Richard Price, the First Bayesian[1]

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Abstract: Roughly half of Bayes's famous essay was written by Richard Price, including the Appendix with all of the numerical examples. A study of this Appendix reveals Price (1) unusually for the time, felt it necessary to allow in his analysis for a hypothesis having been suggested by the same data used in its analysis, (2) was motivated (covertly in 1763, overtly in 1767) to undertake the study to refute David Hume on miracles, and (3) displayed a remarkable sense of collegiality in scientific controversy that should stand as a model for the present day. Price's analysis of the posterior in one particular example, including locating the posterior median and giving and interpreting credible regions, qualifies him as the first person to apply Bayes's theory.

Richard Price (1723-1791) was a noted British moral philosopher, an expert in actuarial science and population statistics, and a friend of America and Benjamin Franklin. In his own time, he was best known for a few books (two on moral and religious philosophy in 1758 and 1767, and one on actuarial science in 1771); he also published about two dozen tracts on political and ethical issues of the day. He was elected to the Royal Society in 1765; Benjamin

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Franklin was one of his sponsors. In statistics, he is principally (perhaps only) known for having presented Thomas Bayes's Essay on chances to the Royal Society in late 1763, more than two years after Bayes died in April 1761. But to view Price as simply a passive conduit – a loyal secretary – is a considerable underestimation. Price wrote about half of the published paper and was the first person who publicly applied the theory; some of the material he added has a remarkable relevance to some of the most vexing issues in statistical applications today.

Thomas Bayes was born in 1701 with probability 0.8 (Stigler, 2004), and Richard Price was born in 1723, about 22 years Bayes's junior. Notwithstanding the difference in ages, they had a great deal in common. Both came from families that dissented from the established Church of England; both were at times ministers; both had strong intellectual interests, including in mathematics. D. O. Thomas (1977, page 128) suggests that they may have become acquainted through John Eames, who had tutored Price in mathematics about 1740 and 20 years earlier taught Bayes. Eames also co-sponsored Bayes for election to the Royal Society in 1742, about the same time he was teaching Price, and he may well have introduced the two around that time. If so, they would have met in the early 1740s (Eames died in 1744).

There were two noted signs of the strength of the relationship. First, Bayes left Price a substantial £100 bequest, and second, Price dedicated considerable energy and time to preserving, developing and presenting to the world Bayes's manuscript on chance and inductive logic. I believe it is a reasonable speculation that Price and Bayes discussed this work in the 1750s, and that when Bayes died, Price, knowing the force and potential application of the results, sought and obtained permission to retrieve the papers with the aim of preparing them for publication: we know from the will that Bayes and Price were close, and there is evidence that Bayes had told David Hartley about the work in 1749; surely he would have told his close friend, co-religionist and fellow student of mathematics as well (see remarks at the conclusion of this article and Stigler, 2013, page 285).

As for determining the respective contributions to the work, we have both Price's clear statements within the publication and the record in the minutes of the Royal Society at the time of its submission. There it is clearly stated that the submission was not a single document, but three quite separate parts:

> "Mr. Canton [The Secretary of the RS] presented to this Society a manuscript Dissertation, by the late Revd., Mr. Tho. Bayes F.R.S. consisting of 34 pages in quarto and entitled an Essay towards Solving the following Problem in the Doctrine of Chances: viz. Supposing nothing is known concerning an event, but the number of times it has happened, and failed, in a great number of trials; required, the chances that the probability of its happening in a single trial, shall be somewhere between two degrees of probability which may be named. This Dissertation was accompanied with a Letter to Mr. Canton signed Richard Price; and also an Appendix to the Essay containing an application of the Rules in it to some particular cases, consisting of 20 quarto pages; for which presents thanks were returned to Mr. Canton, and ordered to Mr. Price." (*Royal Society Journal Boo*k Vol. 25, 22 December 1763, courtesy of David Bellhouse)

In its published form these were presented as one document: Price's letter served as an introduction, followed by the Bayes's Dissertation, and finally Price's Appendix. Price's own comments make it clear that he based the introduction in part upon a brief introduction found with Bayes's papers that has not survived, and that the theoretical portion (referred to as the

"Dissertation" in the RS Journal Book) was entirely Bayes's work (Price added a few footnotes). The Appendix was claimed by Price as his own. Price also ordered the publication of 50 offprints under the much more provocative title, *A Method of Calculating the Exact Probability of All Conclusions founded on Induction* (Stigler, 2013).

