} t^{\frac{-\alpha}{1-\alpha}} e^{-t^{\frac{1}{1-\alpha}}} \frac{i}{1-\alpha} t^{\frac{\alpha}{1-\alpha}} dt \\ &= \frac{i^{1-\alpha}}{1-\alpha} e^{-t^{\frac{1}{1-\alpha}}} dx. \end{aligned}$$

Simply putting this expression as the integrand, Laplace claimed that

$$\int_0^\infty x^{-\alpha} e^{ix} dx = \frac{i^{1-\alpha}}{1-\alpha} \int_0^\infty e^{-t^{\frac{1}{1-\alpha}}} dt. \tag{2}$$

However, a non-trivial step seems to be missing in this line of reasoning. When performing a substitution, the domain of integration should be adjusted according to the variable transformation. One might think that by taking 't from 0 to infinite' [22, p. 194], Laplace meant that t goes to infinity in absolute terms, in which case he would be right; later on we will see that this is not what he meant. In contrast to Laplace's claim, by virtue of the used substitution being imaginary, the domain of integration should change from $x \in [0, \infty)$ to t on some ray $l(\alpha)$, which does not align with the positive real axis. Noting that $x = it^{\frac{1}{1-\alpha}}$ implies $t = i^{\alpha-1} x^{1-\alpha} = e^{-i\frac{\pi}{2}(1-\alpha)} x^{1-\alpha}$, we see that for $0 < \alpha < 1$ the ray $l(\alpha)$ is in the fourth quadrant of the complex plane at a negative angle of magnitude $(1-\alpha)\frac{\pi}{2}$ to the positive real axis, as in Figure 1.

As such, we would expect the new expression arising from the complex substitution $x = iz^{\frac{1}{1-\alpha}}$ to involve a line integral along this ray $l(\alpha)$, as follows:

$$\int_0^\infty x^{-\alpha} e^{ix} dx = \frac{i^{1-\alpha}}{1-\alpha} \int_{l(\alpha)} e^{-z^{\frac{1}{1-\alpha}}} dz.$$

Curiously, this oversight of Laplace's does not impact the truth of his findings. It seems that in this particular case, it doesn't matter whether we integrate along the positive real line or the ray $l(\alpha)$. However, the equality of the two integrals

$$\int_0^\infty e^{-t^{\frac{1}{1-\alpha}}} dt = \int_{l(\alpha)} e^{-z^{\frac{1}{1-\alpha}}} dz \tag{3}$$

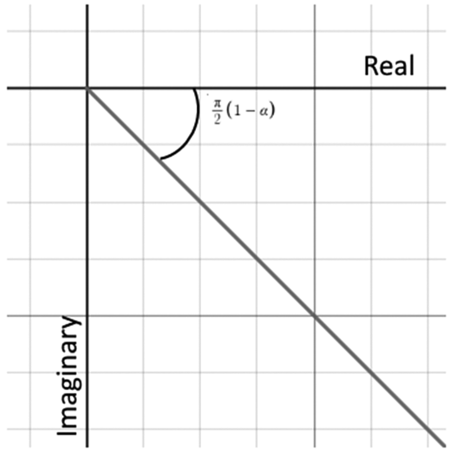

Figure 1. The domain of integration after complex substitution, for $\alpha = 0.5$.

is not a simple matter of rotational invariance. The ray $l(\alpha)$ is given by the numbers on the positive real axis 'rotated' by multiplying with $\beta := i^{\alpha-1}$, so it would be convenient if $e^{-t^{\frac{1}{1-\alpha}}}dt = e^{-(\beta t)^{\frac{1}{1-\alpha}}}(\beta dt)$. If this were the case, the proof would be finished: we'd have

$$\int_0^\infty e^{-t^{\frac{1}{1-\alpha}}}dt = \int_0^\infty e^{-(\beta t)^{\frac{1}{1-\alpha}}}(\beta dt)$$
$$= \int_{l(\alpha)} e^{-z^{\frac{1}{1-\alpha}}}dz,$$

where we used the substitution $z = \beta t$ which gives $dz = \beta dt$. Unfortunately, it's not that simple. To see that $e^{-t^{\frac{1}{1-\alpha}}} \neq e^{-(\beta t)^{\frac{1}{1-\alpha}}}\beta$, we must rewrite

$$e^{-(\beta t)^{\frac{1}{1-\alpha}}}\beta = e^{-i^{\frac{\alpha-1}{1-\alpha}}t^{\frac{1}{1-\alpha}}}e^{i(\frac{\pi}{2}(\alpha-1))}$$
$$= e^{i\left(t^{\frac{1}{1-\alpha}}+\frac{\pi}{2}(\alpha-1)\right)},$$

which is evidently a number on the unit circle in the complex plane, whereas $e^{-t^{\frac{1}{1-\alpha}}}$ takes on values on all of $\mathbb{R}_+$. Therefore, the equality (3) is not obvious, as simply comparing integrands to 'realign' $l(\alpha)$ with $\mathbb{R}_+$ gets you nowhere.

Using our modern theory of complex integration, we can, however, prove the equality of the integrals. We will use some findings by Cauchy on the integration between imaginary limits, published some years after Laplace's article; Cauchy's integral theorem in particular is essential.

We show that

$$\int_{l(\alpha)} e^{-z^{\frac{1}{1-\alpha}}}dz = \int_0^\infty e^{-t^{\frac{1}{1-\alpha}}}dt. \tag{3}$$

Consider the closed curve Γ obtained by the concatenation of three curves, defined by real constants $R > 0$ and $0 < \alpha < 1$; a first curve along the interval $[0, R]$, a second

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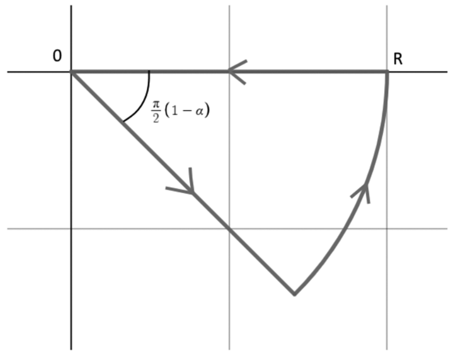

Figure 2. Γ plotted in the complex plane.

curve which we will call $l(\alpha, R)$ along $l(\alpha)$ with length R and a final curve connecting the two along an arc, as in Figure 2, with the entire contour oriented counterclockwise:

Since $t \mapsto e^{-t^{\frac{1}{1-\alpha}}}$ is analytic on all of $\mathbb{C}$, Cauchy's integral theorem tells us that the contour integral along Γ of this function is equal to 0. Using a straightforward parametrisation of the arc curve, we find

$$\begin{aligned} 0 &= \oint_\Gamma e^{-z^{\frac{1}{1-\alpha}}} dz, \\ 0 &= \int_{l(\alpha,R)} e^{-z^{\frac{1}{1-\alpha}}} dz + \int_{\frac{\pi}{2}(\alpha-1)}^{0} e^{-(Re^{i\theta})^{\frac{1}{1-\alpha}}} iRe^{i\theta} d\theta \\ &\quad + \int_R^0 e^{-t^{\frac{1}{1-\alpha}}} dt, \\ \int_0^R e^{-t^{\frac{1}{1-\alpha}}} dt &= \int_{l(\alpha,R)} e^{-z^{\frac{1}{1-\alpha}}} dz + \int_{\frac{\pi}{2}(\alpha-1)}^{0} e^{-(Re^{i\theta})^{\frac{1}{1-\alpha}}} iRe^{i\theta} d\theta. \end{aligned}$$

From this we see that when we let R approach infinity, the difference between the 'correct' integral and the one that Laplace finds is given by

$$\lim_{R\to\infty} \int_{\frac{\pi}{2}(\alpha-1)}^{0} e^{-(Re^{i\theta})^{\frac{1}{1-\alpha}}} iRe^{i\theta} d\theta,$$

since $\lim_{R\to\infty} l(\alpha, R) = l(\alpha)$. We will show that the limit above is equal to 0. First, notice that for $\alpha \in (0, 1)$, we can derive an upper bound for the absolute value of the integrand:

$$\begin{aligned} |e^{-(Re^{i\theta})^{\frac{1}{1-\alpha}}} iRe^{i\theta}| &= |e^{-R^{\frac{1}{1-\alpha}} e^{i\frac{\theta}{1-\alpha}}}| \cdot |i| \cdot |R| \cdot |e^{i\theta}| \\ &= R|e^{-R^{\frac{1}{1-\alpha}}(\cos(\frac{\theta}{1-\alpha})+i\sin(\frac{\theta}{1-\alpha}))}| \\ &= R|e^{-R^{\frac{1}{1-\alpha}}\cos(\frac{\theta}{1-\alpha})}||e^{-iR^{\frac{1}{1-\alpha}}\sin(\frac{\theta}{1-\alpha})}| \\ &= R|e^{-R^{\frac{1}{1-\alpha}}\cos(\frac{\theta}{1-\alpha})}| \\ &= R(e^{-R^{\frac{1}{1-\alpha}}\cos(\frac{\theta}{1-\alpha})}). \end{aligned}$$

Let $f_\theta(R) := R(e^{-R^{\frac{1}{1-\alpha}}\cos(\frac{\theta}{1-\alpha})})$ denote this last expression. We now wish to evaluate

$$\lim_{R\to\infty} f_\theta(R) = \lim_{R\to\infty} \frac{R}{e^{R^{\frac{1}{1-\alpha}}\cos(\frac{\theta}{1-\alpha})}}.$$

Since $\theta \in [\frac{\pi}{2}(\alpha-1), 0]$ along the domain of integration, we have $\cos(\frac{\theta}{1-\alpha}) \in (0, 1]$. Therefore, both the numerator and the denominator of $f_\theta(R)$ are real-valued functions that go to infinity as $R \to \infty$. Because of this we can use l'Hôpital's rule to find the limit $\lim_{R\to\infty} f_\theta(R)$. The derivative of the numerator is simply 1, while the derivative of the denominator is

$$\frac{d}{dR} e^{R^{\frac{1}{1-\alpha}}\cos(\frac{\theta}{1-\alpha})} = \frac{\cos(\frac{\theta}{1-\alpha})}{1-\alpha} e^{\cos(\frac{\theta}{1-\alpha})R^{\frac{1}{1-\alpha}}} R^{\frac{\alpha}{1-\alpha}}.$$

Letting $R \to \infty$, this denominator goes to infinity: we note that $\frac{\alpha}{1-\alpha} \in (0, \infty)$, so $R^{\frac{\alpha}{1-\alpha}}$ goes to infinity. In addition, $\frac{\theta}{1-\alpha} \in [-\frac{\pi}{2}, 0]$ for all θ in the domain of integration, so $\cos(\frac{\theta}{1-\alpha}) \in [0, 1]$, so $e^{\cos(\frac{\theta}{1-\alpha})R^{\frac{1}{1-\alpha}}}$ goes to infinity as well. As such, l'Hôpital's rule teaches us that the upper bound $f_\theta(R)$ for the absolute value of the integrand goes to 0 as $R \to \infty$.

