

# On Individual Risk

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## Abstract

We survey a variety of possible explications of the term “Individual Risk.” These in turn are based on a variety of interpretations of “Probability,” including Classical, Enumerative, Frequency, Formal, Metaphysical, Personal, Propensity, Chance and Logical conceptions of Probability, which we review and compare. We distinguish between “groupist” and “individualist” understandings of Probability, and explore both “group to individual” (G2i) and “individual to group” (i2G) approaches to characterising Individual Risk. Although in the end that concept remains subtle and elusive, some pragmatic suggestions for progress are made.

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# 1 Introduction

“Probability” and “Risk” are subtle and ambiguous concepts, subject to a wide range of understandings and interpretations. Major differences in the interpretation of Probability underlie the fundamental Frequentist/Bayesian schism in modern Statistical Science. However, these terms, or their synonyms, are also in widespread general use, where lack of appreciation of these subtleties can lead to ambiguity, confusion, and outright nonsense. At the very least, such usages deserve careful attention to determine whether and when they are meaningful, and if so in how many different ways. The focus of this article will be on the concept of “Individual Risk,” which I shall subject to just such deep analysis.

To set the scene, §2 presents some examples displaying a variety of disparate usages of the concept of Individual Risk. These, in turn, are predicated on a variety of different understandings of the concept of Probability. I survey these in §3, returning to discuss the examples in their light in §4. Section 5 describes the concept of “expert assignment,” which is common to a number of understandings of Risk. In §6 and §7 I consider various aspects of “group to individual” (G2i) inference—the attempt to make sense of Individual Risk when Probability is understood as a group phenomenon—and conclude that this can not be done in an entirely satisfactory way. So §8 reverses this process and considers “individual to group” (i2G) inference, where we take Individual Risk as a fundamental concept, and explore what that implies about the behaviour of group frequencies. The i2G approach appears to lead to essentially unique individual risk values; but these can not be considered absolute, but are relative to a specified information base, and moreover are typically not computable. In §9 I take stock of the overall picture. I conclude that the concept of “Individual Risk” remains highly problematic at a deep philosophical level, but that does not preclude our making some pragmatically valuable use of that concept—so long as we are aware of the various pitfalls that may lie in our path.

## 2 Examples

We start with a *ménagerie* of examples of “individual risk” in public discourse.

**Example 1** A weather forecaster appears on television every night and issues a statement of the form “The probability of precipitation tomorrow is 30%” (where the quoted probability will of course vary from day to day). Different forecasters issue different probabilities. □

**Example 2** There has been much recent interest concerning the use of Actual Risk Assessment Instruments (ARAI): statistical procedures for assessing “the risk” of an individual becoming violent (Singh and Petrila 2013). Thus a typical output of the Classification of Violence Risk (COVR) software program, that can be used to inform diagnostic testimony in civil commitment cases, might be: “The likelihood that XXX will commit a violent act toward another person in the next several months is estimated to be between 20 and 32 percent, with a best estimate of 26 percent.” □

**Example 3** Aharoni *et al.* (2013) tested a group of released adult offenders on a go/no-go task using fMRI, and examined the relation between task-related activity in the anterior cingulate cortex (ACC) and subsequent rearrest (over four years), allowing for a variety of other risk factors. They found a significant relationship between ACC activation on the go/no-go task and subsequent rearrest; whereas subjects with high ACC activity had a 31% chance of rearrest, subjects with low ACC activity had a 52% chance. They conclude: “These results suggest a potential neurocognitive biomarker for persistent antisocial behavior.”

A newly released offender has low ACC activity: how should we judge his chance of rearrest? □

**Example 4** Writing in the New York Times (May 14 2013) about her decision to have a preventive double mastectomy, the actress Angelina Jolie said: “I carry a faulty gene, BRCA1, which sharply increases my risk of developing breast cancer and ovarian cancer. My doctors estimated that I had an 87 percent risk of breast cancer and a 50 percent risk of ovarian cancer, although the risk is different in the case of each woman.” □

**Example 5** The Fifth Assessment Report of the Intergovernmental Panel on Climate Change, issued in September 2013, contains the statements “It is *extremely likely* that human influence has been the dominant cause of the observed warming since the mid-20th century,” “It is *virtually certain* that the upper ocean (0–700 m) warmed from 1971 to 2010, and it *likely* warmed between the 1870s and 1971,” and “It is *very likely* that the Arctic sea ice cover will continue to shrink and thin and that Northern Hemisphere spring snow cover will decrease during the 21st century” (their italics). It is explained that *virtually certain* is equivalent to a probability of at least 99%, *extremely likely*, at least 95%, *very likely*, at least 90%, and *likely*, at least 66%. □

**Example 6** On 4 May 2011, three days after he announced that American troops had killed Osama bin Laden in Pakistan, US President Barack Obama said in an interview with “60 Minutes” correspondent Steve Kroft:

At the end of the day, this was still a 55/45 situation. I mean, we could not say definitively that bin Laden was there.

□

**Example 7** In a civil court case, the judgment might be expressed as: This is typically interpreted as “with probability exceeding 50%.” □

In all the above examples we can ask: How were the quoted probabilities interpreted? How might they be interpreted? And how might the quality of such probability forecasts be measured?

### 3 Interpretations of Probability

We have already remarked that the concept of “Probability” is a hotly contested philosophical issue.<sup>1</sup> Even the many who have no patience for such philosophising are usually in thrall to some implicit philosophical conception, which shapes their approach and understanding, and their often fruitless arguments with others who (whether or not so recognised) have a different understanding.

One axis along which the different theories and conceptions of Probability can be laid out—and which is particularly germane to our present purpose—is whether they regard Probability as fundamentally an attribute of groups, or of individuals. I will refer to these as, respectively, “groupist” and “individualist” theories.

Below, in a necessarily abbreviated and admittedly idiosyncratic account, I outline some of the different conceptions of Probability, and hope to bring out their relationships, similarities and differences. For a fuller discussion see *e.g.* Hájek (2012); Galavotti (2014).

#### 3.1 Classical Probability

If you studied any Probability at school, it will have focused on the behaviour of unbiased coins, well-shuffled packs of cards, perfectly balanced roulette wheels, *etc.*, *etc.* In short, an excellent training for a life misspent in the Casino. This is the ambit of *Classical Probability*.

The underlying conception is that we have a number of *elementary outcomes* of an experiment, exactly one of which will actually be realised when the experiment is performed. For example, there are  $N = 53,644,737,765,488,792,839,237,440,000$  ways in which the cards at Bridge can be distributed among 4 players, and just one of these ways will materialise when the cards are dealt. Any event of interest, for example “North holds 3 aces,” can be represented by the set of all the elementary outcomes for which it is the case; and the number  $n$  of these, divided by the total number  $N$  of all elementary outcomes, is taken as the measure of the probability of the event in question. The mathematics of Classical Probability is thus really a branch of Combinatorial Analysis, the far from trivial mathematical theory of counting.

Since the focus is on the specific outcome of (say) a particular deal of cards or roll of a die, this classical conception is individualist. But questions as to the interpretation of the “probabilities” computed rarely raise their heads. If they do, it would typically be assumed (or regarded as a precondition for the applicability of the theory) that each of the elementary outcomes is just as likely as any other to become the realised outcome. However, in the absence of any independent understanding of the meaning of “likely,” such an attempt at interpretation courts logical circularity. Furthermore, paradoxes arise when there is more than one natural way to describe what the elementary events should be. If we toss two coins, we could either form 3 elementary events: “0 heads,” “1 head,” “2

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<sup>1</sup>Interestingly, this is not the case for its mathematical properties, where the “Kolmogorov axioms” are largely accepted by all the main factions.

heads”; or, taking order into account, 4 elementary events: “tail tail,” “tail head,” “head tail,” “head head.” There is nothing within the theory to say we should prefer one choice over the other. Also, it is problematic to extend the classical conception to cope with an infinite number of events: for example, to describe a “random positive integer,” or a line intersecting a given circle “at random.”

The principal modern application of Classical Probability is to situations—for example, clinical trials—where randomisation is required to ensure fair allocation of treatments to individuals. This can be effected by tossing a “fair coin” (or by simulating such tosses on a computer).

### 3.2 Enumerative Probability

What I<sup>2</sup> here term *Enumerative Probability* can also be regarded as an exercise in counting. Only now, instead of counting elementary outcomes of an experiment, we consider a finite collection of individuals (of any nature), on which we can measure one or more pre-existing attributes.

Thus consider a set of individuals,  $I_1, I_2, \dots, I_N$ , and an attribute  $E$  that a generic individual  $I$  may or may not possess: *e.g.* “smoker.” A specific individual  $I_k$  can be classified according to whether or not its “instance,”  $E_k$ , of that attribute is present—*e.g.*, whether or not  $I_k$  is a smoker. If we knew this for every individual, we could compute the *relative frequency* with which  $E$  occurs in the set, which is just the number of individuals having attribute  $E$ , divided by the total number  $N$  of individuals. This relative frequency is the “enumerative probability” of  $E$  in the specified set. Clearly this is a “groupist” conception of probability.