Thus the theory can be taken as entirely due to Bayes. This included the argument giving the posterior distribution for a binomial experiment, supposing a uniform prior distribution for the unknown probability, and the argument for that choice of prior as representing having no *a priori* information about the unknown probability (Stigler, 1982). Price also attributed to Bayes a crude approximation to the posterior distribution when the numbers of successes and failures were moderately large; Price gave a condensed account of that work, and a year later Price published as his own work an improved version (see Hald, 1998 pp. 147ff, for details on the approximation).

Bayes's theoretical result stated (in modern notation) that in $n = p + q$ independent trials, if an event M was observed to happen p times and fail q times, and *a priori* the chance the probability x of M happening in a single trial was uniformly distributed between 0 and 1, then the *a posteriori* chance that x was between a and b ($0 \le a < b \le 1$) equaled the integral of $x^p(1-x)^q$ over (a, b) divided by the integral of the same function over (0, 1); that is, a Beta (p+1, q+1) distribution. These integrals are of course quite simple if either p or q is 0, or 1, or small enough to break the integral into a sum of simple integrals. But if both p and q are moderately large that is not feasible, and Bayes (and Price in his subsequent article) expended considerable mathematical energy developing approximations through the use of different quadrature formulae. Bayes's mathematical ability was certainly on display: In his preface to a reprinting of

Bayes's Essay, E. C. Molina (1940) credits W. E. Deming with recognizing in Bayes's development the equivalent of this identity involving the cumulative distributions of the Beta and Binomial:

$$\int_0^x x^p(1-x)^q dx \Big/ \int_0^1 x^p(1-x)^q dx = \sum_{s=p+1}^{p+q+1} \binom{p+q+1}{s} x^s (1-x)^{p+q+1-s}.$$

But nowhere in the Dissertation did Bayes give even a simple arithmetical example, much less a fully developed application.

Bayes's concentration on theory and apparent lack of interest in arithmetical examples is also evident in the other work of his that has survived. This can be seen in the parts of Bayes's notebooks that Andrew Dale has published, including a fragment on probability (Dale, 1986, 2003). David Bellhouse has inspected as many of Bayes's manuscripts as can be found and observes, "This [lack of application] is in accord with what I have seen of Bayes's other papers – all theory with not a hint of application (to anything)." (Bellhouse, 2002 and personal communication). Price's Appendix repaired that deficiency.

Price's Appendix.

Bayes had distilled his analysis into 3 Rules, and the Appendix presented illustrations of all of Bayes's Rules in 15 printed pages. The first Rule gave exact answers for $\Pr\{a<x<b|p,q\}$, but its use was only feasible for small q; the second and third gave approximations based upon a quadrature formula related to one Newton had used, divided into two cases. Price focused on simple cases for the most part, and the simplest had q = 0 or q = 1, where Bayes's Rule 1 gave

$\Pr\{x < a|\text{ any } p, q=0\} = a^{p+1}$ for $0<a<1$, and

$$\Pr\{x < a \mid \text{any } p, q=1\} = (p+2)a^{p+1} - (p+1)a^{p+2} \quad \text{for } 0<a<1.$$

From these formulae, results for p = 0 or p = 1 followed by interchanging the roles of p and q.