Now we use the integral triangle inequality to find

$$\begin{aligned}\lim_{R\to\infty}\left|\int_{\frac{\pi}{2}(\alpha-1)}^{0} e^{-(Re^{i\theta})^{\frac{1}{1-\alpha}}} iRe^{i\theta}\,d\theta\right| &\le \lim_{R\to\infty}\int_{\frac{\pi}{2}(\alpha-1)}^{0}\left|e^{-(Re^{i\theta})^{\frac{1}{1-\alpha}}} iRe^{i\theta}\right| d\theta \\ &\le \frac{\pi}{2}(1-\alpha)\lim_{R\to\infty}\left[\sup_{\theta\in[\frac{\pi}{2}(\alpha-1),0]} f_\theta(R)\right] \\ &= 0.\end{aligned}$$

Because the absolute value of the integral goes to 0 as R grows infinitely large, the integral itself also goes to 0. As such, we have proven that for any value α between zero and one Laplace's 'mistake' doesn't matter, and we indeed have

$$\int_0^\infty e^{-t^{\frac{1}{1-\alpha}}}\,dt = \int_{l(\alpha)} e^{-z^{\frac{1}{1-\alpha}}}\,dz, \tag{3}$$

that is,

$$\lim_{R\to\infty}\int_0^R e^{-t^{\frac{1}{1-\alpha}}}\,dt = \lim_{R\to\infty}\int_{l(\alpha,R)} e^{-z^{\frac{1}{1-\alpha}}}\,dz.$$

Therefore Laplace's apparent oversight does not invalidate his findings. After the substitution, he used some specific values of α to find some improper integrals. To do this, he first wrote $k := \int_0^\infty e^{-t^{\frac{1}{1-\alpha}}}\,dt$, so that he could rewrite the original integral as $\int_0^\infty x^{-\alpha}e^{ix}\,dx = \frac{i^{1-\alpha}}{1-\alpha}k$ [22, p. 194]. For some values of α he already knew k, enabling him to solve the original integral in these cases.

But what is $i^{1-\alpha}$? As i is a value on the unit circle in the complex plane, the value $i^{1-\alpha}$ will also be on this circle. For example, taking $\alpha = \frac{1}{2}$, we have $i^{1-\alpha} = \sqrt{i} = \frac{1}{\sqrt{2}} + \frac{1}{\sqrt{2}}i$. For a general α we can write

$$i^{1-\alpha} = \cos(\phi) + i\sin(\phi)$$

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for some ϕ which depends on α. To express ϕ in terms of α, we can use the fact that $(i^{1-\alpha})^{\frac{2}{1-\alpha}} = i^2 =- 1$ to see that $(i^{1-\alpha})^{\frac{2}{1-\alpha}} = (\cos(\phi) + i\sin(\phi))^{\frac{2}{1-\alpha}} = \cos(\frac{2\phi}{1-\alpha}) + i\sin(\frac{2\phi}{1-\alpha}) =- 1$. From this it follows that $\cos(\frac{2\phi}{1-\alpha}) = \Re(-1) =- 1$, that is, $\frac{2\phi}{1-\alpha} = (2r+1)\pi$. In other words, $\phi = (2r+1)(1-\alpha)\frac{\pi}{2}$ for some $r \in \mathbb{Z}$, while we also have $\sin(\frac{2\phi}{1-\alpha}) = \Im(-1) = 0$, so $\frac{2\phi}{1-\alpha} = j\pi$, or in other words, $\phi = j(1-\alpha)\frac{\pi}{2}$ for some $j \in \mathbb{Z}$. This second condition imposes no further restriction since $2r+1$ is already an integer. Therefore the values of ϕ that yield correct values for both the real and the imaginary components of $i^{1-\alpha}$ are $\phi = (2r+1)(1-\alpha)\frac{\pi}{2}$ for $r \in \mathbb{Z}$. Thus,

$$\int_0^\infty x^{-\alpha} e^{ix}\, dx = i^{1-\alpha}\frac{k}{1-\alpha}$$
$$= \left[\cos\left((2r+1)(1-\alpha)\frac{\pi}{2}\right) + i\sin\left((2r+1)(1-\alpha)\frac{\pi}{2}\right)\right]\frac{k}{1-\alpha}.$$

Noting that $\int_0^\infty x^{-\alpha}e^{ix}dx = \int_0^\infty x^{-\alpha}(\cos(x) + i\sin(x))dx$, Laplace found, by taking real and imaginary components:

$$\int_0^\infty x^{-\alpha}\cos(x)dx = \frac{k}{1-\alpha}\cos\left((2r+1)(1-\alpha)\frac{\pi}{2}\right) \quad \text{and} \tag{4}$$

$$\int_0^\infty x^{-\alpha}\sin(x)dx = \frac{k}{1-\alpha}\sin\left((2r+1)(1-\alpha)\frac{\pi}{2}\right). \tag{5}$$

Laplace wanted to find the improper integrals above for some specific values of α. For these values he already knew k, but we are now left with the issue of choosing the right value $r \in \mathbb{Z}$. Laplace went about finding the proper choice ‘heuristically’: by analysing the behaviour of these functions on the domains of integration, he could deduce what value of r is right.

To start off his investigation of these integrals, Laplace claimed that

$$\int_0^\infty x^{-\alpha}\sin(x)dx > 0$$

for all $\alpha < 2$. Note that he did not merely take $\alpha < 1$ as before: this is because he needed the same to hold for $1+\alpha$ later on, with $0 < \alpha < 1$ as before. He demonstrated the truth of his claim by proving that the integral is positive over the interval $[0, 2\pi]$, whereafter he asserted that this proof can be carried out analogously for the interval $[2n\pi, 2(n+1)\pi]$ for all $n \in \mathbb{N}$.

To show that $\int_0^\infty x^{-\alpha}\sin(x)dx > 0$, Laplace noted that the integrand is strictly positive for $x \in [0, \pi]$. Comparing the integrals from $x = 0$ to $x = \pi$ and $x = \pi$ to $x = 2\pi$, we notice that every positive value $\frac{dx\sin(x)}{x^\alpha}$ in the first interval corresponds to a negative value $\frac{dx\sin(x+\pi)}{(x+\pi)^\alpha} =- \frac{dx\sin(x)}{(x+\pi)^\alpha}$ in the second interval, which is clearly smaller in absolute terms. Thus, summing corresponding values, the integral from 0 to 2π is positive. Laplace stated that this argument can be continued for the integral from 0 to infinity [22, p. 195], which is indeed not hard to see.

Next, Laplace noted that while $\int_0^\infty x^{-\alpha}\sin(x)$ is positive, it is smaller than the same integral taken from $x = 0$ to $x = \pi$. He motivated this by substituting

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$x = \pi + x'$; this gives $\int_\pi^\infty x^{-\alpha}\sin(x)dx = \int_0^\infty (x'+\pi)^{-\alpha}\sin(x'+\pi)dx' = -\int_0^\infty (x'+\pi)^{-\alpha}\sin(x')dx'$, which is negative by virtue of the same reasoning applied above to show that $\int_0^\infty x^{-\alpha}\sin(x)dx$ is positive. Because the integral from $x = \pi$ to x infinite is negative, but the integral from $x = 0$ to x infinite is positive overall, we must have that

$$\int_0^\pi x^{-\alpha}\sin(x)dx > \int_0^\infty x^{-\alpha}\sin(x)dx > \int_\pi^\infty x^{-\alpha}\sin(x)dx. \qquad (6)$$

Laplace then moved on to investigate the integral $\int_0^\infty x^{-\alpha}\cos(x)dx$. He noted that $\int x^{-\alpha}\cos(x)dx = \frac{\sin(x)}{x^\alpha} + \alpha\int \frac{\sin(x)}{x^{\alpha+1}}dx$, which can be derived directly using integration by parts. He noted that by integrating over $x \in [0,\infty)$ this expression becomes $\int_0^\infty x^{-\alpha}\cos(x)dx = \frac{\sin(x)}{x^\alpha}\Big|_{x=0}^{x=\infty} + \alpha\int_0^\infty \frac{\sin(x)}{x^{\alpha+1}}dx$. Laplace stated that the term $\frac{\sin(x)}{x^\alpha}\Big|_{x=0}^{x=\infty}$ is zero, but did not demonstrate this. Using l'Hôpital's rule, we can see for ourselves that

$$\begin{aligned}\frac{\sin(x)}{x^\alpha}\Big|_{x=0}^{x=\infty} &= \lim_{x\to\infty}\frac{\sin(x)}{x^\alpha} - \lim_{x\to 0}\frac{\sin(x)}{x^\alpha}\\ &= -\lim_{x\to 0}\frac{\cos(x)}{\alpha x^{\alpha-1}}\\ &= 0,\end{aligned}$$

where we used the fact that $\alpha - 1 < 0$. We have thus found that $\int_0^\infty x^{-\alpha}\cos(x)dx = \alpha\int_0^\infty \frac{\sin(x)}{x^{\alpha+1}}dx = \alpha\int_0^\infty \sin(x)x^{-(\alpha+1)}dx$. Now, because $1+\alpha < 2$, Laplace was able to use his previous result to conclude that this last integral must be positive and finite. Consequently, the integral $\int_0^\infty x^{-\alpha}\cos(x)dx$ is positive and finite as well.

As he did with the sine integral, Laplace showed that the significant positive part of this entire integral occurs in the leftmost part of the domain: By substituting $x = \frac{\pi}{2} + x'$ into the integral $\int_{\frac{\pi}{2}}^\infty \frac{\cos(x)}{x^\alpha}dx$, we get

$$\begin{aligned}\int_{\frac{\pi}{2}}^\infty \frac{\cos(x)}{x^\alpha}dx &= \int_0^\infty \frac{\cos(x'+\frac{\pi}{2})}{(x'+\frac{\pi}{2})^\alpha}dx'\\ &= -\int_0^\infty \frac{\sin(x')}{(x'+\frac{\pi}{2})^\alpha}dx'\\ &< 0,\end{aligned}$$

but the integral from 0 to infinity is positive, so we must have

$$\int_0^{\frac{\pi}{2}} \frac{\cos(x)}{x^\alpha}dx > \int_0^\infty \frac{\cos(x)}{x^\alpha}dx > \int_{\frac{\pi}{2}}^\infty \frac{\cos(x)}{x^\alpha}dx. \qquad (7)$$

With these results out of the way, Laplace returned to equations (4) and (5). He was interested in the case where $1-\alpha$ is 'infinitely small', that is, he let $\alpha \to 1$ [22, p. 196]. Laplace stated that in this case, equation (5) gives

$$\int_0^\infty \frac{\sin(x)}{x}dx = (2r+1)\frac{\pi}{2}k$$

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for some $r \in \mathbb{Z}$. This isn't immediately obvious, as Laplace did not explain why $\lim_{\alpha\to 1} \frac{k}{1-\alpha} \sin((2r+1)(1-\alpha)\frac{\pi}{2}) = (2r+1)\frac{\pi}{2}k$. This can be seen by applying l'Hôpital's rule:

$$\begin{aligned}\lim_{\alpha\to 1} \frac{k}{1-\alpha} \sin\left((2r+1)(1-\alpha)\frac{\pi}{2}\right) &= \left(\lim_{\alpha\to 1} k\right) \lim_{\alpha\to 1} \frac{\sin((2r+1)(1-\alpha)\frac{\pi}{2})}{1-\alpha} \\ &= \left(\lim_{\alpha\to 1} k\right) \lim_{\alpha\to 1} -\frac{(2r+1)\frac{\pi}{2}\cos((2r+1)(1-\alpha)\frac{\pi}{2})}{-1} \\ &= \left(\lim_{\alpha\to 1} k\right)(2r+1)\frac{\pi}{2},\end{aligned}$$

which is our desired result, given that Laplace simply wrote $k = \lim_{\alpha\to 1} k$. Now, to derive the value of k when $\alpha \to 1$, we consider the integrand of $k = \int_0^\infty e^{-t^{\frac{1}{1-\alpha}}} dt$. As $\alpha \to 1$, we have $\frac{1}{1-\alpha} \to \infty$, so depending on the value of t we can determine the value of the integrand for every point on the domain of integration. To see this, notice that for $t \in [0, 1)$ we have $\lim_{\alpha\to 1} e^{-t^{\frac{1}{1-\alpha}}} = 1$, and for $t \in (1, \infty)$ we have $\lim_{\alpha\to 1} e^{-t^{\frac{1}{1-\alpha}}} = 0$. For $t = 1$ the integrand is always equal to e^{-1}, but this is inconsequential to the value of the integral as it's just one point in the domain with no continuous neighbourhood. As such, we see that the integrand of k becomes a sort of step function as $\alpha \to 1$, starting with value 1 and dropping to 0 after $t = 1$; we thus have $\lim_{\alpha\to 1} k = 1$.