Of course, the value of such an enumerative probability will depend on the set of individuals considered, as well as what attributes they possess. We will often care about the constitution of some specific large population (say, the population of the United Kingdom), but have data only for a small sample. Understanding the relationship between the known frequencies in the sample and the unknown frequencies in the whole population is the focus of the theory and methodology of sample surveys. This typically requires the use of randomisation to select the sample—so relying, in part, on the Classical conception.

Although based on similar constructions in terms of counting and combinatorial analysis, Classical and Enumerative Probability are quite different in their scope and application. Thus suppose we ask: What is “the probability” that a new-born child in the United Kingdom will be a boy? Using the Classical interpretation, with just two outcomes, boy or girl, the answer would be  $1/2 = 0.5$ . But in the UK population about 53% of live births are male, so the associated Enumerative Probability is 0.53.

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<sup>2</sup>Though probably no one else. Hájek (2012) uses the term “finite frequentism.”

### 3.3 Frequency Probability

“Frequency probability” can be thought of as enumerative probability stretched to its limits: instead of a finite set of individuals, we consider an infinite set.

There are two immediate problems with this:

1. Infinite populations do not exist in the real world, so any such set is an idealisation.
2. Notwithstanding that the great statistician Sir Ronald Fisher manipulated “proportions in an infinite population” with great abandon and to generally good effect, this is not a well-defined mathematical concept.

These problems are to some extent resolved in the usual scenario to which this concept of probability is attached: that of *repeated trials under identical conditions*. The archetypical example is that of tosses of a possibly biased coin, supposed to be repeated indefinitely. The “individuals” are now the individual tosses, and the generic outcome is (say) heads (H), having instances of the form “toss  $i$  results in heads.” It is important that the “individuals” are arranged in a definite sequence, for example time order. Then for any finite integer  $N$  we can restrict attention to the sequence of all tosses from toss 1 up to toss  $N$ , and form the associated enumerative probability,  $f_N$  say, of heads: the relative frequency of heads in this finite set. We next consider how  $f_N$  behaves as we increase the total number  $N$  without limit. If  $f_N$  approaches closer and closer to some mathematical limit  $p$ —the “limiting relative frequency” of heads—then that limiting value may be termed the “frequency probability” of heads in the sequence.

Of course we can never observe infinitely many tosses, so even the existence of the limit must remain an assumption: one that is, however, given some empirical support by the observed behaviour of real-world frequencies in such repeated trial situations. But even when we can happily believe that the limit  $p$  exists, we will never have the infinite amount of data that would be needed to determine its value precisely. Much of the enterprise of *statistical inference* addresses the subtle relationship between actual frequencies, observed in finite sequence of trials, and the ideal “frequency probabilities” that inhabit infinity.

Richard von Mises (1939) attempted to build a sophisticated mathematical theory of probability built upon the above frequency conception as its primitive element, but that is now largely of historical interest.

### 3.4 Formal Probability

A prime task for an applied statistician is to build a “statistical model” of a phenomenon of interest. That phenomenon could be fairly simple, such as recording the outcomes of a sequence of 10 tosses of a coin; or much more sophisticated, such as a description of the earth’s temperature as it varies over space and time. Any such model will have symbols representing particular outcomes and quantities (*e.g.*, the result of the 10th toss of the coin, or the recorded temperature at the Greenwich Royal Observatory at noon GMT on 24 September 2020), and will model these (jointly with all the other outcomes and quantities under consideration) as “random variables,” having a joint probability distribution.

Typically the probabilities figuring in this distribution are treated as unknown, and statistical data-analysis is conducted to learn something about them. There does not seem to be a generally accepted terminology for these probabilities figuring in a statistical model: I shall term them “Formal Probabilities.”

Since the focus of statistical attention is on learning these formal probabilities, I find it remarkable how little discussion is to be found as to their meaning and interpretation. In particular, is Formal Probability an individualist or a groupist conception? The elementary building blocks of the statistical model are assignments of probabilities to specific events, so *prima facie* it looks like an individualist conception. One possible individualist interpretation of a formal probability value is as a propensity—see §3.6 below—although this raises problems of its own.

In any case, many users of statistical models would not be happy to accept an individualist interpretation of their formal probabilities, and prefer a groupist account of them. Thus consider the simple case of coin-tossing. The usual statistical model for this assigns some common (though typically unknown) formal probability  $p$  to each event  $E_k$ : “the  $k$ th toss  $I_k$  results in heads,” for every  $k$ ; and further models all these tosses, as  $k$  varies, as independent. An application of Bernoulli’s Law of Large Numbers shows that this implies that, with probability 1, the limiting relative frequency of heads in the sequence will exist and have value  $p$ . And many statisticians would claim that—notwithstanding that the formal probabilities are attached to individual tosses—this groupist Frequency Probability interpretation is the *only* meaningful content of the (superficially individualist) formal “Bernoulli model.”

However, such a ploy is far from straightforward for more complex models, where there may be no natural way of embedding an individual event in a sequence of similar events. For example, if we construct a spatio-temporal statistical model of the weather, the model might well contain a parameter that measures the correlation between the temperatures in Greenwich and New York at noon GMT on 24 September 2020. But just how is such a correlation, between two “one-off” quantities, to be interpreted? Does it have any real-world counterpart?

One possible way of taking the above “Law of Large Numbers” approach for the Bernoulli model and extending it to more general models (Dawid 2004) is by identifying those events that are assigned probability 1 by the assumed model, and asserting that the *only* valid interpretation of the model lies in its claim that these events will occur. But this may be seen as throwing too much away.

Because there is little discussion and no real shared understanding of the interpretation of Formal Probability, pointless disagreements can spring up as to the appropriateness of a statistical model of some phenomenon. An example of practical importance for forensic DNA profiling arises in population genetics, where there has been disagreement as to whether the genes of distinct individuals within the same subpopulation should be modelled as independent, as claimed by Roeder *et al.* (1998), or correlated, as claimed by Foreman *et al.* (1997). However, without a shared understanding of what (if anything) the correlation parameter in the formal model relates to in the real world, this is a pointless argument. It was pointed out by Dawid (1998) that (like blind men’s disparate understand-



ings of the same elephant) both positions can be incorporated within the same hierarchical statistical model, where the formal correlation can come or go, according to what it is taken as conditioned on. So while the two approaches superficially appear at odds, at a deeper level they are in agreement.

### 3.4.1 Metaphysical Probability

There are those who, insistent on having some sort of frequency-based foundation for probability, would attempt to interpret a “one-off” probability as an average over repetitions of the whole underlying phenomenon. For example, the correlation between the temperatures in Greenwich and New York at noon GMT on 24 September 2020 would be taken to refer to an average over—necessarily hypothetical—-independent repetitions of the whole past and future development of weather on Earth. This conception appears close to the “many worlds” interpretation of quantum probabilities currently popular with some physicists. I confess I find it closer to science fiction than to real science—“metaphysical,” not “physical.” At any rate, since we can never observe beyond the single universe we in fact inhabit, we can not make any practical use of such hypothetical repetitions.

That said, this way of thinking can be useful, purely as an analogy, in helping people internalise a probability value (whatever its provenance or philosophical back-story). Thus one of the options at

<http://understandinguncertainty.org/files/animations/RiskDisplay1/RiskDisplay.html>

for visualising a risk value is a graphic containing a number of symbols for your “possible futures,” marked as red to indicate that the event occurs in that future, green that it does not. The relevant probability is then represented by the proportion of these symbols that are red. It is not necessary to take the “possible futures” story seriously in order to find this enumerative representation of a probability value helpful. The same purely psychological conceit explains the misleading name given to the so-called “frequentist” approach to statistical inference. This involves computing the probabilistic properties of a suggested statistical procedure. For example, a test of a null hypothesis at significance level 5% is constructed to have the property that probability of deciding to reject the hypothesis, under the assumption that it is in fact correct, is at most 0.05. Fisher’s explication of such a test was that, when the it results in the decision to reject, either the null hypothesis was indeed false, or else an event of small probability has happened—and since we can largely discount the latter alternative, we have evidence for the former. Although for this purpose it would be perfectly satisfactory to interpret the probability value 0.05 as an “individual chance,” relating solely to the current specific application of the procedure, it is almost universally expressed metaphysically, by a phrase such as “over many hypothetical repetitions of the same procedure, when the null hypothesis is true, it will rejected at most 5% of the time.”

## 3.5 Personal Probability

*Personal* (often, though less appropriately, termed *Subjective*) *Probability* is very much an

individualist approach. Indeed, not only does it associate a probability value, say  $p$ , with an individual event, say  $E$ , it also associates it with the individual, say “You,” who is making the assignments, as well as (explicitly or implicitly) with the information, say  $H$ , available to You when You make the assignment. Thus if You are a weather forecaster, You might assess Your probability of rain tomorrow, given Your knowledge of historical weather to date, at 40%. Another forecaster would probably make a different personal assessment, as would You if You had different information (perhaps additional output from a meteorological computer system) or were predicting for a different day.

What is the meaning of Your 40%? The standard view is that it simply describes the odds at which You would be willing to bet on “rain tomorrow.” If Classical Probability is modelled on the casino, then Personal Probability is modelled on the race-track.