For his first example, Price supposed (following Bayes) that the event M had a probability x of occurring that was unknown prior to trials (i.e. a uniform prior distribution), and he asked what, after it had occurred once, could be said about the probability it would occur in a second trial? He calculated $\Pr\{x < ½ \mid p=1, q=0\} = (½)^2 = ¼$ and stated that, "The answer is that there would be an odds of three to one for somewhat more than an even chance that it would happen on a second trial." With p = 2, the odds would go to 7 to 1; with p=3, to 15 to 1; in general, to $2^{p+1} - 1$ to 1 "for *more* than an equal chance it will happen on further trials." Price also gave a set of examples involving lotteries, with q = 1, 2, 10, or 100. The larger values gave a chance to show off Bayes's Rule 3. But most of the space was given to what would become the most famous example, his discussion of the Rising of the Sun.

The Probability of the Rising of the Sun

The Rising of the Sun example asked the chances that the Sun will rise tomorrow evaluated in the light of accumulating information, and, since it had never failed, it was an example of the simplest type with q = 0. The philosophical question of how and why we have confidence that the Sun will rise tomorrow has a long history; David Hume discussed it in 1739 (Zabell, 2003, p. 47), and Andrew Dale (2003, p. 328) has found a discussion in 1695 by Edward Eizat, and there is no reason to think that is the first. Price seems to have been the first to address it in explicit quantitative terms.

Most people today encounter the problem in a version like the following, closely related to a later formulation by Laplace: The Sun is observed to rise today; supposing we were *a priori* uncertain about the probability the Sun will on a specified day rise in a sense represented by a uniform distribution over (0, 1), what is the chance it will rise tomorrow? If we observe n risings, what is the chance for the $n+1^{st}$ rising? Put this way, it is a straightforward application of Bayes: after the first rising we have p=1 and q=0; after the nth we have p=n and q=0, and Bayes's first rule would give the *a posteriori* distributions for the probability for all these cases. Laplace would take a step equivalent to obtaining the expectations of this distribution for the different cases (=1/2 if p=q=0, =1/3 if p= 1 and q=0, and =1/(n+2) if p=n and q=0). Then the expected chance that the Sun will rise the day after n risings have been observed will be 1 – 1/(n+2)=(n+1)/(n+2), a result that has come to be called *Laplace's Rule of Succession*. But this was not the way Price posed the problem, and the difference in approaches has a striking relevance to modern dilemmas and debates.

Price had in mind a framework he thought represented general questions of observation in nature, where an observation of a new-to-the-observer phenomenon is made (say, the Sun rising), but there can be no usable way of specifying the prior uncertainty of the event, since it was totally unexpected and the question of its occurrence could not have arisen before the observation was made. Price wrote that "Antecedently to all experience, it would be improbable as infinite to one, that any particular event, before-hand imagined, should follow the application of any one natural object to another; because there would be an equal chance for any one of an infinite of events." What was not *a priori* conceivable was not of quantifiable uncertainty, unless as infinitely unlikely, and certainly not as a uniformly distributed chance. Here is how he then framed the question:

> "Let us imagine to ourselves the case of a person just brought forth into this world and left to collect from his observation of the order and course of events what powers and causes take place in it. The Sun would, probably, be the first object that would engage his attention; but after losing it the first night he would be entirely ignorant whether he should ever see it again. He would therefore be in the condition of a person making a first experiment entirely unknown to him. But let him see a second appearance or one *return* of the Sun, and an expectation would be raised in him of a second return, and he might know that there was an odds of 3 to 1 for *some* probability [i.e. a chance better than ½] of this. This odds would increase, as before represented, with the number of returns to which he was witness."

Thus for Price the question of the Sun rising was a data-determined hypothesis, impossible to think about when even the existence of the Sun was unknown. Yet after that hypothesis was established, Price was comfortable with proceeding. It was a direct analog to the modern question of how you can properly analyze data to judge the support for a hypothesis that would not have occurred to you before the data were at hand. Price's situation was simple and he would take the first observation as establishing the hypothesis and so he took as the data all risings of the Sun after the first; that is, all *returns* of the Sun. Ten risings of the Sun would give p=9 and q=0, not p= 10 and q=0. One data point was sacrificed so a principled analysis could proceed.