It is worth stopping here to consider what this reasoning tells us about Laplace's thinking. The outlined approach follows Laplace's article closely, thus showing that he indeed did not think of k as an integral along a diagonal ray in the complex plane, but as an integral along the positive real axis. Indeed, to evaluate it he only considers values of t on the real line. Now that we know definitively that Laplace did not transform his domain, we can wonder why he made this mistake and if he was aware of it: we will return to these questions later on.

Laplace then substituted the value he deduced for k to find that $\int_0^\infty \frac{\sin(x)}{x} dx = (2r+1)\frac{\pi}{2}$ for some $r \in \mathbb{Z}$. Next, he used the upper bounds (6) he had derived previously to determine the correct choice of r. He noted that

$$\begin{aligned}\int_0^\infty \frac{\sin(x)}{x} dx &< \int_0^\pi \frac{\sin(x)}{x} dx \\ &< \int_0^\pi \frac{x}{x} dx \\ &= \pi,\end{aligned}$$

where he used the fact that $\sin(x) < x$ on $(0, \pi]$. Therefore $(2r+1)\frac{\pi}{2} \in [0, \pi]$, so the only choice for r that makes sense is $r = 0$. Laplace had thus found that $\int_0^\infty \frac{\sin(x)}{x} dx = \frac{\pi}{2}$, which is indeed correct.

Laplace noted that we then also find $\int_0^\infty \frac{\cos(x)}{x}$ to be infinite, 'as expected' [22, p. 196]. This can be seen by filling in $r = 0$ in equation (4); letting $\alpha \to 1$, the integral diverges.

He continued by considering $\alpha = \frac{1}{2}$. In this case, we have $k = \int_0^\infty e^{-t^2} dt$. Laplace noted [22, p. 196] that he already considered this integral in a publication from 1782,

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wherein he derived that $\int_0^\infty e^{-t^2}dt = \frac{1}{2}\sqrt{\pi}$ [21, p. 223]. Filling in these values for k and α in equations (4) and (5), we get

$$\int_0^\infty \frac{\cos(x)}{\sqrt{x}}dx = \sqrt{\pi}\cos\left(\frac{(2r+1)\pi}{4}\right),$$
$$\int_0^\infty \frac{\sin(x)}{\sqrt{x}}dx = \sqrt{\pi}\sin\left(\frac{(2r+1)\pi}{4}\right).$$

We have seen earlier that both of these integrals should be positive, so we want both the sine and cosine of $\frac{(2r+1)\pi}{4}$ to be positive. This only happens when $\frac{(2r+1)\pi}{4} \in [0, \frac{\pi}{2}]$, or in the same interval but shifted by 2π, that is, when (again) $r = 0$ or when r is a multiple of 4. In this case, we have

$$\cos\left(\frac{(2r+1)\pi}{4}\right) = \sin\left(\frac{(2r+1)\pi}{4}\right) = \frac{1}{\sqrt{2}},$$

so

$$\int_0^\infty \frac{\cos(x)}{\sqrt{x}}dx = \int_0^\infty \frac{\sin(x)}{\sqrt{x}}dx = \sqrt{\frac{\pi}{2}}.$$

Here, Laplace referred to a work by the Italian mathematician Lorenzo Mascheroni, wherein Mascheroni found that $\int_0^\infty \frac{\cos(x)}{\sqrt{x}}dx = \sqrt{2\pi}$ [27, p. 57]. Laplace thus rejected these findings, and easily demonstrated why Mascheroni's result could not be true. Using (7), he noted that

$$\begin{aligned}\int_0^\infty \frac{\cos(x)}{\sqrt{x}}dx &< \int_0^{\frac{\pi}{2}} \frac{\cos(x)}{\sqrt{x}}dx \\ &< \int_0^{\frac{\pi}{2}} \frac{dx}{\sqrt{x}} \\ &= [2\sqrt{x}]_{x=0}^{x=\frac{\pi}{2}} \\ &= \sqrt{2\pi},\end{aligned}$$

so Mascheroni indeed overestimated the integral. In this case, Laplace's 'passage from the real to the imaginary' is arguably more successful than the usual strictly real methods used by earlier mathematicians like Mascheroni.

Before we move on to the next section, it's worth stopping to discuss whether it is possible that Laplace did know about his 'oversight'. The article suggests that Laplace either didn't recognise the shakiness of his argument, or that he simply didn't care. We have already seen that he did intend for (2) to mean integration along $\mathbb{R}_+$. This suggests a few potential explanations for Laplace's thinking.

First, we could suppose that Laplace legitimately believed that the domain of integration does not change. Historically, one could argue that this makes sense. It is probable that Laplace was aware of the geometric interpretation of complex numbers in the plane, as discussion of this notion can be found in the work of Wessel and more notably Argand [15]. However, it is unlikely that Laplace thought about his domain transformation in such a geometric sense, involving line integrals; rather, it is apparent that he thought about integration in terms of intervals defined by bounds, not along lines in some space. He only ever discussed transformation of integration domains by stating the new endpoints—in Laplace's words, integrals are always taken 'from $x = a$ to $x = b$'. Therefore, the substitution he used would not seem to him to transform the integral along one dimension into a multi-dimensional line integral: it was some years before Cauchy published his work on integration between imaginary limits [9]. If we view the complex unit i as just another constant, it makes sense to assume that $i[0, \infty) = [0, \infty)$, as this is actually true for any non-negative real constant.

Alternatively, one could hypothesise that Laplace should have at least hesitated to believe that the transformation is as straightforward as he claimed in his finalised work. In his time it would not be possible to just check his results using a computer, but there were numerical methods for the approximation of integrals. It is conceivable that Laplace may have carried out some manual computation to approximate the results he should be getting. Seeing how his approximations match up with the values he finds later on, this could have reaffirmed Laplace's trust in the shaky methods he was using, prompting him to just write the methods off as correct.

A final hypothesis is that Laplace was fully aware of the necessary complex transformation, but he deemed it obvious enough to leave unexplained why the domain of integration doesn't matter. This seems unlikely, as we have shown earlier that simple intuitive arguments of rotational invariance do not work here. We had to employ theory which would be developed by Cauchy some years after Laplace's publication, which seems a bit out of reach for Laplace, and definitely not obvious enough to leave out of his written proof. Of course, it is possible that Laplace found another approach which does not rely on Cauchy's integral theorem, but we would expect this method to be explained in the article, seeing how thorough he was in the rest of the work.

The phrasing used in the text does not help us much. After performing the transformation, in the usual matter-of-fact style that is common in both nineteenth century as well as present-day mathematics, Laplace merely wrote that 'by taking the first integral from $x = 0$ to x infinite; the second integral should be taken from $t = 0$ to t infinite' [22, p. 194], the brevity of which suggests that he did not think about the domain change at all.

As such, there is no evidence to suggest that Laplace was aware of his mistake: he evidently deemed his incomplete proof to be perfectly valid. Fortunately, we have seen that his results were still correct.

§3. Poisson's *Sur les intégrales définies*.

A lot of Poisson's work was published in response to publications by others, something for which he has been criticised as having 'no ideas of his own' [10, p. 483]. According to the same biography, Poisson's contributions to mathematics have thus been characterised [10, p. 490] by his development of established theory through his talent for calculation: we will see this talent for calculation amply displayed in this section.

In Poisson's article *Sur les intégrales définies*, he gave justifications for the integrals computed by Laplace in [22], but he insisted on using strictly real methods, or 'direct proofs' [30, p. 243]. Rather than addressing the mistake we found—which he did not seem to notice either—Poisson objected to the fact that Laplace used complex numbers in the first place, deeming this method 'indirect'. Poisson's alternative proof is long and pretty tedious, involving more trigonometry, more coordinate transformations and more other integrals along the way, but it does achieve what Poisson set out to do:

to prove the same results as Laplace, without ever resorting to the use of complex numbers.

Laplace had previously found that

$$\int_0^\infty x^{-\alpha}\cos(x)dx = \frac{k}{1-\alpha}\cos\left((2r+1)(1-\alpha)\frac{\pi}{2}\right), \tag{4}$$

$$\int_0^\infty x^{-\alpha}\sin(x)dx = \frac{k}{1-\alpha}\sin\left((2r+1)(1-\alpha)\frac{\pi}{2}\right), \tag{5}$$

where $k = \int_0^\infty e^{-t^{\frac{1}{1-\alpha}}}$, $\alpha \in (0,1)$, and $r \in \mathbb{Z}$. From this, he had found values for the two integrals by choosing proper values of r, where he chose r to fit the expected behaviour of the functions involved. It turned out that r should be chosen to be zero for the values of α that Laplace considered. All the complex number reasoning happens in the derivation of these two identities (4) and (5), and all the work afterwards uses ordinary real methods. As such, Poisson was interested in finding 'direct proofs' of the identities above. Because $r = 0$ in the rest of Laplace's proof, Poisson would derive the identities with this value of r, yielding the following identities:

$$\begin{aligned}\int_0^\infty x^{-\alpha}\cos(x)dx &= \frac{k}{1-\alpha}\cos\left((1-\alpha)\frac{\pi}{2}\right)\\ &= \frac{k}{1-\alpha}\cos\left(\frac{\pi}{2}-\alpha\frac{\pi}{2}\right)\\ &= \frac{k}{1-\alpha}\sin\left(\frac{\alpha\pi}{2}\right),\end{aligned} \tag{8}$$

$$\begin{aligned}\int_0^\infty x^{-\alpha}\sin(x)dx &= \frac{k}{1-\alpha}\sin\left((1-\alpha)\frac{\pi}{2}\right)\\ &= \frac{k}{1-\alpha}\sin\left(\frac{\pi}{2}-\alpha\frac{\pi}{2}\right)\\ &= \frac{k}{1-\alpha}\cos\left(\frac{\alpha\pi}{2}\right).\end{aligned} \tag{9}$$

However, before proving these identities, Poisson had to lay some groundwork. He stated that 'in order to bring together under a single point of view what has so far been found to be most general about definite integrals, we shall begin by dealing with those which contain exponentials' [30, p. 244]. He considered the integral[7] $\int_0^\infty e^{-x^n}x^{p-1}dx$ wherein p and n are positive integers. For his purposes, Poisson considered n to be a given quantity, whereafter he wanted to know what happens when p changes. To reflect this, he considered the value of this integral ϕ as a function of p:

$$\phi(p) := \int_0^\infty e^{-x^n}x^{p-1}dx, \tag{10}$$

whereafter he continued his investigation of this function ϕ. Using integration by parts, he rewrote

[7] Poisson used an interesting notation for infinity. Where Laplace wrote 'x infinite' to denote that x goes to infinity, Poisson wrote $x = \frac{1}{0}$.