If You really really had to bet, You would have to come up with a specific numerical value for Your personal probability. Thus to a Personalist there is no such thing as an “unknown probability,” and it appears that these should not feature in any probability model You might build. There is however an interesting and instructive relationship between such Personalist models and the statistical models, as described in §3.4, that do feature unknown formal probability values—for more on this see §6.2 below.

### 3.6 Propensity and chance

The interpretation of Probability as a “Propensity” was championed by Popper (1959), and is still much discussed by philosophers—though hardly at all by statisticians. The overall idea is that a particular proposed coin-toss (under specified circumstances) has a certain (typically unknown) “propensity” to yields heads, if it were to be conducted—just as a particular lump of arsenic has a propensity to cause death if it were to be ingested. This is clearly an individualist (though non-Personalist) conception of Probability, but there is little guidance available as to how a propensity probability is to be understood, or how its value might be assessed, except by reference to some “groupist” frequency interpretation. Indeed, some versions of the propensity account (Gillies 2000) take them as referring directly to the behaviour of repetitive sequences, rather than to that of individual events.

Another individualist term that is very close in spirit to “propensity” is “(objective) chance.” The “Principal Principle” of Lewis (1980), while not defining chance, relates it to personal probability by requiring that, if You learn (don’t ask how!) that the chance of an event  $A$  is (say) 0.6, and nothing else, then Your personal probability of  $A$  should be updated to be 0.6. And further conditions may be required: for example, that this assessment would be unaffected if You learned of any other “admissible” event, where “admissible” might mean “prior to  $A$ ,” or “independent of  $A$  in their joint chance distribution.” Such conditions are difficult to make precise and convincing (Pettigrew 2102). In any case, the Principal Principle gives no guidance on how to compute or estimate an individual chance.

### 3.7 Logical Probability

Yet another non-Personalist individualist conception of Probability is Logical Probability, associated with Jeffreys (1939); Carnap (1950) and others. This is similar to the propensity/chance account in considering that there is an “objective” probability value associated with a given outcome, but differs from those in emphasising the relativity of that value to the information upon which it is premised. Once again there would seem to be no routine and unambiguous way of computing such logical probability values.

## 4 Examples Revisited

We here revisit the examples of §2 in the light of some of the above discussion. My own comments are interpolated in [...].

**Example 1** Gigerenzer *et al.* (2005) randomly surveyed pedestrians in five metropolises located in countries that have had different degrees of exposure to probabilistic forecasts: Amsterdam, Athens, Berlin, Milan, and New York. Participants were told to imagine that the weather forecast, based on today’s weather constellation, predicts “There is a 30% chance of rain tomorrow,” and to explain what they understood by that.

Several people in New York and Berlin thought that the rain probability statement means “3 out of 10 meteorologists believe it will rain” [a form of enumerative probability?]. A woman in Berlin said, “Thirty percent means that if you look up to the sky and see 100 clouds, then 30 of them are black” [a different form of enumerative probability]. Participants in Amsterdam seemed the most inclined to interpret the probability in terms of the amount of rain. “It’s not about time, it indicates the amount of rain that will fall,” explained a young woman in Amsterdam. Some people seemed to intuitively grasp the essence of the “days” interpretation, albeit in imaginative ways. For instance, a young woman in Athens in hippie attire responded, “If we had 100 lives, it would rain in 30 of these tomorrow” [a metaphysical interpretation?]. One of the few participants who pointed out the conflict between various interpretations observed, “A probability is only about whether or not there is rain, but does not say anything about the time and region,” while another said, “It’s only the probability that it rains at all, but not about how much” [two individualist views]. Many participants acknowledged that, despite a feeling of knowing, they were incapable of explaining what a probability of rain means.

[According to the authors of the paper, the standard meteorological interpretation is: when the weather conditions are like today, in 3 out of 10 cases there will be (at least a trace of) rain the next day.]

□

**Example 2** There has been heated recent debate centred on the construction and validity of confidence intervals for the “individual risks” output by an ARAI. This has been initiated and promoted by Hart *et al.* (2007); Cooke and Michie (2010); Hart and Cooke (2013), whose analysis has had a strong influence (see *e.g.* Singh and Petrila (2013)), but has been widely criticised for serious technical statistical errors and confusions (Harris *et al.* 2008; Hanson and Howard 2010; Imrey and Dawid 2014; Mossman 2014). However, to date that debate has largely sidestepped the crucial question of the meaning of such an individual risk.

Hart *et al.* (2007) make the following argument in an attempt to distinguish between group and individual risks:

To illustrate our use of Wilson’s method for determining group and individual margins of error, let us take an example. Suppose that Dealer, from an ordinary deck of cards, deals one to Player. If the card is a diamond, Player loses; but if the card is one of the other three suits, Player wins. After each deal, Dealer replaces the card and shuffles the deck. If Dealer and Player play 10 000 times, Player should be expected to win 75% of the time. Because the sample is so large, the margin of error for this group estimate is very small, with a 95% CI of 74–76% according to Wilson’s method. Put simply, Player can be 95% certain that he will win between 74 and 76% of the time. However, as the number of plays decreases, the margin of error gets larger. If Dealer and Player play 1000 times, Player still should expect to win 75% of the time, but the 95% CI increases to 72–78%; if they play only 100 times, the 95% CI increases to 66–82%. Finally, suppose we want to estimate the individual margin of error. For a single deal, the estimated probability of a win is still 75% but the 95% CI is 12–99%. The simplest interpretation of this result is that Player cannot be highly confident that he will win—or lose—on a given deal

[On 10 000, 1000 or 100 deals, the actual success rate will vary randomly about its target value of 75%, and the “confidence intervals” (CIs) described for these cases are intended to give some idea of the possible extent of that variation. But on a single deal the actual success rate can only be 0 (which will be the case with probability 25%) or 100% (with probability 75%). This purely binary variation is not usefully described by any “confidence interval,” let alone the above one of 12–99%, based on a misconceived application of Wilson’s method. See Imrey and Dawid (2014) for further deconstruction of the spurious philosophical and mathematical arguments presented by these authors.] □

**Example 3** Poldrack *et al.* (2014) point out that the results given by Aharoni *et al.* (2013) provide inflated estimates of the predictive accuracy of the model when generalizing to new individuals: their reanalysis using a cross-validation approach found that addition of brain activation to the predictive model improves predictive accuracy by less than 3% compared to a baseline model without activation. The alternative bootstrap-based reanalysis in Aharoni *et al.* (2014) likewise finds the original estimates to have been over-inflated, though their correction is smaller. The message is that it can be extremely difficult to

obtain reliable estimates of reoffending rates, and any causal attribution, *e.g.* to ACC activity, would be even more precarious.

All these objections aside, suppose we had reliable estimates of the statistical parameters in a well-fitting statistical model, from which we could compute a “reoffending rate” for this new individual. Is it appropriate to regard such a formal probability value, based on a statistical analysis of group data, as the appropriate expression of his individual risk?  $\square$

**Example 4** Angelina Jolie’s statement sounds very much like an “individualist” interpretation of the figure “87 percent risk of breast cancer,” especially in the light of “the risk is different in the case of each woman.” However it would appear that this figure is taken from the website of Myriad Genetics, on which it says “People with a mutation in either the BRCA1 or BRCA2 gene have risks of up to 87% for developing breast cancer by age 70.” This is clearly an enumerative probability. So was Angelina (or her doctor) right to interpret it as her own individual risk?  $\square$

**Example 5** The events and scenarios considered by the IPCC are fundamentally “one-off,” so any attempt at interpreting the quoted probabilities must be individualistic. But of what nature? (It was suggested in a radio commentary that a probability of 95% means that 95% of scientists agree with the statement).  $\square$

**Example 6** It would be very interesting to know just how the President conceived of this 55% probability of success for a one-off event. We might also ask: Was that probability assessment justified (in any sense) by the turn-out of events?  $\square$

**Example 7** While there has been much discussion as to relationship between “legal” and “mathematical” probabilities, this is apparently an assertion about uncertainty in an individual case. Such assessments might best be construed as personal probabilities, though they are notoriously subject to bias and volatility (Fox and Birke 2002).  $\square$

## 5 Risk and Expert Assignments

Our focus in this article is on the concept of the single-case “individual risk,” and we shall be exploring how this is or could be interpreted from the point of view of the various different conceptions of probability outlined above. One theme common to a number of those conceptions is that of risk as an “expert assignment” (Gaifman 1988). This means that, if You start from a position of ignorance, and then somehow learn (only) that “the risk” (however understood) of outcome  $A$  is  $p$ , then  $p$  should be the measure of Your new uncertainty about  $A$ .

### 5.1 Personal Probability

Consider this first from the Personalist point of view. Suppose You learn the personal probability  $p$ , for event  $A$ , of an individual  $E$  (the “expert”) who started out with exactly

the same overall personal probability distribution as You did, but has observed more things, so altering her uncertainty. By learning  $p$  You are effectively learning, indirectly, all the relevant extra data that  $E$  has brought to bear on her uncertainty for  $A$ , so You too should now assign personal probability  $p$  to event  $A$ . Note that this definition of expert is itself a personal one: an expert for You need not be an expert for some one else with different opinions or knowledge.

This example shows that the property of being an expert assignment is quite weak, since there could be a number of different experts who have observed different things and so have different updated personal probabilities. Whichever one of these You learn, You should now use that value as Your own.