Price even supplied a model for how this might be encompassed in a Bayesian framework. He imagined a very large die with an essentially infinite number of sides, representing the possibilities for observation by our newly arrived person – all unknown prior to his arrival. All sides of the die were unknown; the observer would then see only that the die delivers a face with a risen Sun, but that is only an existence proof: Are there other faces that show a Sun? Or is this an exceedingly rare event? What fraction of the faces would give a Sun? After the first day this question can be asked, and a uniform distribution of the fraction is at least a feasible way to express uncertainty. But that was not possible before the first Sun was seen.

Price's caution in addressing the probabilities of hypotheses suggested by data is rare in early literature. One exception is the Cambridge logician John Venn. Venn in his *Logic of Chance* noted that William Stanley Jevons had been too eager to accept a naïve view when Jevons wrote this in his *Principles of Science*:

> "The first time a casual event happens it is 2 to 1 that it will happen again; if it does happen it is 3 to 1 that it will happen a third time; and on successive occasions the like kind of odds become 4, 5, 6, &c., to 1. … Thus on the first occasion on which a person sees a shark, and notices that it is accompanied by a little pilot fish, the odds are 2 to 1, or the probability 2/3, that the next shark will be so accompanied." (Jevons, 1877, p. 258)

Venn rejoined, "To say nothing of the fact that recognizing and naming the fish implies that they have often been seen before, how many of the observed characteristics of that single 'event' are to be considered essential? Must the pilot precede; and at the same distance? Must we consider the latitude, the ocean, the season, the species of shark, as matter also of repetition on the next occasion? and so on. I cannot see how the Inductive problem can be even intelligently stated, for quantitative purposes, on the first occasion of any event." (Venn, 1888, p. 198)

Venn's comment was new with his 3rd edition of 1888, and he quotes correctly from Jevons's 2nd edition of 1877, where Jevons's numbers are in line with Laplace's interpretation, where after the first occurrence the odds are 2 to 1, 3 to 1, and so forth. Had Venn looked at Jevons's 1st edition of 1871 he would have found at p.299 of Volume 1 a slight but interesting difference: There Jevons gives the example (shark and all) but with what are essentially Price's odds, 1 to 1, 2 to 1, and so forth! Jevons gave no source in either case and no explanation for the change. Venn presumably was unaware of this change, but one wonders if his indictment of a data determined hypothesis would have been softened if he had been aware of Price's argument.

Price's Motive

Why did Bayes write the Dissertation? Why did Price undertake the considerable effort to work out the extensions and produce the examples for Bayes's work? A number of writers (see references) have speculated that one or both of them were provoked by David Hume's skepticism on induction, and some (including me) that it was even more specifically directed towards Hume's essay On Miracles, first published in 1748. But the direct evidence is skimpy – Hume is nowhere mentioned, and the applications, while possibly suggestive, are not developed to any such apparent end. There is, however, one of Price's examples that gives a strong indirect clue.

When Price is discussing his die of very many sides, he does two specific calculations that in modern notation amount to the following:

Let a = 1,600,000/1,600,001 and b = 1,400,000/1,400,001. Suppose the die were thrown a million times, with the x = the probability of face M appearing, and suppose M occurs on every trial. Then p = 1,000,000 and q = 0, and then $\Pr\{x<b|p,q\} = b^{p+1} = 0.4895$ and $\Pr\{x>a|p,q\} = 1- \Pr\{x<a|p,q\} = 1-a^{p+1} = 1 - 0.5353 = 0.4647$. These two calculations establish that the posterior median of x lies between a and b, but since $\Pr\{a<x<b|p,q\} = 0.5353-0.4895 = 0.0457$, the interval has a relatively small probability, Price then adds as a more reasonable posterior interval (c,d), where c = 600,000/600,001 and d = 3,000,000/3,000,001, and he computes $\Pr\{c<x<d|p,q\} = d^{p+1} - c^{p+1} = .527$. All of these calculations are correct.