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$$\begin{aligned}\phi(p) &= \int_0^\infty e^{-x^n} x^{p-1} dx \\ &= \frac{1}{p}\, e^{-x^n} x^p \Big|_{x=0}^{x=\infty} + \frac{n}{p} \int_0^\infty e^{-x^n} x^{p+n-1} dx.\end{aligned}$$

Poisson claimed that the first term vanishes when evaluated at the specified endpoints. It's easy to see that $\frac{1}{p} e^{-x^n} x^p \Big|_{x=0} = 0$, and we can see that $\frac{1}{p} e^{-x^n} x^p \Big|_{x\to\infty} = 0$ by repeated application of l'Hôpital's rule. To simplify some steps, we use the fact that $e^{-x^n} < e^{-x}$ for all $x > 1$.

$$\begin{aligned}\frac{1}{p} e^{-x^n} x^p \Big|_{x\to\infty} &= \frac{1}{p} \lim_{x\to\infty} \frac{x^p}{e^{x^n}} \\ &\leq \frac{1}{p} \lim_{x\to\infty} \frac{x^p}{e^x} \\ &= \frac{1}{p} \lim_{x\to\infty} \frac{p x^{p-1}}{e^x} \\ &= \frac{1}{p} \lim_{x\to\infty} \frac{p(p-1) x^{p-2}}{e^x} \\ &\ldots \\ &= \frac{1}{p} \lim_{x\to\infty} \frac{p!}{e^x} \\ &= 0.\end{aligned}$$

Because $\frac{1}{p} e^{-x^n} x^p$ is non-negative for any $x \in [0, \infty)$, we then have

$$0 \leq \lim_{x\to\infty} \frac{1}{p} e^{-x^n} x^p \leq 0,$$

so $\lim_{x\to\infty} \frac{1}{p} e^{-x^n} x^p = 0$. We have thus found the following expression for $\phi(p)$:

$$\begin{aligned}\phi(p) &= \frac{n}{p} \int_0^\infty e^{-x^n} x^{p+n-1} dx \\ &= \frac{n}{p} \phi(p+n).\end{aligned} \tag{11}$$

Poisson noted that for any $p > n$, he could repeatedly subtract n from p; let's say $m \in \mathbb{Z}$ times, until we are left with a number r smaller than n. Using a different notation, we can write $p \equiv r (\text{mod } n)$, or $p = mn + r$. The equality (11) then teaches us that $\phi(r) = \frac{n}{r}\phi(r+n) = \frac{n}{r}(\frac{n}{r+n}\phi(r+2n)) = \cdots = \frac{n^m}{r(r+n)\ldots(r+(m-1)n)}\phi(r+mn)$, or that $\phi(p) = \frac{r(r+n)\ldots(r+(m-1)n)}{n^m}\phi(r)$. As such, for any $p > n$ we can find $\phi(p)$ from $\phi(r)$ where r is the remainder of p after division by n, that is, a number smaller than n. As Poisson noted, the scope of our investigation of ϕ is now significantly smaller: we only have to find the values of $\phi(p)$ for $p \leq n$.

It turns out that $\phi(n)$ is readily found. Substituting $p = n$ into the definition (10) of ϕ, Poisson claimed that[8]

$$\phi(n) = \int_0^\infty e^{-x^n} x^{n-1} dx$$
$$= \frac{1}{n} e^{-x^n} \Big|_{x=0}^{x=\infty}.$$

Now, the only values of p left to consider when investigating $\phi(p)$ are those for which $p < n$. Poisson continued his investigation using double integrals. He introduced a new value q and takes its ϕ-value $\phi(q) = \int_0^\infty e^{-y^n} y^{q-1} dy$, wherein he used y as a dummy variable instead of x as in equation (10) to distinguish between $\phi(p)$ and $\phi(q)$. He notes that

$$\phi(p)\phi(q) = \int_0^\infty e^{-x^n} x^{p-1} dx \int_0^\infty e^{-y^n} y^{q-1} dy$$
$$= \iint_V e^{-(x^n+y^n)} x^{p-1} y^{q-1} dxdy,$$

where V denotes the first quadrant of $\mathbb{R}^2$, that is, $V := [0, \infty) \times [0, \infty)$. Poisson then substituted $y = xz$, which gives $dy = xdz$. He stated that this substitution is well-defined because 'integration relative to y supposes that x is held constant' [30, p. 245], which ensures that $y = xz$ is still uniquely defined when integrating y even though x is a variable quantity. Because x and y are both positive on the entire domain of integration,[9] we have $z \in [0, \infty)$ as well:

$$\phi(p)\phi(q) = \iint_{[0,\infty)\times[0,\infty)} e^{-(x^n+y^n)} x^{p-1} y^{q-1} dxdy$$
$$= \iint_V e^{-(x^n+(xz)^n)} x^{p-1} (xz)^{q-1} dx(xdz)$$
$$= \iint_V e^{-x^n(1+z^n)} x^{p+q-1} z^{q-1} dxdz.$$

Next, we perform another change of variables. Let $x = t(1+z^n)^{-\frac{1}{n}}$, which gives $dx = dt(1+z^n)^{-\frac{1}{n}}$, with $t \in [0, \infty)$. Then

---

8 There is a typo here in the antiderivative of $e^{-x^n} x^{n-1}$. The antiderivative $\frac{1}{n} e^{-x^n}$ is incorrect: using the chain rule we see that $\frac{d}{dx} \frac{1}{n} e^{-x^n} =- e^{-x^n} x^{n-1}$. As such, the correct antiderivative is $-\frac{1}{n} e^{-x^n}$. Poisson knew the correct anti-derivative, it seems, because he goes on to state that 'because of the limits $x = 0$ and x infinite, we have $\phi(n) = \frac{1}{n}$' [30, p. 245]. However, if we would evaluate the expression for $\phi(n)$ as the text suggests it, we would find $\phi(n) = \lim_{x\to\infty} \left[\frac{1}{n} e^{-x^n}\right] - \left[\frac{1}{n} e^{-x^n}\right]_{x=0} =- \frac{1}{n}$; if we use the correct antiderivative $-\frac{1}{n} e^{-x^n}$, we indeed get

$$\phi(n) = \frac{1}{n}. \tag{12}$$

It is thus probable that Poisson did find the correct antiderivative in his own notes, but that he or some editor missed a minus sign when writing the article.

9 Of course, $x = 0$ at the left end of the domain, but we can equivalently take the domain to be $(0, \infty)$.

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$$\begin{aligned}\phi(p)\phi(q) &= \iint_V e^{-x^n(1+z^n)} x^{p+q-1} z^{q-1} dx dz \\ &= \iint_V e^{-\left(\frac{t}{\sqrt[n]{1+z^n}}\right)^n (1+z^n)} \left(\frac{t}{\sqrt[n]{1+z^n}}\right)^{p+q-1} z^{q-1} \left(\frac{dt}{\sqrt[n]{1+z^n}}\right) dz \\ &= \iint_V e^{-t^n} t^{p+q-1} z^{q-1} (1+z^n)^{-\frac{p+q}{n}} dt dz.\end{aligned}$$

This last double integral can be rewritten as the product of two integrals, yielding the following identity:

$$\begin{aligned}\phi(p)\phi(q) &= \iint_V e^{-t^n} t^{p+q-1} z^{q-1} (1+z^n)^{-\frac{p+q}{n}} dt dz \\ &= \int_0^\infty e^{-t^n} t^{p+q-1} dt \int_0^\infty \frac{z^{q-1}}{(1+z^n)^{\frac{p+q}{n}}} dz \\ &= \phi(p+q) \int_0^\infty \frac{z^{q-1}}{(1+z^n)^{\frac{p+q}{n}}} dz, \qquad (13)\end{aligned}$$

so the product of ϕ-values $\phi(p)\phi(q)$ can be deduced from the ϕ-value of the sum $p+q$, provided that we can solve the integral $\int_0^\infty \frac{z^{q-1}}{(1+z^n)^{\frac{p+q}{n}}} dz$. To solve this integral, Poisson made yet another substitution, letting $1+z^n = \frac{1}{1-x^n}$. This gives $z = \frac{x}{1-x^n}$ and $dz = \frac{dx}{(1-x^n)\sqrt[n]{1-x^n}}$, and for $z \in [0, \infty)$ we have $x \in (0, 1)$. With this substitution, we find a new integral that depends on p and q, for which Poisson introduced the notation $\left(\frac{q}{p}\right)$, which was first used by Euler [30, p. 246]:

$$\begin{aligned}\int_0^\infty \frac{z^{q-1}}{(1+z^n)^{\frac{p+q}{n}}} dz &= \int_0^1 \frac{x^{q-1}}{(1-x^n)^{\frac{q-1}{n}}} (1-x^n)^{\frac{p+q}{n}} (1-x^n)^{-\frac{n+1}{n}} dx \\ &= \int_0^1 x^{q-1} (1-x^n)^{\frac{1-q+p+q-n-1}{n}} dx \\ &= \int_0^1 \frac{x^{q-1}}{\sqrt[n]{(1-x^n)^{n-p}}} dx \qquad (14) \\ &=: \left(\frac{q}{p}\right).\end{aligned}$$

Using this notation, equation (13) becomes

$$\phi(p)\phi(q) = \phi(p+q)\left(\frac{q}{p}\right). \qquad (13)$$

Note that because $\phi(p+q) = \phi(q+p)$ and $\phi(p)\phi(q) = \phi(q)\phi(p)$, we have $\left(\frac{q}{p}\right) = \left(\frac{p}{q}\right)$. For some particular values of q and p, we can find $\left(\frac{q}{p}\right)$. If we take for example $p = n$ and q arbitrary, we have

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$$
\begin{aligned}
\left(\frac{q}{p}\right) &= \left(\frac{q}{n}\right) \\
&= \int_0^1 \frac{x^{q-1}}{\sqrt[n]{(1-x^n)^{n-n}}}dx \\
&= \int_0^1 x^{q-1}dx \\
&= \left[\frac{1}{q}x^q\right]_{x=0}^{x=1} \\
&= \frac{1}{q},
\end{aligned}
\tag{15}
$$

and if we take $p+q=n$, or $p=n-q$, we find

$$
\begin{aligned}
\left(\frac{q}{p}\right) &= \int_0^1 \frac{x^{q-1}}{\sqrt[n]{(1-x^n)^{n-p}}}dx \\
&= \int_0^\infty \frac{z^{q-1}}{(1+z^n)^{\frac{p+q}{n}}}dz \\
&= \int_0^\infty \frac{z^{q-1}}{(1+z^n)^{\frac{(n-q)+q}{n}}}dz \\
&= \int_0^\infty \frac{z^{q-1}}{1+z^n}dz,
\end{aligned}
$$

where we reversed the transformation used to obtain (14). Poisson did not solve this last integral himself, but instead cited Lacroix [20, p. 411] who had found the value of this integral to be

$$
\int_0^\infty \frac{z^{q-1}}{1+z^n}dz = \frac{\pi}{n\sin(\frac{q\pi}{n})},
\tag{16}
$$

so

$$
\left(\frac{q}{n-q}\right) = \frac{\pi}{n\sin(\frac{q\pi}{n})}.
\tag{17}
$$

Combining equations (12), (13) and (17),

$$
\begin{aligned}
\phi(p)\phi(n-p) &= \phi(p+n-p)\left(\frac{p}{n-p}\right) \\
&= \phi(n)\frac{\pi}{n\sin(\frac{p\pi}{n})} \\
&= \frac{\pi}{n^2\sin(\frac{p\pi}{n})},
\end{aligned}
\tag{18}
$$

where we used the fact that $(n-p)+p=n$, enabling us to apply equation (17).