Regardless of which expert You are considering consulting (but have not yet consulted), Your current expectation of her expert probability  $p$  will be Your own, unupdated, personal probability of  $A$ . In particular, mere knowledge of the existence of an expert has no effect on the odds You should currently be willing to offer on the outcome of  $A$ .

What if You learn the personal probabilities of several different experts? It is far from straightforward to combine these to produce Your own revised probability (Dawid *et al.* 1995), since this must depend on the extent to which the experts share common information, and would typically differ from each individual expert assignment—even if these were all identical.

## 5.2 Chance

As for “objective chance,” the Principal Principle makes explicit that (whatever it may be) it should act as an expert assignment, and moreover that this should hold for every personalist—it is a “universal expert.” But while this constrains what we can take objective chance to be, it is far from being a characterisation.

## 5.3 Frequency Probability

The relationship between frequencies and expert assignments is considered in detail in §6 below.

# 6 Group to Individual Inference for Repeated Trials

As we have seen, some conceptions of Probability are fundamentally “groupist,” and others fundamentally “individualist.” That does not mean that a groupist approach has nothing to say about individual probabilities, nor that an individualist approach can not address group issues. But the journey between these two extremes, in either direction, can be a tricky one. Our aim in this article is to explore this journey, with special emphasis on the interpretation of individual probabilities, or “risks.” In this Section, we consider the “group to individual” (G2i) journey; the opposite (i2G) direction will be examined in §8 below.

We start by considering a simple archetypical example. A coin is to be tossed repeatedly. What is “the probability” that it will land heads (H) up (event  $E_1$ ) on the first toss ( $I_1$ )? We shall consider how various conceptions of Probability might approach this question.

## 6.1 Frequency probability

The frequency approach apparently has nothing to say about the first toss  $I_1$ . Suppose however that (very) lengthy experimentation with this coin has shown that the limiting relative frequency of heads, over infinitely many tosses, is 0.3. Can we treat that value 0.3 as representing uncertainty about the specific outcome  $E_1$ ? Put otherwise, can we treat the limiting relative frequency of heads as a (universal) expert assignment for the event of heads on the first toss?

While there is no specific warrant for this move within the theory itself, it would generally be agreed that we are justified in doing so if all the tosses of the coin (including toss  $I_1$ ) can be regarded as

“repeated trials of the same phenomenon under identical conditions.”

We shall not attempt a close explication of the various terms in this description, but note that there is a basic assumption of *identity of all relevant characteristics* of the different tosses.

## 6.2 Personal probability

By contrast, the Personalist You would be perfectly willing to bet on whether or not the next toss will land heads up, even without knowing how other tosses of the coin may turn out. But how do Your betting probabilities relate to frequencies?

In order to make this connexion, we have to realise that You are supposed to be able to assess Your betting probability, not merely for the outcome of each single toss, but for an arbitrary specified combination of outcomes of different tosses: for example, for the event that the results of the first 7 tosses will be HHTTHTH in that order. That is, You have a full personal *probability distribution* over the full sequence of future outcomes. In particular, You could assess (say) the *conditional* probability that the 101th toss would be H, given that there were (say) 75 Hs and 25 Ts on the first 100 tosses. Now, before being given that information You might well have no reason to favour H over T, and so assign *unconditional* probability close to 0.5 to getting a H at the 101th toss. However, after that information becomes available You might well favour a *conditional* probability closer to 0.75. All this is by way of saying that—unlike for the Bernoulli model for repeated trials introduced in §3.4 above—in Your joint betting distribution the tosses would typically *not* be independent; since, if they were, no information about the first 100 tosses could change Your probability of seeing H on the 101th.

But if You cannot assume independence, what can You assume about Your joint distribution for the tosses? Here is one property that might seem reasonable: that You simply do not care what order You will see the tosses in. This requires, for example, that Your

probability of observing the sequence HHTTHTH should be the same as that of the sequence HTHHTHT, and the same again for any other sequence containing 4 Hs and 3 Ts. This would not be so if, for example, You felt the coin was wearing out with use and acquiring an increasing bias towards H as time passes.

This property of the irrelevance of ordering is termed *exchangeability*. It is much weaker than independence, and will often be justifiable, at least to an acceptable approximation.

Now it is a remarkable fact (de Finetti 1937) that, so long only as Your joint distribution is exchangeable, the following must hold:

1. You believe, with probability 1, that there will exist a *limiting relative frequency*,  $p = \lim_{N \rightarrow \infty} f_N$ , of H's, as the number  $N$  of tosses observed tends to infinity.
2. You typically do not initially know the value of  $p$  (though You could place bets on that value—You have a distribution for  $p$ ); but *if* You were somehow to learn the value of  $p$ , then conditionally on that information You would regard all the tosses as being independent, with common probability  $p$  of landing H. (There are echoes here of the Principal Principle).

According to 1, under the weak assumption of exchangeability, the “individualist” Personalist can essentially accept the “groupist” Frequency story: more specifically, the Bernoulli model (with unknown probability  $p$ ). But the Personalist can go even further than the Frequentist: according to 2, exchangeability provides a warrant for equating the “individual risk,” on each single toss, with the overall “group probability”  $p$  (if only that were known...). That is, the Bernoulli model will be agreed upon by all personalists who agree on exchangeability (Dawid 1982a), and the limiting relative frequency  $p$ , across the repeated trials, constitutes a universal expert assignment for this class.

However, an important way in which this Personalist interpretation of the Bernoulli model differs from that of the Frequentist is in the conditions for its applicability. Exchangeability is justifiable when You have *no sufficient reason* to distinguish between the various trials. This is a much weaker requirement than the Frequentist’s “identity of all relevant characteristics.” For example, suppose You are considering the examination outcomes of a large number of students. You know there will be differences in ability between the students, so they can not be regarded as identical in all relevant respects. However, if You have no specific knowledge about the students that would enable You to distinguish the geniuses from the dunces, it could still be reasonable to treat these outcomes as exchangeable.

An important caveat is that Your judgment of exchangeability is, explicitly or implicitly, conditioned on Your current state of information, and can be destroyed if Your information changes. In the above example of the students, since You are starting with an exchangeable distribution, You believe there will be an overall limiting relative frequency  $p$  of failure across all the students, and that if You were to learn  $p$  that would be Your correct revised probability that a particular student, Karl, will fail the exam. Suppose, however, You were then to learn something about different students’ abilities. This would not affect Your belief (with probability 1) in the existence of the group limiting relative frequency  $p$ —but



You would no longer be able to treat that  $p$  as the individual probability of failure for Karl (who, You now know, is particularly bright).

As an illustration of the above, suppose You are considering the performances of the students (whom You initially consider exchangeable) across a large number of examination papers. Also, while You believe that some examinations are easier than others, You have no specific knowledge as to which those might be, so initially regard the examinations as exchangeable. Now consider the full collection of all outcomes, labelled by student and examination. As a Personalist, You will have a joint distribution for all of these. Moreover, because of Your exchangeability judgments, that joint distribution would be unchanged if You were to shuffle the names of the students, or of the examinations. Suppose now You are interested in “the risk” that Karl will fail the Statistics examination. You might confine attention to Karl, and use the limiting relative frequency of his failures, across all his other examinations, as his “risk” of failing Statistics. This appears reasonable since You are regarding Karl’s performances across all examinations (including Statistics) as exchangeable. In particular, this limiting relative frequency of Karl’s failure, across all examinations, constitutes an expert assignment for the event of his failing Statistics. Alternatively, You could concentrate on the Statistics examination, and take the limiting relative frequency of failure on that examination, by all the other students, as measuring Karl’s risk of failing. This too seems justifiable—and supplies an expert assignment—because You consider the performances of all the students (including Karl) on the Statistics examination as exchangeable. However, these two “risks” will typically differ—because they are conditioned on different information. Indeed, neither can take account of the full information You might have, about the performances of all students on all examinations. Given that full information, You should be able to assess both Karl’s ability (by comparing his average performance with those of the other students) and the difficulty of the Statistics examination (on comparing the average performance in Statistics with those for other examination). Somehow or other You need to use all this information (as well as the performances of other students on other examinations) to come up with Your “true” risk that Karl will fail Statistics—but it is far from obvious how You should go about this.<sup>3</sup>

We can elaborate such examples still further. Thus suppose students are allowed unlimited repeat attempts at each examination, and that (for each student-examination combination) we can regard the results on repeated attempts as exchangeable. Then yet another interpretation of Karl’s risk of failing Statistics on some given attempt—yet another universal expert assignment—would be the limiting relative frequency of failure across all Karl’s repeat attempts at the Statistics examination. This would typically differ from all the values discussed above.

Now it can be shown that, for this last interpretation of risk, its value would be unaffected by further taking into account all the rest of the data on other students’ performances on other examinations: it is truly conditional on all that is (or could be) known about the

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<sup>3</sup>There is in principle a way to do this (Aldous 1981; Hoover 1982), supplying a universal expert assignment for the class of all personalists agreeing on exchangeability both across students and across examinations—but it relies on advanced mathematics and is highly non-trivial to implement.

various students' performances. Does this mean that we have finally identified the "true" risk of Karl failing Statistics?