There is an indication Price did more investigation: He wrote "It may deserve to be added, that it is more probable that this proportion [i.e. the odds of seeing M, given p=1,000,000 and q=0] lies somewhere between that of 900,000 to 1 and 1,900,000 to 1 than between any other two proportions whose antecedents are to one another as 900,000 to 1,900,000, and consequents unity." Apparently he claimed that for any a_1= 900,000k/(900,000k +1) and a_2 = 1,900,000k/(1,900,000k +1), the probability $\Pr\{a_1<x<a_2|p=1{,}000{,}000 \text{ and } q = 0\}$ is maximum when k = 1.0. This is very close to being true: The maximizing k is in the interval (.6, 1.0) and the probability is to two places equal to 0.26 throughout that interval, but the true maximum is not as claimed.

The point is that Price went to considerable trouble to look carefully at a particular situation involving a die that on the face of it does not deserve such attention. What did he have in mind that led him to do this? What was his motive, if we rule off-the-table it being only a simple illustration of the application of Bayes's theoretical rules? A reader of Bayes's Essay alone will not find the answer to this.

Price and Hume

David Hume's essay "On Miracles" was published in April 1748 and received a considerable (and mostly hostile) reaction. He argued that testimony in favor of any given miracle should be ruled irrelevant as evidence in support of any religion, that a miracle was by his definition a violation of natural law, a law such as that dead men never return to life, established through observation over such a very long time period that it was in all these cases more likely that the report of the miracle was a lie, than that the event actually occurred. "No testimony is sufficient to establish a miracle, unless the testimony be of such a kind, that its falsehood would be more miraculous than the fact which it endeavors to establish. … . When any one tells me that he saw a dead man restored to life, I immediately consider with myself, whether it be more probable that the person should either deceive or be deceived, or that the fact he relates should really have happened." (Hume, 1748) In effect, it was a comparison of two roughly defined probabilities, the one based on past experience with inaccuracy of human accounts and the other on a near eternity of observation. Whether it was this essay that inspired Bayes (who seems to have worked on probability later in 1748) remains a matter of speculation. Not so with Price.

Price was no stranger to probability. In his first book, published three years before Bayes died, he had used an analogy to a die – indeed a die with a million faces – to explain the difference between *impossibility* as used commonly in conversation and *physical impossibility*. The former was like the event of throwing a designated one of the million sides, and the latter the throwing of a face that did not exist. In the former case, in very many throws the face would eventually occur; in the latter case, it never would (Price, 1758, pp. 431-436).

When Price had the chance to edit Bayes, he took this further, and the proof of his motive was in his second book, published not long after the Bayes articles, where he took the argument I have described from the Appendix and applied it, in a new and specific context, with exactly the numbers previously given, in order to refute Hume's "On Miracles" on Hume's own terms – reasoning based upon experience (Price, 1767, pp. 382ff). Writing in 1767, but with the die now representing the regular recurrence of the high tide, a natural law indeed, he could now suppose the tide had come in as scheduled p= 1,000,000 times without fail – Hume's "experience" – and yet Price claimed there was still an appreciable chance that it could fail in the future. Now we can see why Price expanded so much effort trying to pin down the posterior distribution, showing not only that the median corresponded to odds between 1,600,000 to 1 and 1,400,000 to 1, but also that ranges for the odds of 3,000, 000:1 to 600,000:1 or from 1,900,000:1 to 900,000:1 contained appreciable posterior probability (.527 and .26, respectably). These odds corresponded to different degrees of what he called *impossibility* in 1758, but not to *physical impossibility*, and in 1767 he drove this difference home by adding that if the more extreme median value (1,600,000:1) was assumed, and the tide observed to run for an additional 1,600,000 cycles, it would be expected that there would be at least one failure. Indeed, as Price had shown already in the appendix, p=1,000,000 successes and q=0 failures in 1,000,000 trials would put the chance that the *next* 1,000,000 trials would produce no failures at a mere .5353, if the chance of success were 1,600,000/1,600,001. You do not have to accept the relevance of Price's model to the physical process to admire his facility in using it in a dispute where the willingness to accept it in the philosophical sense was less likely to be challenged.