Poisson spent some more time discussing how to find values of ϕ, but these results were not used in the derivation of Laplace's equations. Therefore we will not go into these parts of the article, instead of moving on to the first introduction of integrals

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involving sines and cosines. Poisson introduced another function of p for one such integral:

$$\psi(p) := \int_0^\infty x^{p-1} \cos(a + x^n) dx, \tag{19}$$

wherein $n \in \mathbb{N}$ is a given positive integer, while $a \in \mathbb{R}$ is a given real constant. Next, he multiplied $\psi(p)$ by $\phi(n-p)$ to get

$$\begin{aligned}\phi(n-p)\psi(p) &= \int_0^\infty e^{-y^n} y^{n-p-1} dy \int_0^\infty x^{p-1} \cos(a + x^n) dx \\ &= \iint_V e^{-y^n} y^{n-p-1} x^{p-1} \cos(a + x^n) dx dy,\end{aligned}$$

wherein we again let V denote $[0, \infty) \times [0, \infty)$. Poisson performed a change of variables by substituting $y = xz$, which yields $dy = xdz$. The domain of integration for z is still $[0, \infty)$:

$$\begin{aligned}\phi(n-p)\psi(p) &= \iint_V e^{-(xz)^n} (xz)^{n-p-1} x^{p-1} \cos(a + x^n) dx (xdz) \\ &= \iint_V e^{-x^n z^n} z^{n-p-1} x^{n-1} \cos(a + x^n) dx dz. \end{aligned} \tag{20}$$

Poisson approached this expression by 'first integrating relative to x' [30, p. 249]. He evaluated the integral $\int e^{-x^n z^n} x^{n-1} \cos(a + x^n) dx$, which we will denote by I. Using integration by parts twice, he found

$$\begin{aligned}I &= \frac{1}{n} e^{-x^n z^n} \sin(a + x^n) \\ &\quad + z^n \int e^{-x^n z^n} x^{n-1} \sin(a + x^n) dx, \\ &= \frac{1}{n} e^{-x^n z^n} \sin(a + x^n) - \frac{z^n}{n} e^{-x^n z^n} \cos(a + x^n) \\ &\quad - z^{2n} \int e^{-x^n z^n} x^{n-1} \cos(a + x^n) dx, \\ &= \frac{1}{n} e^{-x^n z^n} \sin(a + x^n) - \frac{z^n}{n} e^{-x^n z^n} \cos(a + x^n) - z^{2n} I,\end{aligned}$$

and since this last integral equals the one we started with, solving for I gives

$$I = \int e^{-x^n z^n} x^{n-1} \cos(a + x^n) dx = \frac{e^{-x^n z^n}}{n(1 + z^{2n})} \left(\sin(a + x^n) - z^n \cos(a + x^n)\right).$$

We can then evaluate the definite integral

$$\begin{aligned}\int_0^\infty e^{-x^n z^n} x^{n-1} \cos(a + x^n) dx &= \lim_{x \to \infty} \frac{e^{-x^n z^n}}{n(1 + z^{2n})} \left(\sin(a + x^n) - z^n \cos(a + x^n)\right) \\ &\quad - \lim_{x \to 0} \frac{e^{-x^n z^n}}{n(1 + z^{2n})} \left(\sin(a + x^n) - z^n \cos(a + x^n)\right) \\ &= 0 - \frac{1}{n(1 + z^{2n})} (\sin(a) - z^n \cos(a)) \\ &= \frac{z^n \cos(a) - \sin(a)}{n(1 + z^{2n})}.\end{aligned}$$

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Substituting this back into the original double integral (20), we find

$$\begin{aligned}\phi(n-p)\psi(p) &= \iint_V e^{-x^n z^n} z^{n-p-1} x^{n-1}\cos(a+x^n)\,dx\,dz \\ &= \int_0^\infty z^{n-p-1}\left(\int_0^\infty e^{-x^n z^n} x^{n-1}\cos(a+x^n)\,dx\right)dz \\ &= \int_0^\infty z^{n-p-1}\left(\frac{z^n\cos(a)-\sin(a)}{n(1+z^{2n})}\right)dz \\ &= \frac{\cos(a)}{n}\int_0^\infty \frac{z^{2n-p-1}}{1+z^{2n}}dz - \frac{\sin(a)}{n}\int_0^\infty \frac{z^{n-p-1}}{1+z^{2n}}dz,\end{aligned}$$

which we can rewrite using equation (16):

$$\begin{aligned}\phi(n-p)\psi(p) &= \frac{\cos(a)}{n}\int_0^\infty \frac{z^{2n-p-1}}{1+z^{2n}}dz - \frac{\sin(a)}{n}\int_0^\infty \frac{z^{n-p-1}}{1+z^{2n}}dz \\ &= \frac{\cos(a)}{n}\cdot\frac{\pi}{2n\sin\left(\frac{(2n-p)\pi}{2n}\right)} - \frac{\sin(a)}{n}\cdot\frac{\pi}{2n\sin\left(\frac{(n-p)\pi}{2n}\right)} \\ &= \frac{\cos(a)}{n}\cdot\frac{\pi}{2n\sin(\frac{p\pi}{2n})} - \frac{\sin(a)}{n}\cdot\frac{\pi}{2n\cos(\frac{p\pi}{2n})} \\ &= \frac{\pi}{2n^2}\left(\frac{\cos(a)}{\sin(\frac{p\pi}{2n})} - \frac{\sin(a)}{\cos(\frac{p\pi}{2n})}\right).\end{aligned}$$

Multiplying both sides by $\phi(p)$ and applying equation (18), Poisson found that

$$\begin{aligned}\phi(p)\phi(n-p)\psi(p) &= \frac{\pi\phi(p)}{2n^2}\left(\frac{\cos(a)}{\sin(\frac{p\pi}{2n})} - \frac{\sin(a)}{\cos(\frac{p\pi}{2n})}\right), \\ \frac{\pi}{n^2\sin(\frac{p\pi}{n})}\psi(p) &= \frac{\pi\phi(p)}{2n^2}\left(\frac{\cos(a)}{\sin(\frac{p\pi}{2n})} - \frac{\sin(a)}{\cos(\frac{p\pi}{2n})}\right), \\ \psi(p) &= \phi(p)\sin\left(\frac{p\pi}{n}\right)\left(\frac{\cos(a)}{2\sin(\frac{p\pi}{2n})} - \frac{\sin(a)}{2\cos(\frac{p\pi}{2n})}\right).\end{aligned}$$

By a trigonometric sum formula, we have $\sin\left(\frac{p\pi}{n}\right) = 2\cos\left(\frac{p\pi}{2n}\right)\sin\left(\frac{p\pi}{2n}\right)$, so

$$\begin{aligned}\psi(p) &= \phi(p)\sin\left(\frac{p\pi}{n}\right)\left(\frac{\cos(a)}{2\sin(\frac{p\pi}{2n})} - \frac{\sin(a)}{2\cos(\frac{p\pi}{2n})}\right) \\ &= \phi(p)\left(2\cos\left(\frac{p\pi}{2n}\right)\sin\left(\frac{p\pi}{2n}\right)\right)\left(\frac{\cos(a)}{2\sin(\frac{p\pi}{2n})} - \frac{\sin(a)}{2\cos(\frac{p\pi}{2n})}\right) \\ &= \phi(p)\left(\cos(a)\cos\left(\frac{p\pi}{2n}\right) - \sin(a)\sin\left(\frac{p\pi}{2n}\right)\right). \qquad (21)\end{aligned}$$

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Using the trigonometric difference formula $\cos(\alpha+\beta)=\cos(\alpha)\cos(\beta)-\sin(\alpha)\sin(\beta)$, Poisson also derived another expression for $\psi(p)$ from its definition (19):

$$\begin{aligned}\psi(p) &= \int_0^\infty x^{p-1}\cos(a+x^n)dx \\ &= \int_0^\infty x^{p-1}\left[\cos(a)\cos(x^n)-\sin(a)\sin(x^n)\right]dx \\ &= \cos(a)\int_0^\infty x^{p-1}\cos(x^n)dx - \sin(a)\int_0^\infty x^{p-1}\sin(x^n)dx. \end{aligned} \tag{22}$$

Now, Poisson claimed that by 'equating on both sides the terms that contain $\cos(a)$ and those that contain $\sin(a)$' [30, p. 251], we can combine equations (21) and (22) to get

$$\begin{aligned}\int_0^\infty x^{p-1}\cos(x^n)dx &= \cos\left(\frac{p\pi}{2n}\right)\phi(p) \\ &= \cos\left(\frac{p\pi}{2n}\right)\int_0^\infty e^{-y^n}y^{p-1}dy,\end{aligned} \tag{23}$$

$$\begin{aligned}\int_0^\infty x^{p-1}\sin(x^n)dx &= \sin\left(\frac{p\pi}{2n}\right)\phi(p) \\ &= \sin\left(\frac{p\pi}{2n}\right)\int_0^\infty e^{-y^n}y^{p-1}dy.\end{aligned} \tag{24}$$

'Equating terms on both sides' boils down to claiming that $\cos(a)\alpha+\sin(a)\beta=\cos(a)\gamma+\sin(a)\delta$ implies $\alpha=\gamma$ and $\beta=\delta$. This is generally not true for fixed a, but a is arbitrary in the definition of $\psi(p)$, enabling us to deduce the result. By setting $a=0$ we have $\cos(a)=1$ and $\sin(a)=0$, so we must have $\alpha=\gamma$, and setting $a=\frac{\pi}{2}$ we have $\cos(a)=0$ and $\sin(a)=1$ so we get $\beta=\delta$.[10] Poisson's argument thus checks out, so equations (23) and (24) are valid.

Next, Poisson substituted $x=z^{\frac{1}{n}}$, $dx=\frac{1}{n}z^{\frac{1}{n}-1}dz$, $y=t^{\frac{1}{p}}$ and $dy=\frac{1}{p}t^{\frac{1}{p}-1}dt$ into equation (23) to obtain

$$\int_0^\infty x^{p-1}\cos(x^n)dx = \cos\left(\frac{p\pi}{2n}\right)\int_0^\infty e^{-y^n}y^{p-1}dy,$$

$$\int_0^\infty (z^{\frac{1}{n}})^{p-1}\cos((z^{\frac{1}{n}})^n)(\frac{1}{n}z^{\frac{1}{n}-1}dz) = \cos\left(\frac{p\pi}{2n}\right)\int_0^\infty e^{-(t^{\frac{1}{p}})^n}(t^{\frac{1}{p}})^{p-1}\frac{1}{p}t^{\frac{1}{p}-1}dt,$$

$$\frac{1}{n}\int_0^\infty z^{\frac{p}{n}-1}\cos(z)dz = \frac{\cos\left(\frac{p\pi}{2n}\right)}{p}\int_0^\infty e^{-t^{\frac{n}{p}}}dt, \tag{25}$$

and by performing the same substitutions in equation (24), we get

$$\frac{1}{n}\int_0^\infty z^{\frac{p}{n}-1}\sin(z)dz = \frac{\sin\left(\frac{p\pi}{2n}\right)}{p}\int_0^\infty e^{-t^{\frac{n}{p}}}dt. \tag{26}$$

10 In more abstract terms, we used the fact that the sine and cosine functions are linearly independent in function space; for linearly independent vectors $\vec{v},\vec{w}$ it is true that $\alpha\vec{v}+\beta\vec{w}=\gamma\vec{v}+\delta\vec{w}$ implies $\alpha=\gamma$ and $\beta=\delta$.