Not so fast. . . Suppose You consider that Karl's confidence, and hence his performance, on future attempts will be affected by his previous results, his risk of future failure going up whenever he fails, and down whenever he passes. It is not difficult to make this behaviour consistent with the exchangeability properties already assumed (Hill *et al.* 1987). As a simple model, consider an urn that initially contains 1 red ball (representing success) and 1 green ball (representing failure). Karl's successive performances are described by the sequence of draws of balls from this urn, made as follows: whichever ball is drawn is immediately replaced, together with an additional ball of the same colour. At each stage it is assumed the draws are made "at random," with each ball currently in the urn being "equally likely" to be the next to be drawn (this being a reasonably straightforward application of the classical interpretation). Thus Karl's future performance is influenced by his past successes and failures, with each success [failure] increasing [decreasing] the (classical) chance of success at the next attempt.

Now it can be shown that the sequence of colours drawn forms an exchangeable process, and it follows that the relative frequency with which a red ball is drawn, over a long sequence of such draws, will converge to some limit  $p$ . However, at the start of the process  $p$  is not known, but is distributed uniformly over the unit interval. How would you assess the probability that the first draw will result in a red ball? Useless to speculate about the currently unknown value of the limiting relative frequency  $p$ ; the sensible answer is surely the "classical" value  $1/2$ .

In this case, while You still believe that there will exist a limiting relative frequency of failure across all Karl's attempts at Statistics (and this constitutes a universal expert assignment), not only is this initially unknown to You, but its very value can be regarded as being constructed over time, as Karl experiences successes and failures and his level of performance gets better and worse accordingly. So why should You regard the limiting relative frequency of future failures, dependent as this is on Karl's randomly varying performance in his future attempts, as an appropriate measure of his risk of failing on this, his first attempt? As commented by Cane (1977) in a parallel context: "...if several clones were grown, each under the same conditions, an observer...might feel that the various values [e.g., of a *limiting proportion*—APD] they showed needed explanation, although these values could in fact be attributed to chance events." This point is relevant to the assessment of risk in the context of an individual's criminal career (*cf.* Example 2), where the very act of committing a new offence might be thought to raise the likelihood of still further offences. "An increase in criminal history increases the likelihood of recidivism, and a lack of increase can reduce that likelihood. Because criminal history can increase (or not) over time and each crime's predictive shelf life may be limited, it seems important to conceptualize criminal history as a variable marker" (Monahan and Skeem 2014).

It is interesting to view this ambiguity as to what should be taken as the "real" risk of success—the "classical" proportion of red balls currently in the urn, or the "frequentist" proportion of red balls drawn over the whole sequence—from a Personalist standpoint. Both constitute expert assignments for You, but they differ. The former is more concrete,

in that You can actually observe (or compute) it at any stage, which You can not do for the latter.

Suppose You are forced to bet on the colour of the first ball to be drawn. From the classical view, the probability value to use is 0.5 (and that classical value is fully known to You). Alternatively, taking a frequency view, You would consider the unknown limiting proportion  $p$ , with its uniform distribution over the unit interval. For immediate betting purposes, the relevant aspect of this is its expectation—which is again 0.5. Thus the personalist does not have to choose between the two different ways of construing “the probability” of success. (This is a special case of the result mentioned in §5.1 above). And this indifference extends to each stage of the process: if there are currently  $r$  red and  $b$  black balls in the urn, Your betting probability for next drawing a red, based on Your current expectation (given Your knowledge of  $r$  and  $b$ , which are determined by the results of previous draws) of the unknown limiting “frequency probability”  $p$ , will be  $r/(r+b)$ —again agreeing with the known “classical” value.

But what is the relevance of the above “balls in urn” model to the case of Karl’s repeat attempts? Even though the two stories may be mathematically equivalent, there is no real analogue, for Karl, of the “classical” probability based on counting the balls in the urn: Your probability  $r/(r+b)$  of Karl’s failing on his first attempt is merely a feature of Your Personalist view of the world, with limited relevance for any one else. So—what is “the risk” in this situation?

## 7 Individual Risk

Stories of coin tosses and such are untypical of real-world applications of risk and probability. So now we turn to a more realistic example:

**Example 8** What is “the risk” that Sam will die in the next 12 months? □

You might have good reason to be interested in this risk: perhaps You are Sam’s life assurance company, or You Yourself are Sam. There is plenty of mortality data around; but Sam is an individual, with many characteristics that, in sum (and in Sam), are unique to him. This makes it problematic to apply either of the G2i arguments above. As a Frequentist, You would need to be able to regard Sam and all the other individuals in the mortality data-files as “identical in all relevant characteristics,” which seems a tall order; while the Personalist You would need to be able to regard Sam and all those other individuals as exchangeable—but will typically know too much about Sam for that condition to be appropriate.

How then could You tune Your risk of Sam’s death to the ambient data? A common way of proceeding is to select a limited set of background variables to measure on all individuals, Sam included. For example, we might classify individuals by means of their age, sex, smoking behaviour, fruit and alcohol intake, and physical activity. We could then restrict attention to the subset of individuals, in the data, that match Sam’s values for these variables, and regard the (ideal limiting) relative frequency of death within 12 months in

that subset<sup>4</sup> as a measure of Sam’s own risk.<sup>5</sup> To the extent that You can regard Sam as exchangeable with all the other individuals sharing his values for the selected characteristics, the limiting relative frequency in that group constitutes an expert assignment for Sam. There can be no dispute that, from a pragmatic standpoint, such information about frequencies in a group of people “like Sam” (or “like Angelina”) can be extremely helpful and informative. In Imrey and Dawid (2014) such a group frequency, regarded as relevant to an individual member of the group, is termed an “individualized risk.” The foundational philosophical question, however, is: Can we consider this as supplying a measure of “individual risk”? For the Frequentist, that would require a belief that the chosen attributes capture “all relevant characteristics” of the individuals; for the Personalist, it would require that You have no relevant additional information about Sam (or any of the other individuals in the data), and can properly assume exchangeability—conditional on the limited information that is being taken into account. Neither of these requirements is fully realistic.

In any case, irrespective of philosophical considerations, there are two obvious difficulties with the above “individualization” approach:

- The “risk” so computed will depend on the choice of background variables.
- We may be obliged to ignore potentially relevant information that we have about Sam.

These difficulties are often branded “the problem of the reference class,” and would seem to bedevil any attempt to construct an unassailable definition of “individual risk” from a groupist perspective.

## 7.1 “Deep” risk

The above difficulties might disappear if it were the case that, as we added more and more background information, the frequency value in the matching subpopulation settled down to a limit: what we might term the “deep” risk, conditional on *all* there is to know about Sam. However, it is not at all clear why such limiting stability should be the case, nor is there much empirical evidence in favour of such a hypothesis. Indeed, it would be difficult to gather such evidence, since, as we increase the level of detail in our background information, so the set of individuals who match Sam on that information will dwindle, eventually leaving just Sam himself.

Even for the case of tossing a coin, it could be argued that if we know “too much” about the circumstances of its tossing, that would lead to a very different assessment of the probability of H; perhaps even, with sufficiently microscopic information about the initial positions and momenta of the molecules of the thumb, the coin, the table and the

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<sup>4</sup>Or, we could set up a statistical model for the form of the dependence of this risk on the given attributes, estimate its parameters from the ambient data, and apply it to Sam.

<sup>5</sup>An animation for exploring the dependence of this frequency on these characteristics (as well as calendar year) can be found at the website [understandinguncertainty.org](http://understandinguncertainty.org).

air, the outcome of the toss would become perfectly predictable, and the “deep” probability would reduce to 0 or 1 (or perhaps not, if we take quantum phenomena into account...).

I would personally be sympathetic to the view that this *reductio ad absurdum* is a “category error,” since no one really intended the “deep” information to be quite *that* deep. Perhaps it is the case that the addition of more and more “appropriate” background information, at a more superficial level, would indeed lead to a stabilisation of the probability of H at some non-trivial value. But this account is full of vagueness and ambiguities, and begs many questions. And even if we could resolve it for the extremely untypical case of coin-tossing, that would not give us a licence to assume the existence of “deep risk” for more typical practical examples, such as Sam’s dying in the next 12 months.

## 8 A Different Approach: Individual to Group Inference

All our discussion so far—even for the “individualist” Personalist conception of Probability—has centred on “Group to Individual” (G2i) inference: taking group frequencies as our fundamental starting point, and asking how these might be used to determine individual risks. And it has to be said that we have reached no conclusive answer to this question. So instead we now turn things upside down, and ask: Suppose we take individual risks as fundamental—how then could we relate these to group frequencies, and with what consequences? This is the “Individual to Group” (i2G) approach. As we shall see, although we will not now be defining individual risks in terms of group frequencies, those frequencies will nevertheless severely constrain what we can take the individual risks to be.

Our basic framework is again an ordered population of individuals ( $I_1, I_2, \dots$ ). For individual  $I_k$ , You have an outcome event of interest,  $E_k$ , and some background information,  $H_k$ . We denote Your “information base”—Your full set of background information ( $H_1, H_2, \dots$ ) on all individuals—by  $\mathcal{H}$ .