Price's nephew, William Morgan, says in a memoir of Price's life that, "The Dissertation on Miracles [Price's Fourth Dissertation in Price (1767)] had been written as early as the year 1760, and read to Mr. Canton, Mr. Rose, and some other friends, who all concurred in recommending the publication of it." (Morgan, 1815, page 23) In the 1767 publication, the mathematical material appears in footnotes with citation to the Royal Society publication, and this suggests that the earlier version did not have that material, that it was only while working on Bayes's Essay that Price worked up the mathematical case against Hume.

Price's Collegiality

Price's 1767 discussion of Hume began politely, referring to Hume as "a writer whose genius and abilities are so distinguished, as to be above any of *my* commendations." (Price, 1767, p. 383) As he proceeded in attacking Hume's essay the language became stronger, e.g. "…he who should make a mystery of such an expectation, or apprehend any difficulty in accounting for it, would deserve more to be laughed at than argued with." (p. 391). But shortly after the book was published in early 1767, Price met Hume (see Stephens, 1980, who builds on and corrects an error in Morgan, 1815), and the human face of the entirely reasonable Hume led to a change in expression, though no change in view. Price quickly issued a second edition, with this remarkable passage in the introduction:

> "Every expression likewise in that Dissertation which had any appearance of an undue severity with respect to Unbelievers has been altered. — The Author is sensible that there are few or no controversies in which it is right to charge an adversary with want of

candour and disingenuity. Such charges give no strength to an argument. They always irritate instead of doing good, and it seldom happens that they are not capable of being retorted." (Price, 1768, p. v)

And indeed there were numerous small changes of expression. One example will suffice:

Price, 1767, p. 388-389: "This is the objection in its complete force. It has, we see, a plausible appearance, and is urged with uncommon confidence. But, it is founded on indisputable fallacies, and is indeed nothing but a poor though specious sophism. I cannot hesitate in making this assertion; and, I think, it must appear to be true, to any one who will bestow attention on the following observations."

Price, 1768, p. 388: "This is the objection in its complete force. It has, we see, a plausible appearance, and is urged with much confidence. But I cannot hesitate in asserting that it is founded on false principles; and, I think, this must appear to be true, to any one who will bestow attention on the following observations."

After Price and Hume had first encountered and Price had apologized for his tone, Hume wrote to Price: "So far from there being any occasion to make me an Apology for your late Publication that you have prevented me in my Intentions of writing to you, and of returning you thanks for the Civility with which you have treated me. I had almost said unusual Civility. For to the Reproach of Learning, it is but too rare to find a literary Controversy conducted with proper decency and Good manners, especially where it turns upon Religious Subjects, in which men often think themselves at Liberty to give way to their utmost Rancour and Animosity. But you like a true Philosopher, while you overwhelm me with the Weight of your Arguments, give me encouragement by the Mildness of your Expressions: and instead of *Rogue, Rascal,* and *Blockhead*, the illiberal language of the Bishop of Glocester [sic] and his School, you address me, as a man mistaken, but capable of Reason and conviction. I own to you, that the Light, in which you have put this Controversy, is new and plausible and ingenious, and perhaps solid. But I must have some more time to weigh it, before I can pronounce this Judgment with Satisfaction to myself." (Hume to Price, 18 March 1767, in Thomas & Peach, 1983, pp. 45-47)

Apparently Hume never felt the need to retract his argument, but the collegiality these two expressed may stand as a model for philosophical and scientific discourse.