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Finally, Poisson derived Laplace's results (8) and (9) by letting $\alpha = 1 - \frac{n}{p}$, which gives $p = n(1-\alpha)$, $n = \frac{p}{1-\alpha}$ and $\frac{1}{1-\alpha} = \frac{n}{p}$. Substituting this into (25), we get

$$\begin{aligned}\frac{1}{n}\int_0^\infty z^{\frac{p}{n}-1}\cos(z)dz &= \frac{\cos\left(\frac{p\pi}{2n}\right)}{p}\int_0^\infty e^{-t^{\frac{n}{p}}}dt,\\ \int_0^\infty z^{-(1-\frac{p}{n})}\cos(z)dz &= \frac{n}{p}\cos\left(\frac{n(1-\alpha)\pi}{2n}\right)\int_0^\infty e^{-t^{\frac{1}{1-\alpha}}}dt,\\ \int_0^\infty z^{-\alpha}\cos(z)dz &= \frac{1}{1-\alpha}\cos\left(\frac{\pi}{2}-\frac{\alpha\pi}{2}\right)\int_0^\infty e^{-t^{\frac{1}{1-\alpha}}}dt,\\ \int_0^\infty z^{-\alpha}\cos(z)dz &= \frac{k}{1-\alpha}\sin\left(\frac{\alpha\pi}{2}\right), \qquad (27)\end{aligned}$$

wherein we let $k := \int_0^\infty e^{-t^{\frac{1}{1-\alpha}}}dt$ as before. Similarly, by substituting $\alpha = 1 - \frac{p}{n}$ into equation (26), we get

$$\int_0^\infty z^{-\alpha}\sin(z)dz = \frac{k}{1-\alpha}\cos\left(\frac{\alpha\pi}{2}\right). \qquad (28)$$

Apart from different variable names, Poisson had seemingly derived equations (8) and (9) perfectly. However, there is one difference between the results derived by Laplace and those derived by Poisson. When transforming his results to match those of Laplace, Poisson let $\alpha = 1 - \frac{p}{n}$. This seems to work, since both n and p are arbitrarily chosen positive integers, according to our definition (10). This enables us to find the result for any rational $\alpha \in (0,1)$. This can easily be verified: given any rational number between 0 and 1, say $\frac{a}{b}$ with $b, a \in \mathbb{N}$ and $b > a$, we can let $p = b - a$ and $n = b$ to get $\alpha = 1 - \frac{p}{n} = \frac{b}{b} - \frac{b-a}{b} = \frac{a}{b}$. However, the expression $1 - \frac{n}{p}$ can only ever be a rational number for any choice of p and n, while Laplace's findings hold for any value $\alpha \in (0,1)$, including irrational numbers. It is likely that Poisson did not worry about the irrational cases, as his results seem easy to extend using a limit argument. In any case, in the rest of Laplace's article he only ever considered rational values of α, so for his purposes the rational results suffice. It should, however, be noted that we can attribute some epistemic benefit to Laplace's proof over Poisson's, in that Laplace's proof derives a more general partial result in at least this part of the proof—if we are willing to accept Laplace's methods, his proof brings stronger results. This apparent reduced generality of Poisson's proof could also be seen earlier, when Poisson reduced equations (4) and (5) which Laplace had found to the less general equations (8) and (9). It seems that Laplace arrived at these results more easily and naturally, while Poisson's direct methods required a longer and arguably more complicated proof to arrive at the same conclusions.

As mentioned in the introduction, the case has been made that pure proofs are generally more simple than impure proofs. We will soon discuss whether Poisson's proof is an example of a pure proof, but we can see already that 'direct' proofs in Poisson's sense are generally not simpler than indirect ones.

§4. The issue of directness. The two articles which we just discussed initiated an exchange between Laplace and Poisson, where both authors continued to dwell on the

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use of complex substitutions and the issue of 'directness'. We will now turn to this discussion beyond the particular computations we have seen thus far.[11]

Laplace's initial article from 1809 contains some brief comments on his choice of method. Laplace explained that 'when results are expressed in indeterminate quantities, the generality of the notation embraces all cases, whether real or imaginary' [22, p. 193]. By 'indeterminate quantities', Laplace meant variables: he thus seems to have used the 'generality of analysis' as a justification for his use of complex numbers [7, p. 96]. Citing an article from 1785 [21] wherein he had used another complex substitution to solve different integrals, he then announced that in this article he would give some new applications of this *artifice remarquable*, or 'remarkable artifice'. His phrasing here suggests that he was surprised by the usefulness of this 'passage from the real to the imaginary', but he also seems to have expressed quite a bit of confidence in the 'generality of analysis'. Though he may not have been sure about the exact scope of his methods and possible circumstances under which they may break down, he did believe in the correctness of his findings, while at the same time recognising that his approach was at least unusual.

A year later Laplace would use the same technique to deduce other results, commenting that they follow 'very simply' from the 'reciprocal passage of the imaginary to the real results'. He compared this passage to similar passages from the natural numbers to the rationals which enabled some geometers to deduce theorems 'by induction', that is to say, by generalisation [7, p. 96]. He notes that these methods 'confirm the generality of analysis', but only when used 'with reserve' [23, p. 304]. This particular comment makes Laplace's thoughts cloudy: he goes on about the 'generality of analysis', while also warning that the passage should be used 'with reserve'. Laplace thus seems to proclaim the general validity of such methods, while also admitting that cases do exist where we should refrain from using them.

Poisson was more explicitly opposed to the use of this 'artifice'. His response to Laplace's use of it in 1809 was to propose a 'direct' method of proving the identities that Laplace derived. For Poisson, 'Laplace's method was based on a "kind of induction," based on the "passage from the real to the imaginary quantities"' [7, p. 97]. Rather than accepting Laplace's proof, Poisson proposed to derive the same results 'directly' [30, p. 243]. Poisson did not communicate a clear judgment of the worth of Laplace's proof by calling it 'indirect', but by publishing his (laborious) direct proof he did let on that he deemed direct proof to be at least important enough to think and write about.

In turn, Laplace published another article in 1811 wherein he also gives a direct computation of an integral which he had computed by indirect methods earlier [25]. In another article from 1811 [24], Laplace again referred to some of his earlier results obtained using imaginary substitutions, stating that he had obtained them through

[11] We focused on these two articles in particular because they initiated the back-and-forth between Laplace and Poisson, but we will also look at later articles to highlight other relevant comments. I am indebted to a section from *Hidden Harmony—Geometric Fantasies* by Umberto Bottazzini and Jeremy Gray [7, pp. 95–98] for the general direction of Laplace and Poisson's discussion, as well as for their translations: though I have checked the original sources myself, I will use Bottazzini and Gray's translations extensively in this section.

a 'singular analogy based on the passage from the real to the imaginary' [7, p. 97], whereafter he again emphasised that these methods should be used with 'great care and discretion'. His confidence in the 'generality of analysis' seems to have weakened a bit here: he even stated that this passage should be used as a 'means of discovery' and that we still require a 'direct proof' of the results so obtained [7, p. 97]. Despite this, Laplace went on to provide even more examples of these passages from the real to the imaginary in the same article.

Summarising the latter 1811 article by Laplace, Poisson remarked that Laplace gave more examples of the passage from the real to the imaginary, calling it an 'inductive method' while again insisting on the need for more direct methods, such as the one he would provide in a follow-up article in the same year [7, p. 97].

This process of using the passage from the real to the imaginary 'with discretion' continued for several years, where Laplace continued applying it to discover new identities in order to then verify them directly, or to see Poisson verify them. Eventually Poisson also admitted that these complex substitutions might be employed as a 'means of discovery' in 1813, when he himself found some integrals using the method only to insist on always verifying them 'directly' as well [7, p. 98].

As such, we see that both authors had their doubts about the use of complex substitutions. Laplace seemed to appeal to the 'generality of analysis' to justify his use of complex numbers, while also noting that the method should be used sparingly. Poisson was a bit more conservative, responding to Laplace's work with repeated warnings against the use of these indirect methods and always providing direct proofs instead.

Using our modern perspective, we saw that the careless application of these substitutions can easily lead to mistakes like the one we saw in Laplace's work. It should be noted, however, that Poisson's objection to Laplace's proof does not necessarily arise from a concern for correctness. He does not identify instances where Laplace applies complex numbers incorrectly, but instead objects to the use of complex numbers in the first place which he calls indirect in this context, not incorrect.

The discussion between the two raises some interesting questions. Why did Poisson insist on the use of 'direct' methods, and what exactly qualifies a proof as 'direct'? Unfortunately Poisson did not pause to offer a reflective commentary on what makes a proper proof: brief comments like the ones we discussed above are all we get. Luckily, these questions about what methods *belong* in proofs and questions about the validity of using methods which *don't* belong come up more frequently, so we can consult other sources on the matter. Today, we would identify this type of methodological concern about using methods that are in some sense intrinsic to a problem as a concern for mathematical purity. In the next section we will compare Laplace and Poisson's directness discussion to more recent discussions of mathematical purity.

§5. Mathematical purity. As we have seen in the introduction, a concern for purity of proof methods was shared by thinkers like Aristotle, Bolzano and Hilbert. We found that Aristotle's definition of purity relies on a division of the sciences, while Bolzano's definition relies on the categorisation of some branches of math as 'more specialised' than others, or 'derived from analysis'. Neither of these definitions are completely satisfactory for our case study, though they do give us some examples of how notions of purity were developed throughout history, as well as examples of the difficulties that arise when trying to define such a nebulous concept.[12]

Below we will discuss first Detlefsen and Arana's topical definition, which Arana has stated to be an interpretation of Hilbert's statement on purity [3, p. 29]. Second, we will discuss Kahle and Pulcini's operational definition, which was proposed as an alternative to the topical definition. We will then compare them to our case study. Not only will this contextualise our understanding of what Laplace and Poisson meant by 'directness', we will also see that the comparison reveals both the differences between the two contemporary definitions as well as their intrinsic limits.

§6. Topical purity: Detlefsen and Arana. The 2011 article *Purity of Methods* by Detlefsen and Arana formalises what they believe to be one of the central conceptions of mathematical purity, which they call 'topical purity' [13, p. 2]. Along with their definition, the authors build an argument for the epistemic virtue of topical proofs over non-topical proofs. It is worth noting that Detlefsen and Arana do not warn against the use of impure proof methods, they merely argue that pure proof methods have benefits of their own. This is different from the views of Aristotle and Bolzano, for example, who argue that purity of method is absolutely necessary for the validity of (mathematical) reasoning. We have seen that Laplace and Poisson existed somewhere in between the two extremes, by repeatedly emphasising the necessity of direct (or pure) proofs, while also admitting that there is some use in impure proofs, at least as a tool of discovery. Before we can follow Detlefsen and Arana's reasoning on the epistemic benefit of topical purity, we must understand their definition, which is rooted in their view of how our understanding of a problem is determined.

Detlefsen and Arana state that 'generally speaking, a purity constraint restricts the resources that may be used to solve a problem to those which *determine* it' [13, p. 13]. They introduce the notion of 'topically determining commitments', that is, the commitments that together determine what the content of a problem is to the investigator. They elaborate by saying that 'in mathematics, among those things which determine contents are definitions, axioms concerning primitive terms, inferences, etc.' [13, p. 13]. They define the set of topically determining commitments of a problem to be the *topic* of said problem. As an example they refer to the 'infinitude of primes'-problem, which asks us to prove that there are infinitely many primes. They give a complete list of what they believe to be the topic of the problem, which we won't quote in full here. It includes at least a definition of natural numbers and a definition of primality: in order to ask whether or not there are infinitely many primes, we need to understand what numbers are, as well as what it means for a number to be prime.

Using this notion of determining commitments, the authors give their definition of topical purity. In their own words: 'We say that a solution $\mathcal{E}$ of $\mathcal{P}$ is *topically pure* when it draws only on such commitments as topically determine $\mathcal{P}$'.

This definition has a useful property: something interesting happens when for whatever reason, we wish to retract one of our commitments. Because the topically pure solution $\mathcal{E}$ only draws on commitments that topically determine $\mathcal{P}$, any retraction of a

12 Of course, Aristotle, Bolzano and Hilbert weren't the only people to discuss purity: other notable figures who have touched on the subject include Archimedes, Leibniz, Gauss, Dedekind, Frege, and many others. Michael Detlefsen provides a more complete account of the 'history of purity' in [12].

commitment that determines the contents of the solution $\mathcal{E}$ also leads to a retraction of one of the commitments that topically determine $\mathcal{P}$. As such, it is impossible for a pure solution $\mathcal{E}$ to stop being a solution (through retraction of one of the necessary commitments of $\mathcal{E}$) to the problem $\mathcal{P}$ without dissolving said problem: If we drop one of the topically determining commitments, the original problem ceases to exist as we have previously understood it.

For a solution that is impure, we cannot say the same. It is possible that one of the commitments that is necessary for the solution but which is not part of the topic of the problem is retracted: in this case, the solution ceases to solve our problem, while the problem remains in place.