Note, importantly, that in this approach we need not assume that You have similar information for different individuals, nor even that the different outcome events are of the same type (though in applications that will typically be the case). In particular, we shall not impose any analogue of either the Frequentist’s condition of “repeated trials under identical conditions,” or the Personalist’s judgment of exchangeability.

We start with an initial bold assumption: that You have been able to assess, for each individual  $I_k$ , a probability,  $p_k = \Pr(E_k \mid H_k)$ , for the associated event  $E_k$ , in the light of the associated background information  $H_k$ . This might, but need not be, interpreted as a Personalist betting probability: all that we want is that it be some sort of “individualist” assessment of uncertainty. We put no other constraint on these “individual risks”—most important, they will typically vary from one individual to another. We term  $p_k$  a *probability forecast* for  $E_k$ . For some purposes we will require that the forecasts are based *only* on the information in  $\mathcal{H}$ , and nothing else. There are some subtleties involved in making this intuitively meaningful condition mathematically precise—one approach is through the

theory of computability (Dawid 1985a). Here we will be content to note that this can be done. We shall term such forecasts *H*-based.

There are two similar but slightly different scenarios that we can analyse, with essentially identical results. In the first, which we may term the *independence* scenario, the background information  $H_k$  pertains solely to individual  $I_k$ , and it is supposed that, given  $H_k$ , Your uncertainty about  $E_k$ , as expressed by  $p_k$ , would not change if You were to receive any further information (be it background information or outcome information) on any of the other individuals. In the second, *sequential* scenario,  $H_k$  represents the total background information on all previous and current individuals  $I_1, \dots, I_k$ , as well as the outcomes of  $E_1, \dots, E_{k-1}$  for the previous individuals; no other conditions need be imposed.

An important practical application, in the sequential formulation, is to weather forecasting, where  $I_k$  denotes day  $k$ ,  $E_k$  denotes “rain on day  $k$ ,”  $H_k$  denotes the (possibly very detailed) information You have about the weather (including whether or not it rained) up to and including the previous day  $k - 1$ , and You have to go on TV at 6pm each evening and announce Your probability  $p_k$  that it will rain the following day  $I_k$ . We will often use this particular example to clarify general concepts.

Our proposed relationship between individual probabilities and group frequencies will be based on the following idea: Although You are free to announce any probability values You want for rain tomorrow, if You are to be trusted as a reliable weather forecaster, these values should bear some relationship to whether or not it does actually rain on the days for which You have issued forecasts. Note that this approach judges Your probabilities by comparison with the outcomes of the events—not with the “true probabilities” of the events. No commitment as to the existence of “true probabilities” is called for.

## 8.1 Calibration

Let  $e_k$  denote the actual outcome of event  $E_k$ , coded 1 if  $E_k$  happens, and 0 if it does not.

### 8.1.1 Overall calibration

We start by proposing the following *overall calibration criterion* of agreement between the probability forecasts ( $p_k$ ) and the outcomes ( $e_k$ ): over a long initial sequence  $I_1, I_2, \dots, I_N$ , the overall proportion of the associated events that occur,

$$\frac{\sum_{i=1}^N e_i}{N}, \quad (1)$$

which is a “groupist” property, should be close to the average of the “individualist” forecast probabilities,

$$\frac{\sum_{i=1}^N p_i}{N}. \quad (2)$$

This seems a plausible requirement, but do we have a good warrant for imposing it? Yes. It can be shown (Dawid 1982b) that (for either of the scenarios) the overall calibration property is assigned probability 1 by Your underlying probability distribution. That is to

say, You firmly believe that Your probability forecasts will display overall calibration. So, if overall calibration turns out *not* to be satisfied, an event that You were convinced was going to happen has failed to occur—a serious anomaly, that discredits Your whole distribution, and with it Your probability forecasts (Dawid 2004). Overall calibration thus acts as a minimal “sanity check” on your probability forecasts: if it fails, You are clearly doing something wrong.

However, that does not mean that, if it holds, You are doing everything right: overall calibration is a weak requirement. For example, in an environment where it rains 50% of the time, a weather forecaster who, ignoring all information about the past weather, always announces a probability of 50%, will satisfy overall calibration—but if in fact it rains every alternate day (and much more generally) he will be showing little genuine ability to forecast the changing weather on a day-by-day basis. Another forecaster, who has a crystal ball, always gives probability 1 or 0 to rain, and always gets it right. Her perfect forecasts also satisfy overall calibration.

In order to make finer distinctions between forecasts of such very different quality, we will progressively strengthen the calibration criterion, through a number of stages. Note, importantly, that every such strengthened variant will share the “sanity check” function described above for overall calibration: according to Your probability distribution, it will be satisfied with probability 1. So, if it fails, Your probability forecasts are discredited.

### 8.1.2 Probability calibration

For our next step, instead of taking the averages in (1) and (2) over all days until day  $N$ , we focus on just those days  $I_k$  for which the forecast probability  $p_k$  was equal to (or very close to) some pre-assigned value. If that value is, say, 30% (and that value is eventually used infinitely often) then *probability calibration* requires that, in the limit, the proportion of these days on which it in fact rains should be (close to) 30%; and similarly for any other pre-assigned value.

However, although probability calibration is again a very natural idea, it is still too weak for our purpose. In particular, it will still be satisfied for both our above examples of uninformative forecasts and of perfect forecasts.

### 8.1.3 Subset calibration

For our next attempt, we allow the averages in (1) and (2) to be restricted to a subset of the individuals, arbitrarily chosen except for the requirement that it must be selected without taking any account of the values of the  $E$  and the  $H$ 's. For example, we might choose every second day. If in fact the weather alternates wet, dry, wet, dry, . . . , then the uninformative forecaster, who always says 50%, will now fail on this criterion, since if we restrict to the odd days alone his average forecast probability is still 50%, but the proportion of rainy days will now be 100%. The perfect forecaster will however be announcing a probability forecast of 100% for every odd day, and so will satisfy this criterion.

However, although subset calibration has succeeded in making the desired distinction, even this is not strong enough for our purposes.

#### 8.1.4 Information-based calibration

In all our attempts so far, we have not made any essential use of the background information base  $\mathcal{H}$ , and the requirement that the forecasts be  $\mathcal{H}$ -based. But we cannot properly check whether a forecaster is making appropriate use of this background information without ourselves taking account of it.

In the sequential weather forecasting scenario, the forecaster is supposed to be taking account of (at least) whether or not it rained on previous days, and to be responding appropriately to any pattern that may be present in those outcomes. To test this, we could form a test subset in a dynamic way, ourselves taking account of all the forecaster’s background information (but, to be totally fair to the forecaster, nothing else<sup>6</sup>). Thus we might consider, for example, the subset comprising just those Tuesdays when it had rained on both previous days. If the forecaster is doing a proper job, he should be calibrated (*i.e.*, his average probability forecast should agree with the actual proportion of rainy days), even if we restrict the averages to be over such an “ $\mathcal{H}$ -based” subset. An essentially identical definition applies in the independence scenario.

When this property is satisfied for all  $\mathcal{H}$ -based subsets, we will call the probability forecasts  $\mathcal{H}$  *calibrated*. A set of forecasts that is both  $\mathcal{H}$ -based and  $\mathcal{H}$ -calibrated will be called  $\mathcal{H}$ -*valid*. Again we stress that You assign probability 1 to Your  $\mathcal{H}$ -based forecasts being  $\mathcal{H}$ -valid, so this is an appropriate condition to impose on them.

Finally, we have a strong criterion relating individual probability forecasts and frequencies. Indeed, it constrains the individuals forecasts so much that, in the limit at least, their values are fully determined. Thus suppose that we have two forecasters, who issue respective forecasts  $(p_k)$  and  $(q_k)$ , and that both sets are  $\mathcal{H}$ -valid. It can then be shown (Dawid 1982b) that, as  $k$  increases without limit, the difference between  $p_k$  and  $q_k$  must approach 0. We may term this result *asymptotic identification*.

This is a remarkable result. We have supposed that “individual risks” are given, but have constrained these only through the “groupist”  $\mathcal{H}$ -calibration criterion. But we see that this results “almost” in full identification of the individual values, in the sense that, if two different sets of probability forecasts both satisfy this criterion, then they must be essentially identical. In particular, if there exists any set of  $\mathcal{H}$ -valid forecasts,<sup>7</sup> then those values are the “essentially correct” ones: any other set of  $\mathcal{H}$ -based forecasts that does not agree, asymptotically, with those values can not be  $\mathcal{H}$ -valid—and is thus discredited. So in this sense the i2G approach has succeeded—where the more traditional G2i approaches failed—in determining the values of individual risks on the basis of group frequencies.

As an almost too simple example of this result, in the independence scenario, suppose that  $H_k$ , the background information for individual  $I_k$ , is his score on the Violence Risk Appraisal Guide ARAI (Quinsey *et al.* 2006), with 9 categories. If this limited information

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<sup>6</sup>Again, this requirement can be formalised using computability theory.