Conclusion

Stephen Fienberg has traced the term "Bayesian" back only to about 1950, where the use was by R. A. Fisher as a mild term of disapprobation, a sense of meaning that it has long since shed (Fienberg, 2006). Despite the anachronism, Richard Price's principled and careful application of Bayes's Rules to a specific problem, albeit one in moral philosophy, seems sufficient to earn for him the sobriquet, The First Bayesian. "Bayesian" because he argued for a particular prior, deduced the posterior, and went to lengths in calculating and interpreting descriptions of it – the posterior median and two different posterior credible regions: all of this is agreeable with the general sense of the modern term. And this was not as a simple illustration; indeed, it seems to have been the driving motive behind his bringing Bayes's own work to public attention. And "First" because, well, who could come closer in time to Bayes than his own editor?

This does not detract from Bayes's own credit; Price was quite clear on what was due to Bayes. In his own paper of 1765 Price summed up as follows:

> "The solution of the problem enquired after in the papers I have sent you has, I think, been hitherto a *desideratum* in philosophy of some consequence. To this we are now in

> a great measure helped by the abilities and skill of our late worthy friend; and thus are furnished with a necessary guide in determining the nature and proportions of unknown causes from their effects, and an effectual guard against one great danger to which philosophers are subject; I mean, the danger of founding conclusions with more assurance than the number of experiments will warrant." (Price, 1765, p. 297)

To the end of his life Price held Bayes in the highest esteem. In 1787 Price added to a footnote to the 3rd edition of his very first book this statement: "The author [of an anonymously published 1731 tract, *Divine Benevolence*] was Mr. Bayes, one of the most ingenious men I ever knew, and for many years the minister of a dissenting congregation at Tunbridge Wells." (Price, 1758, edition of 1787, p. 429) In this Price was echoing an assessment by David Hartley in 1749, when Hartley wrote, "An ingenious Friend has communicated to me a Solution of the inverse Problem," and went on the state Bayes's result in essentially the same words Bayes and Price used, without directly giving the friend's name. (Hartley, 1749, p. 339; Stigler, 1983). And so we have the ingenious mathematical theoretician Thomas Bayes, and his friend, the perceptively subtle explorer of applications Richard Price: like Crick and Watson, von Neumann and Morgenstern, Neyman and Pearson, they are two people who combined complementary skills to achieve a special end.

Yale University held a full day commencement on September 12, 1781, the University's first public celebration after six years of war. On that occasion they awarded the honorary degree of Doctor of Laws to each of two distinguished men. Both were revolutionaries in their own ways, and both were friends of Benjamin Franklin, who probably played a role in their selection. One was George Washington, the hero of the American Revolution; the other was

Richard Price, a consistent British friend of America throughout that period. Neither attended – it was not the custom at that time – and Price only learned of the degree in 1783, after peace was formally signed. It was a fitting pairing, of Washington, who through his life adhered to a list he had copied into a notebook in his youth of 110 "Rules of Civility and Decent Behavior in Company and Conversation," and Price, who set a high standard for collegial behavior and rational discourse, both men models of behavior that should be imitated more often today.

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Figure: A Large Die. This die, smaller, but otherwise of the sort Richard Price envisioned, dates from the 18th century, but nothing more is known about its origin or purpose. It has 32 sides, and is made of brass. Of course there is no convex regular polyhedron (no Platonic solid) with more than 20 sides, so there are necessarily irregularities. Quite likely the 32 sides are not equally probable, although that cannot be proved by simple inspection. But regardless of that problem, any experimentation with this die makes clear that accurately determining the "up side" after a roll is quite difficult with a large number of faces; the best one can do is to determine which face is flat against the table. One implication of this is that writers who describe an experiment with such a die have never actually used one (this includes De Moivre and Price). As a mental experiment, an imagined concrete physical analog for many chances, such a die is fine. But it is impractical for actual experimentation.