In order to demonstrate their definition and the useful quality of a topically pure proof, the authors return to the infinitude of primes problem, for which they compare two alternative proofs by Euclid and Fürstenberg. Euclid's proof merely uses the definitions of natural numbers and primality, while Fürstenberg defines a topology on the integers to then use topological properties to arrive at the result. Euclid's proof is topically pure, seeing as how it only draws on commitments that topically determine the problem. Fürstenberg's proof, however, is not topically pure: we could retract our commitment to topological definitions. If we stop believing in the concept of a topology, Fürstenberg's proof is no longer a solution to our problem, but our problem persists. If we drop our definition of natural numbers, prime numbers, division, or any of the definitions used in Euclid's proof, the proof stops working, but our problem dissolves as well.

Now, this definition of topical purity seems to result in a pretty rigorous way of identifying pure proofs. Rather than relying on a notion of genus like Aristotle, or requiring a strict hierarchy of mathematics where some branches are derived from others like Bolzano, Detlefsen and Arana give more or less precise instructions for how to determine the purity of a proof. The only difficulty lies in determining the topic of a problem, that is, the set of determining commitments.

Before we attempt to apply the topical definition to the case of Laplace and Poisson, it should first be noted that Laplace and Poisson's case has a slightly different setting. Laplace starts out with an open question to which he seeks the answer through computation, rather than a theorem which he seeks to prove. Of course it is easy to reduce the question–answer case to a theorem–proof situation, by interpreting Laplace's work as a proof that the answer he found is indeed the answer to the question posed: this reduction is always possible in this direction, though not vice versa. We can view Poisson's solution as precisely such a reduction, as Poisson seeks to confirm that Laplace's findings are correct through direct methods: the 'theorem' he seeks to prove is then that Laplace's findings were correct.

We are still left with the difficulty of listing the topically determining commitments of a problem. Notwithstanding this difficulty, we can apply the definition of topical purity to the case of Laplace and Poisson. In their example, Detlefsen and Arana composed an exhaustive list of all the commitments necessary to understand the infinitude of primes problem, including axioms for a successor function, ordering axioms, multiplication, a definition of primality, et cetera: attempting to do the same for Laplace's case is both ambitious and unnecessary for our purposes. Without listing all commitments necessary for understanding the problem, we can still note which commitments are not necessary in that list. We have seen that Laplace's proof uses a complex substitution, which relies on a commitment to a definition of complex numbers. This commitment is clearly not necessary to the statement of the original problem, as we can define integration of real functions without ever mentioning complex numbers.

As such, Laplace's proof is not topically pure: if we were to retract our commitment to a definition of complex numbers, Laplace's proof falls apart, while our problem persists. In fact, we have already seen this happen when we ran into the lack of domain transformation after the substitution. Laplace's proof relied on the commitment that complex substitution can be treated similarly to real substitution, which leaves the domain of integration unchanged. Though he may have believed this was correct, we disagree: we had to check the argument using even more advanced mathematics. After reading Cauchy's work on complex integration, it is likely that Laplace would have stopped believing in his solution because of the unfounded transformation step. Thus retracting his commitment to this view of complex substitution, the proof falls apart while the problem persists, which is precisely what Detlefsen and Arana argued to be the main pitfall of impure proofs.

Now, Poisson's proof looks a lot more pure. It only ever uses definitions of integration and standard, real substitutions in order to arrive at the result. We might be tempted to conclude that Poisson has given a topically pure proof, and that this topical definition of purity really is a good formulation of the ideals according to which Poisson did his work. Unfortunately, things are not that simple. The critical reader may note that Poisson uses double integration in his proof, a definition of which is not at all required to understand the original problem.

When we opt to define integration merely for functions of one variable, that is, we let $\int_a^b$ map functions $f : \mathbb{R} \to \mathbb{R}$ to real values $\int_a^b f(x)dx \in \mathbb{R}$, then double integrals lose their meaning,[13] as in those integrals the integrand would be a function $f : \mathbb{R}^2 \to \mathbb{R}$. Writing the double integral as 'single integration twice' would then not work, as the inner integral would be undefined. We can define integration of functions of one variable perfectly well without generalising to more variables, so the commitment to a definition of multiple integrals is strictly speaking not a determining commitment for the problem. Therefore if we retract it Poisson's proof does fall apart while our original problem persists: this restricted definition of integration is enough to understand the problem Poisson tries to solve.

Thus, perhaps surprisingly, Poisson's proof is not topically pure either. While Poisson considered his method to be 'direct' and obviously preferable to Laplace's, he was apparently not as strict as Detlefsen and Arana. A 'direct' proof as Poisson understood it is therefore not the same thing as a topically pure proof. The 'strictness' of Detlefsen and Arana's definition can be seen as a limitation, as we might argue that *intuitively* it seems like Poisson's proof is perfectly valid and very much by-the-book in the sense that it doesn't seem to appeal to any grossly extraneous concepts.

This 'strict' or conservative quality of the definition has been scrutinised in other cases as well, notably when applied to Euclid's proof of the infinitude of primes, Detlefsen and Arana's original example of a topically pure proof. In an article from 2014 [1], Arana responded to the possible objection that according to their definition, Euclid's proof is actually not pure. The basis for this claim is that Euclid's proof requires the multiplication of numbers in one of its steps, while a notion of multiplication is not

13 Though this definition seems arbitrarily strict, it is not: repeated integration with respect to one variable at a time does introduce some additional complexity, for instance, when we want to switch the order of integration.

strictly necessary to formulate the original problem. If for example we define primality using the sieve of Eratosthenes, we don't need a notion of multiplication to be asking whether or not there are infinitely many primes. We don't actually need to define multiplication to understand the problem, even though it is the inverse operation of division which seems quite central to a notion of primality.

In response to this objection, Arana gives a more fine-grained analysis of the topic of the infinitude of primes problem. To deal with the objection he shows that we can rewrite multiplication in terms of the successor function, which is more obviously topical for the understanding of natural numbers. Unfortunately, this rewriting results in a proof which is considerably longer than Euclid's original formulation, and harder to grasp quickly. Recall that Arana has elsewhere contested the claim that impure proofs in general are simpler than pure proofs [2].

Similarly to Poisson's proof, most people would agree that Euclid's proof is elementary or pure based on intuition. In response to the objections against Euclid's use of addition and multiplication, Arana rewrote the original proof into a pure version, thus confirming that Euclid's original formulation is *not* pure according to his definition of topical purity. We will return to the issue of rewriting proofs to arrive at a pure version later on.

Intuitively, most would agree that the use of multiplication in a proof of the infinitude of primes should not render a proof impure. The definition of topical purity seems therefore to be too far removed from our intuitive understanding. To remedy this, we might simply admit that multiplication is part of the topic of the infinitude of primes problem, even though it is not strictly necessary for our understanding of the problem. This context-dependent, liberal use of the topical definition can be extended to our case study as well: If we admit that multiple integration is just a more general definition of single-variable integration and that therefore it should be part of the topic of single-variable problems, Poisson's proof does not draw on non-topical commitments and is thus topically pure in that context. In a more recent article, Arana commits to this context-dependent, or agent-relative quality of the definition, saying that 'the topic of a theorem is agent-relative: it is the family of commitments that determine a particular agent's understanding of that theorem' [3, p. 37].

However, when we stray from the black-and-white definition, we make room for more debatable conclusions. In the same vein, we could pose that complex numbers are a mere generalisation of the real numbers, and that we ought to consider them topical to problems involving real numbers. This would make Laplace's proof topically pure.[14] We can see that this agent-relative view permits a very liberal use of the topical definition, allowing the definition to stretch and fit widely varying viewpoints depending on context—context which can be readily chosen at an agent's leisure.

As such, even though it might be tempting to opt for this context-dependent notion of topical purity where we admit ourselves the luxury of manually determining the topic of a problem while disregarding whether or not we actually need a commitment, this gives

[14] Detlefsen and Arana propose Fürstenberg's proof as their example of a topically impure proof, but depending on context we might consider topological definitions to belong to the topic of the infinitude of primes problem: in fact, Detlefsen and Arana mention that this view was expressed by Colin McLarty in correspondence [13, p. 15]. McLarty thus aligns himself with the *Bourbakiste* tradition of arithmetic research, a tradition to which Fürstenberg's work also belongs, which affords a central role to topology in arithmetic [13, pp. 15–16].

rise to new issues. By manually deciding amongst ourselves on which commitments to include in a problem's topic, we reduce our notion of purity to a question of consensus or agreement—this use of topical purity is in practice pretty close to simply using our intuition, agreeing on the purity of a proof based on consensus. In the same article mentioned before Arana grants that the difficulty of topic determination remains, leaning into the agent-relative definition of topical purity: 'We thus treat topic determination for the time being in the naive way [...], doing so is consistent with the way mathematicians have treated purity in practice' [3, p. 29]. As noted, this loose view of topical purity may be better adapted to account for the widely varying intuitions about purity held by mathematicians in practice, but unfortunately it limits its usefulness on its own in analysing individual cases like Laplace–Poisson.

The notion of topical purity does not completely pinpoint the issues that Poisson and Laplace were discussing, at least not when we apply the definition as strictly as Arana and Detlefsen originally define it. Neither of their proofs can definitively be considered topically pure, so Poisson's preference for 'direct' method does not correspond directly to topical purity in Detlefsen and Arana's sense. Adopting the less strict agent-relative definition of topical purity, we could say that Poisson did not deem complex substitution to belong to the topic of the problem, and thus disapproved of Laplace's proof because of its topical impurity. This, however, seems like a mere reformulation, not giving much deeper insight into the reasons behind Poisson's objection.

The definition given by Detlefsen and Arana is probably the most fleshed out definition of purity existing in the literature today, but there are cases where it is not completely satisfactory, because the original formulation was too conservative or strict, while the agent-relative interpretation is not strict enough. Though their definition seems very rigorous, we still cannot completely accept it as the definitive notion of mathematical purity, which illustrates just how nebulous purity is as a concept and how hard it is to define.

We have objected to Detlefsen and Arana's definition for our own reasons, but objections based on different grounds have been made as well. In the next section we turn to another article which objects to Detlefsen and Arana and proposes an alternative definition of purity instead. We will consider whether this next definition better matches what Poisson and Laplace were thinking about when they expressed their preference for 'direct' proof.

§7. Operational purity: Kahle and Pulcini. This response to Detlefsen and Arana was given by Reinhard Kahle and Gabriele Pulcini in their 2018 article *Towards an Operational View of Purity* [19]. In the article, the authors highlight a problem with Detlefsen and Arana's definition of purity and propose their own alternative: a definition of 'operational purity'.

The authors' issue with topical purity arises from cases where according to them, topically impure proofs should instead be considered pure. They object to the infinitude of primes example from Detlefsen and Arana's paper where Fürstenberg's topological proof is labelled as impure by citing a brief paper by Idris Mercer [28]. Mercer rewrites Fürstenberg's proof whilst avoiding topological language, in order to show the 'real reason that Fürstenberg's approach works' [28, p. 355]. Recall that Arana also rewrote Euclid's proof in order to arrive at a topically pure version.

To avoid topological language, Mercer divides the integers up into the same sets that Fürstenberg used as a topology on the integers. Fürstenberg then used some general properties of topologies to conclude that there must be infinitely many primes, while Mercer proves that these same 'topological' properties hold for the sets that he defined. By verifying these properties for these particular sets rather than drawing on topological definitions, Mercer produces a topically pure proof of the infinitude of primes, which is 'essentially the same' as Fürstenberg's proof.

According to Kahle and Pulcini, this exposes a flaw in Detlefsen and Arana's definition of purity, as the 'same' proof may be considered either pure or impure depending on how it is formulated. Regarding how to classify a proof π of a theorem T as pure, they state that 'testing purity is thus no longer a matter of confronting the "content" of π with the "content" of T. Actually, one should be able to rule out the possibility of rewriting the proof π into a more elementary version π' whose "content" does not outstrip the "content" of T any more' [19, p. 7]. According to Kahle and Pulcini, Fürstenberg's proof is equivalent to Mercer's 'modulo useless roundabouts' [19, p. 8]; the use of topology is not essential to Fürstenberg's argument, so by eliminating this 'useless roundabout' we obtain Mercer's pure proof, which the authors consider 'equal' to Fürstenberg's.