<sup>7</sup>which is however not guaranteed (Schervish 1985; Belot 2013)



$\mathcal{H}$  is all that is available to the forecaster, his forecasts will be  $\mathcal{H}$ -based just when they announce the identical probability value for all individuals in the same VRAG category. And they will be  $\mathcal{H}$ -calibrated if and only if, within each of the 9 categories, his announced probability agrees with the actual reoffending rates. That is,  $\mathcal{H}$ -validity here reduces to the identification of *individual* risk with *individualized* risk; and this identification is thus justified so long as Your complete information is indeed restricted to that comprised by  $\mathcal{H}$ .

What this simple example fails to exhibit is that, in more general cases, the “determination” of the individual forecasts is only asymptotic: given a set of  $\mathcal{H}$ -valid forecasts, we could typically change their values for a finite number of individuals, without affecting  $\mathcal{H}$ -validity. So we are not, after all, able to associate a definitive risk value with any single individual.

Another big downside of the above result is that, while assuring us of the essential uniqueness of  $\mathcal{H}$ -valid forecasts, it is entirely non-constructive. It seems reasonable to suppose that, once we know that such uniquely determined forecasts exist, there should be a way to construct them: for example, it would be nice to have an algorithm that would issue probability forecasts for the following day, based on past weather ( $\mathcal{H}$ ), that will be properly calibrated, however the future weather in fact turns out. But alas! in general this is not possible, and there is no  $\mathcal{H}$ -based system that can be guaranteed to be  $\mathcal{H}$ -calibrated (Oakes 1985; Dawid 1985b).

Another potentially serious limitation of the i2G approach is its dependence on the population of individuals considered, and moreover on the order in which they are strung out as a single sequence. It does not seem easy to accommodate a structure, such as considered in §6.2, where many students take many resits of many examinations. For one thing, it seems problematic to incorporate the judgements of exchangeability made there into the i2G approach; for another, different ways of forming a sequence of student-examination-resit combinations could well lead to mutually inconsistent calibration requirements.

## 8.2 Varying the information base

A fundamental aspect of the i2G approach to individual risk is that this is a relative, not an absolute, concept: its definition and interpretation depend explicitly on the information that is being taken into account,<sup>8</sup> as embodied in  $\mathcal{H}$ . If we change the information base, the associated valid probability values will change.

We can relate the i2G risks based on different information bases, one more complete than the other. Thus suppose that  $\mathcal{K} = (K_k)$  is an information base that is more detailed than  $\mathcal{H}$ : perhaps  $H_k$  only gives information about whether or not it rained on days prior to day  $k$ , while  $K_k$  also contains information about past maximum temperature, wind speed, *etc.* Suppose  $(p_k)$  is a set of  $\mathcal{H}$ -valid forecasts, and  $(q_k)$  a set of  $\mathcal{K}$ -valid forecasts. These would typically differ, even asymptotically: We would expect the  $(q_k)$ , being based on more information, to be “better” than the  $(p_k)$ , and so would *not* expect  $p_k - q_k \rightarrow 0$ . This does not contradict asymptotic identification. The  $(q_k)$  are not  $\mathcal{H}$ -based, so not  $\mathcal{H}$ -valid.

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<sup>8</sup>In this it has some of the flavour of Logical Probability

Also, while the  $(p_k)$  are  $\mathcal{K}$ -based, they have not been required to be  $\mathcal{K}$ -calibrated, so they are not  $\mathcal{K}$ -valid. So, whether we take the underlying information base to be  $\mathcal{H}$  or  $\mathcal{K}$ , the conditions implying asymptotic identification simply do not apply.

It does however follow from the argument for asymptotic identification that, if we consider the subsequence for which, say,  $p_k = 0.4$  (to a good enough approximation), then—while we can expect the  $(q_k)$  in that subsequence to vary (because of variations in the additional information in  $\mathcal{K}$  that they take into account)—their limiting average value in the subsequence will be 0.4 (and similarly for any other target value for the  $(p_k)$ ). In this sense the “deeper” risks  $(q_k)$  can be regarded as varying “randomly” about the “shallower” risks  $(p_k)$ —just as the actual outcomes  $(E_k)$  do. Some of the attempts to model individual risks as random, such that of Robins and Greenland (1989), might be understood as contemplating an expanded but unobservable information base  $\mathcal{K}$ , and interpreting the “true risks” as the  $\mathcal{K}$ -valid ones. However, with access only to the shallower information base  $\mathcal{H}$ , we can only “observe” certain crude averages of these “true risks.” If  $\mathcal{H}$  is indeed all the information at our disposal, there is nothing of value to be gained by extending consideration to the unobservable deeper risks  $(q_k)$ —which in any case depend on the essentially arbitrary specification of the unobserved deeper information base  $\mathcal{K}$ . The flip-side of this is that, when we do have access to the deeper information, we should use that to identify the relevant “individual risk,” rather than be satisfied with a cruder average value based on more superficial information.

An interesting implication of the above argument is that our “individual-to-group” approach allows for the assignment of non-extreme probabilities to events, even when we believe in “deep determinism”—that is, we believe that, given a suitably detailed information base  $\mathcal{K}$ , it would be possible to forecast the future perfectly (in which case  $\mathcal{K}$ -valid probabilities  $(q_k)$  would have to be, asymptotically, 0 or 1). However, if we only have access to a less detailed information base  $\mathcal{H}$ , the associated  $\mathcal{H}$ -valid risk values would normally be non-extreme. This approach thus justifies the use of probability as a description of a system we believe to operate deterministically, so long as we are in ignorance of the deep determining circumstances.

## 9 Discussion

I have surveyed a number of conceptions of the meaning of “individual risk,” and found all of them wanting to some degree. The various G2i approaches all founder on the “problem of the reference class,” which does not have an unambiguous solution—although reasonable pragmatic choices can often be made, and defended as such. The i2G approach avoids ambiguity and delivers individual risk values, nicely calibrated to the information considered available—but only “at infinity,” and even then these asymptotic values are uncomputable. It also does not seem able to take account of sophisticated exchangeability requirements.

What then is one to do? Should the whole idea of individual risk be abandoned? I think this is too extreme, but certainly the concept and nomenclature should not be bandied about carelessly, and it behoves any one using the term to give an account of what

they (think they) mean by it.

For what it is worth, my own tentative attitude is as follows, based on an essentially personalistic viewpoint. Suppose I am tasked with assessing “the risk” that Cain, a prisoner up for possible parole, will if released commit a violent act (say, within the next 24 months). Then I should think about Cain, and, taking full account of all I know about him and others like him (and even unlike him), compute my probability forecast by assessing the odds at which I would be willing to bet that he will commit a violent act. This probability can in principle be considered as formed, by conditioning on the information I have on Cain and others, from my full personal joint probability distribution for the properties and outcomes of all these individuals. Under certain strong conditions on that joint distribution, such as exchangeability, my forecast probability would be close to the proportion of such events in people “like Cain”; but more generally, that proportion, and other relevant proportions, would inform, but not directly constrain, my personal, properly conditioned, individual probability forecast for Cain.

But, you object, what of objectivity? You want to know “the risk” that Cain will be violent. Why should you care about my personal probability assessment?

Well, in the large my probability forecasts can be tested against realised outcomes (Dawid 1986), *e.g.*, using finite-data analogues of calibration (Seillier-Moiseiwitsch and Dawid 1993) to see how closely they align with actual frequencies in appropriately selected sets.<sup>9</sup> If, by such tests (formal or informal), my announced probability forecasts are shown to be out of line with reality, you have every right to discount my risk assessment for Cain. But if they have passed a suitable battery of such tests—are, provisionally, “valid”—then (arguing informally by analogy with our asymptotic identification result) we might expect them to be close to the valid risk assessments of others. So, in the presence of a sufficient quantity of relevant data to allow us to conduct such statistical tests, you should be able to judge whether my risk assessments are reasonably “objective,” and if so have some limited confidence in my—now statistically justified—announced risk for Cain.

But recall that “valid” probability forecasts depend on the information that is being taken into account. Ideally this should be the most detailed information we have about Cain and those like him; but, the more detailed the information is, the harder it will be for me to make my forecasts valid.<sup>10</sup> So I might choose to artificially restrict my information base, perhaps to just a few simple characteristics. I would thus be sacrificing incisiveness—the possibility (however remote) of making “deep” probability forecasts, valid with respect to the deep information that I could, in principle, take into account—in favour of robustness—the enhanced prospect of achieving validity with respect to deliberately restricted information. In the judicial context, and many others, this sacrifice might well

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<sup>9</sup>One criterion I should definitely *not* be judged on is how good my probability forecasts are as estimates of the “objective” individual risks. This is for two reasons: first, in the unavoidable absence of knowledge of the values of those risks, this can not be done; and, secondly, there are no such things!

<sup>10</sup>Individual risk assessment based on very detailed personal information becomes essentially a matter of “clinical judgment.” Although practitioners in many disciplines—medicine, law, psychiatry, *etc. etc.*—often have great confidence in their own clinical judgments, these can be very far from being valid (Meehl 1954).

be considered worthwhile: indeed, a suitable specification of what would be “appropriate” characteristics to take into account could be enshrined in statute.<sup>11</sup> That done, suitable statistical methodology applied to these could produce valid risk assessments—albeit relating to a shallower level of information than would ideally be desirable. The various ARAI systems that have been developed can be thought of as addressing this task.

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## References

- Aharoni, E., Mallett, J., Vincent, G. M., Harenski, C. L., Calhoun, V. D., Sinnott-Armstrong, W., and Kiehl, M. S. G. . K. A. (2014). Predictive accuracy in the neuro-prediction of rearrest. *Social Neuroscience*, **9**, 332–6.
- Aharoni, E., Vincent, G. M., Harenski, C. L., Calhoun, V. D., Sinnott-Armstrong, W., Gazzaniga, M. S., and Kiehl, K. A. (2013). Neuroprediction of future rearrest. *Proceedings of the National Academy of Sciences*, **110**, 6223–8.
- Aldous, D. J. (1981). Representations for partially exchangeable arrays of random variables. *Journal of Multivariate Analysis*, **11**, 581–98.
- Belot, G. (2013). Failure of calibration is typical. *Statistics and Probability Letters*, **83**, 2316–8.
- Cane, V. R. (1977). A class of non-identifiable stochastic models. *Journal of Applied Probability*, **14**, 475–82.
- Carnap, R. (1950). *Logical Foundations of Probability*. University of Chicago Press, Chicago, Illinois.
- Cooke, D. J. and Michie, C. (2010). Limitations of diagnostic precision and predictive utility in the individual case: A challenge for forensic practice. *Law and Human Behavior*, **34**, 269–74.
- Dawid, A. P. (1982a). Intersubjective statistical models. In *Exchangeability in Probability and Statistics*, (ed. G. Koch and F. Spizzichino), pp. 217–32. North-Holland Publishing Company, Amsterdam.
- Dawid, A. P. (1982b). The well-calibrated Bayesian (with Discussion). *Journal of the American Statistical Association*, **77**, 604–13. Reprinted in Hamouda and Rowley (1997), pp. 165–173.

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<sup>11</sup>This leaves the possibility that an individual might appeal on the grounds that relevant information, specific to his case, was left out of consideration. But it would be for the appellant to make a good case for this.

- Dawid, A. P. (1985a). Calibration-based empirical probability (with Discussion). *Annals of Statistics*, **13**, 1251–85. Reprinted in Hamouda and Rowley (1997), pp. 174–208.
- Dawid, A. P. (1985b). The impossibility of inductive inference. (Invited discussion of ‘Self-calibrating priors do not exist’, by D. Oakes). *Journal of the American Statistical Association*, **80**, 340–1.
- Dawid, A. P. (1986). Probability forecasting. In *Encyclopedia of Statistical Sciences*, (ed. S. Kotz, N. L. Johnson, and C. B. Read), pp. 210–8. Wiley-Interscience.
- Dawid, A. P. (1998). Modelling issues in forensic inference. In *1997 ASA Proceedings, Section on Bayesian Statistics*, pp. 182–6.
- Dawid, A. P. (2004). Probability, causality and the empirical world: A Bayes-de Finetti-Popper-Borel synthesis. *Statistical Science*, **19**, 44–57.
- Dawid, A. P., DeGroot, M. H., and Mortera, J. (1995). Coherent combination of experts’ opinions (with Disussion). *Test*, **4**, 263–313.
- de Finetti, B. (1937). Foresight: Its logical laws, its subjective sources. *Annales de l’Institut Henri Poincaré. Probabilités et Statistiques*, **7**, 1–68. Translated by H. E. Kyburg in Kyburg and Smokler (1964), 55–118.
- Foreman, L. A., Smith, A. F. M., and Evett, I. W. (1997). Bayesian analysis of deoxyribonucleic acid profiling data in forensic identification applications (with Discussion). *Journal of the Royal Statistical Society, Series A*, **160**, 429–69.
- Fox, C. R. and Birke, R. (2002). Forecasting trial outcomes: Lawyers assign higher probability to possibilities that are described in greater detail. *Law and Human Behavior*, **26**, 159–73.
- Gaifman, H. (1988). A theory of higher order probabilities. In *Causation, Chance, and Credence*, (ed. B. Skyrms and W. L. Harper), pp. 191–219. Kluwer Academic Publishers, Dordrecht.
- Galavotti, M. C. (2014). Probability theories and organization science: The nature and usefulness of different ways of treating uncertainty. *Journal of Management*. Published online 8 May 2014. DOI:10.1177/0149206314532951.
- Gigerenzer, G., Hertwig, R., Van Den Broek, E., Fasolo, B., and Katsikopoulos, K. V. (2005). “A 30% chance of rain tomorrow”: How does the public understand probabilistic weather forecasts? *Risk Analysis*, **25**, (3), 623–9.
- Gillies, D. (2000). Varieties of propensity. *British Journal for the Philosophy of Science*, **51**, 807–835.
- Hájek, A. (2012). Interpretations of probability. In *The Stanford Encyclopedia of Philosophy*, (ed. E. N. Zalta).  
<http://plato.stanford.edu/archives/win2012/entries/probability-interpret/>.
- Hamouda, O. F. and Rowley, J. C. R. (ed.) (1997). *Probability Concepts, Dialogue and Beliefs*. Edward Elgar Publishing Ltd.
- Hanson, K. R. and Howard, P. D. (2010). Individual confidence intervals do not inform

- decision-makers about the accuracy of risk assessment evaluations. *Law and Human Behavior*, **34**, 275–81.
- Harris, G. T., Rice, M. E., and Quinsey, V. L. (2008). Shall evidence-based risk assessment be abandoned? *British Journal of Psychiatry*, **192**, 154.
- Hart, S. D. and Cooke, D. J. (2013). Another look at the (im-)precision of individual risk estimates made using actuarial risk assessment instruments. *Behavioral Sciences and the Law*, **31**, 81–102.
- Hart, S. D., Michie, C., and Cooke, D. J. (2007). Precision of actuarial risk assessment instruments. Evaluating the ‘margins of error’ of group v. individual predictions of violence. *British Journal of Psychiatry*, **190**, suppl. **49**, s60–5.
- Hill, B. M., Lane, D., and Sudderth, W. (1987). Exchangeable urn processes. *The Annals of Probability*, **15**, 1586–92.
- Hoover, D. N. (1982). Row-column exchangeability and a generalized model for exchangeability. In *Exchangeability in Probability and Statistics*, (ed. G. Koch and F. Spizzichino), pp. 281–91. North-Holland, Amsterdam.
- Imrey, P. B. and Dawid, A. P. (2014). A commentary on statistical assessment of violence recidivism risk. Submitted to *Statistics and Public Policy*.
- Jeffreys, H. (1939). *Theory of Probability*. Oxford University Press, Oxford, United Kingdom.
- Kyburg, H. E. and Smokler, H. E. (ed.) (1964). *Studies in Subjective Probability*. John Wiley and Sons, New York.
- Lewis, D. (1980). A subjectivist’s guide to objective chance. In *Studies in Inductive Logic and Probability*, (ed. R. Carnap and R. C. Jeffrey), pp. 263–293. University of California Press, Berkeley, CA.
- Meehl, P. E. (1954). *Clinical vs. Statistical Prediction: A Theoretical Analysis and a Review of the Evidence*. University of Minnesota Press, Minneapolis.
- Monahan, J. and Skeem, J. L. (2014). Risk redux: The resurgence of risk assessment in criminal sanctioning. *Federal Sentencing Reporter*, **26**, 158–66.
- Mossman, D. (2014). From group data to useful probabilities: The relevance of actuarial risk assessment in individual instances. Available at SSRN: <http://ssrn.com/abstract=2372101>.
- Oakes, D. (1985). Self-calibrating priors do not exist. *Journal of the American Statistical Association*, **80**, 339.
- Pettigrew, R. (2010). Accuracy, chance, and the Principal Principle. *Philosophical Review*, **121**, 241–75.
- Poldrack, R., Reyna, V., Raichle, M., Faigman, D., and Buckholz, J. (2014). Neuroprediction and the law. In preparation.
- Popper, K. R. (1959). The propensity interpretation of probability. *British Journal for the Philosophy of Science*, **10**, 25–42.

- Quinsey, V. L., Harris, G. T., Rice, M. E., and Cormier, C. A. (2006). *Violent Offenders: Appraising and Managing Risk*, (2nd edn). American Psychological Association, Washington, D.C.
- Robins, J. M. and Greenland, S. (1989). The probability of causation under a stochastic model for individual risk. *Biometrics*, **45**, 1125–38.
- Roeder, K., Escobar, M., Kadane, J. B., and Balazs, I. (1998). Measuring heterogeneity in forensic databases using hierarchical Bayes models. *Biometrika*, **85**, 269–87.
- Schervish, M. J. (1985). Discussion: Calibration-based empirical probability. *The Annals of Statistics*, **13**, 1274–82.
- Seillier-Moiseiwitsch, F. and Dawid, A. P. (1993). On testing the validity of sequential probability forecasts. *Journal of the American Statistical Association*, **88**, 355–9.
- Singh, J. P. and Petrila, J. (ed.) (2013). *Special Issue: Methodological Issues in Measuring and Interpreting the Predictive Validity of Violence Risk Assessments*, Behavioral Sciences and the Law, **31**, 1–164. Wiley.
- von Mises, R. (1939). *Probability, Statistics and Truth*. William Hodge, London. (German original J. Springer, 1928).