In order to provide a definition of purity that avoids this problem, Kahle and Pulcini define what they call the *operational content* and *ontology* of a theorem T or proof π. They define the operational content of a theorem T (respectively π) as the set of mathematical operations mentioned in T (respectively π). Given an operational content, its ontology is defined as the smallest numerical domain (like $\mathbb{N}, \mathbb{Z}, \mathbb{Q}$, et cetera) which is closed under all operations in the operational content.

Using these definitions, Kahle and Pulcini then define operational purity. Given a theorem T and a proof ψ of T, we call ψ operationally pure if the ontology of (the operational content of) ψ is a subset of the ontology of (the operational content of[15]) T. This notion of purity can be understood as follows: a proof of a theorem is pure if the operations used in it are at most as 'high-level' as the operations mentioned in the theorem, where by a 'higher-level' operation we mean an operation that is only closed under 'bigger' number systems. For example, division is a 'high-level' operation compared to addition, since $\mathbb{N}$ is closed under addition while the smallest number system closed under division is $\mathbb{Q}$. Similarly, their definition allows us to compare the 'pureness' of two proofs ψ, ϕ, rather than looking at their individual purity; we can say that ψ is *more pure* than ϕ if the ontology of ψ is a subset of the ontology of ϕ.

Kahle and Pulcini then apply their definition to Fürstenberg's proof of the infinitude of primes. To do this, they first consider the operational content of the infinitude of primes problem, wherein they let IP denote the theorem that there are infinitely many primes: 'The definition of prime number relies on the division operation, so the operational content of IP is $\{/\}$'.[16] Therefore, according to [the definition of ontology],

15 For the sake of readability, we will abbreviate 'the ontology of the operational content of the proof/theorem' by 'the ontology of the proof/theorem' from now on.

16 Recall that earlier we have seen that this is not necessarily so. We could formulate our definition of primality using the sieve of Eratosthenes, in which case division would not be part of the operational content of IP. In fact, there would be no operations left, according to Kahle and Pulcini's limited interpretation of operations.

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we get that [the ontology of IP] is $\mathbb{Q}$ [19, p. 9]. Now, if we carefully scan Fürstenberg's proof, which we denote by ψ_F, we find that its operational content is $\{+, \cdot, -, /, \cap, \cup\}$. Kahle and Pulcini then note that 'since the inclusion of union and intersection in any operational content do [sic.] not affect the underlying ontology,[17] we get that the ontology of ψ_F is $\mathbb{Q}$, and so the ontology of ψ_F is a subset of the ontology of IP' [19, p. 9]. As such, Fürstenberg's proof is operationally pure.

Now, before we move on to applying operational purity to our case study, we will discuss some objections to Kahle and Pulcini's definition. First of all, adopting operational purity rather than topical purity as your desired purity-definition comes at the cost of sacrificing some generality: operational purity as Kahle and Pulcini define it has no meaning when considering strictly geometrical theorems and their proofs, as operations on number systems are not strictly required to formulate a geometric theorem. Operational purity therefore has no meaning when we are interested in theorems about mathematical objects which are not numerical. One could consider a geometric equivalent, with for example linear, plane and solid 'ontologies', but this is not proposed by the authors.

Additionally, the definition of operational purity relies on a limited (and arguably arbitrary) interpretation of what an 'operation' is. From the article it seems like the only operations which qualify as part of operational content are binary, arithmetical operations like addition and subtraction, but no mention is made of, for example, unary operations, or operations with input from one set and output in another. The definitions of operational content, ontology and consequently operational purity do not obviously generalise well to the more general notion of what an operation can be. It might, but the authors do not explain how, so we are left with a very situational definition of purity.

Finally, and perhaps most crucially, we could argue that the motivation for Kahle and Pulcini's definition is artificial. Their problem with the definition of topical purity which prompted them to develop their own definition was that Fürstenberg's proof was considered impure, even though it could be rewritten to be pure, as Mercer showed. However, this is only a problem if we insist on the idea that Fürstenberg's proof is equivalent to Mercer's: We could just as easily accept that Fürstenberg's proof is impure while Mercer's is not, since they are different proofs.

Mercer's avoidance of topological language in his proof constitutes a change in the formulative resources he employs which makes his proof different from Fürstenberg's in at least a formulative sense. Detlefsen and Arana spend some time setting up a framework for what exactly constitutes a problem, wherein they separate the formulative resources used in representing a problem to us from the content of the problem, such that the formulative resources at least partially determine the identity of a problem [13, p. 9]. In the same vein it seems reasonable to assume that the formulative resources employed in a proof are also part of its identity. As such, Kahle

17 This casual statement is not explained further by the authors. It is not entirely clear why any number system is closed under union and intersection, because it is unclear what the union or intersection of two numbers is unless we choose to define numbers as a set-theoretic construction. Of course, the power sets of all number systems are closed under union and intersection: for example, the union or intersection of two sets of natural numbers is obviously a set of natural numbers as well (or the empty set).

and Pulcini's criticism that the definition of topical purity distinguishes between the purity of 'different versions of the same proof' is rendered artificial when we consider that the very framework behind topical purity deems a different formulation of a proof to be a different proof altogether. If Mercer's and Fürstenberg's proofs are not the same proof, then the fact that Mercer's is considered pure while Fürstenberg's is not is no longer contradictory.

All that being said, the notion of operational purity is still useful because of how easily applicable it is. Verifying topical purity requires a lengthy analysis of what commitments determine the topic of a theorem or problem, a task for which we do not have a general procedure. Scanning for operations is easy, and comparing ontologies is just as straight-forward, which makes determining the operational purity of a proof almost trivial. Additionally, operational purity has the added functionality of quantifying purity by a sort of 'metric', where we can say that one proof is more or less pure than another, divided by discrete steps of subsequent number systems: this is a bit more subtle than simply qualifying proofs binarily as either pure or impure. Recall that we objected to the strictness of the topical definition of purity earlier—in some cases, the nuance of the 'metric' of operational purity might be preferable to the black and white view of topical purity.

Kahle and Pulcini's definition has another interesting property. When determining the operational purity of a proof, we compare its ontology with the problem's, but the possibility of comparing purity among different proofs merely has us compare the ontologies of the proofs. Interestingly, this does not require us to refer to the theorem's ontology at all: the comparative purity of two proofs can apparently be determined independently of the theorem which they prove.

Taking this one step further, we can take two proofs of *different theorems* and argue that one is more operationally pure than another, purely based on the operations used in both proofs. This is probably merely an anomaly as it is likely that the authors only had purity comparison in mind for two proofs of the same theorem, but what's interesting about this is that it illustrates the problem-independent character of the definition. It seems like the problem-independent hierarchy of ontologies is central to the definition and significantly distinguishes it from, e.g., topical purity, which is decidedly more dependent on context.

Next, let's apply Kahle and Pulcini's definition to our case study. Their definition is particularly suitable to the issue of complex substitutions as the authors have also included a section on complex numbers. They consider addition and multiplication of complex numbers to be a higher-level operation than addition and multiplication of real numbers. To illustrate the difference, they consider the isomorphism of $(\mathbb{C}, +, \cdot)$ and $(\mathbb{R}^2, \oplus, \odot)$, where $\oplus$ denotes component-wise addition and $\odot$ is given by $(a, b) \odot (c, d) = (ac - bd, ad + bc)$. Though component-wise addition can be seen as a logical extension of addition on $\mathbb{R}$ to $\mathbb{R}^2$, the authors state that '$\odot$ is not supposed [to] come automatically with the operations defined on $\mathbb{R}$' [19, p. 10]. As such, complex multiplication is external to real analysis. Formally, for a theorem T of real analysis, Kahle and Pulcini would say that $\odot$ is not part of the operational content of T and 'based on the above isomorphism, [the ontology of] $\{\oplus, \odot\}$ is not just $\mathbb{R}^2$ but, indeed, $\mathbb{C}$' [19, p. 10].

Now, checking Laplace and Poisson's proofs for operational purity is simple. A careful scan of the proof teaches us that the only operations used in Laplace's proof are $\{+, -, /, \cdot, \oplus, \odot\}$. Meanwhile the operations used in Poisson's proof are $\{+, -, /, \cdot\}$.

As such, we see that the ontology of Poisson's proof is $\mathbb{Q}$,[18] while the ontology of Laplace's proof is $\mathbb{C}$, so the ontology of Poisson's proof is a subset of the ontology of Laplace's: we see that Poisson's proof is *more pure* operationally than Laplace's. The operations necessary to formulate the problem (in the way we defined it earlier when discussing topical purity) are $\{+, -, /, \cdot\}$, so by again comparing ontologies we see that Poisson's proof is indeed operationally pure, while Laplace's is not.

This definition of operational purity seems more in line with Poisson and Laplace's notion of 'direct' proof, at least because it calls Poisson's direct proof pure and Laplace's indirect one impure: this is probably so by virtue of the specific case study. In the case study, Poisson objects specifically to the use of a complex substitution, that is, he objects to Laplace's use of this foreign number system which comes with its own operations. A definition of purity which ranks purity of proofs based on the number systems and operations that show up in said proofs fits the discussion between Poisson and Laplace particularly well.

Because this definition is so perfectly tailored to our case study, it's hard to conclude whether 'operational purity' is what Poisson meant by 'direct' method in general—it is unlikely that it's an exact match. However, one could argue that operational purity as a notion is more akin to mathematical thought at the time than topical purity is. The logical treatment of mathematics based on axiomatisation wherein we can list and compare axioms and definitions used in theorems and their proofs, which is necessary for application of the topical definition, was not common in Laplace and Poisson's time, and would not gain traction until the twentieth century. Of course, it is unlikely that Poisson or Laplace would think of purity as explicitly as Kahle and Pulcini, or using the same terms as them, but thinking of 'pure' or 'direct' proofs as proofs which do not appeal to operations of a 'higher ontology' seems more in line with what Poisson, Laplace and others at that time would think.

§8. Conclusion. The discussion between Laplace and Poisson offers some insight into their own conceptions of mathematical purity, or directness, as well as into the difficulty of defining what it means for a proof to be pure. Laplace and Poisson remained wary of indirect proof, consistently seeking out direct alternatives, while still using indirect proof as a 'means of discovery'. All the while, neither one of the two offered a clear formulation of what the actual problem is with indirect proof.

Our contemporary definitions could not satisfactorily explain directness, or what we would now call purity, either. We have compared Detlefsen and Arana's topical as well as Kahle and Pulcini's operational definition of purity to Laplace and Poisson's notion of directness: topical purity deems both Laplace's and Poisson's proofs impure, while operational purity agrees with Poisson.

Unfortunately, even though the operational definition in the end agrees that Poisson's proof is pure, the definition has significant problems. In fact, both the operational and

[18] This is perhaps surprising, because the notion of integration presupposes that the integrand is a function of $\mathbb{R}$, so perhaps the ontology of Poisson's proof should be $\mathbb{R}$. But in this case we should consider multiple integration again, which supposes integration over $\mathbb{R}^2$, so the ontology is $\mathbb{R}^2$. This would then render Poisson's proof operationally impure when compared to the problem's ontology $\mathbb{R}$. Kahle and Pulcini do not mention $\int$ or $\iint$ as operations, so we will stick to the direct application of their definition which yields $\mathbb{Q}$ as the ontology of Poisson's proof.

the topical definitions reveal their problems when confronted with the Laplace–Poisson case, with the topical definition being either too strict or not strict enough depending on our interpretation, while the operational definition is too situational, arbitrarily defined and arguably artificially motivated.

As such, we are left in the same situation as Laplace, Poisson and all mathematicians that came before and have come since. Although we all have an intuitive understanding of what constitutes a pure, direct, or elementary proof, we cannot properly define what that means and are left to disagree on our personal preferences.

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