Frequentism-as-model

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Abstract: Most statisticians are aware that probability models interpreted in a frequentist manner are not really true in objective reality, but only idealisations. I argue that this is often ignored when actually applying frequentist methods and interpreting the results, and that keeping up the awareness for the essential difference between reality and models can lead to a more appropriate use and interpretation of frequentist models and methods, called frequentism-as-model. This is elaborated showing connections to existing work, appreciating the special role of i.i.d. models and subject matter knowledge, giving an account of how and under what conditions models that are not true can be useful, giving detailed interpretations of tests and confidence intervals, confronting their implicit compatibility logic with the inverse probability logic of Bayesian inference, re-interpreting the role of model assumptions, appreciating robustness, and the role of “interpretative equivalence” of models. Epistemic (often referred to as Bayesian) probability shares the issue that its models are only idealisations and not really true for modelling reasoning about uncertainty, meaning that it does not have an essential advantage over frequentism, as is often claimed. Bayesian statistics can be combined with frequentism-as-model, leading to what Gelman and Hennig (2017) call “falsificationist Bayes”.

Key Words: foundations of statistics, Bayesian statistics, interpretational equivalence, compatibility logic, inverse probability logic, misspecification testing, stability, robustness

1 Introduction

The frequentist interpretation of probability and frequentist inference such as hypothesis tests and confidence intervals have been strongly criticised recently (e.g., Hajek (2009); Diaconis and Skyrms (2018); Wasserstein et al. (2019)). In applied statistics they are still in very widespread use, and in theoretical statistics the number of still appearing new works based on frequentist principles is probably not lower than that of any competing approach to statistics.
1 INTRODUCTION

Even defenders of frequentist statistics such as [Mayo (2018)] admit that frequentist inference is often misinterpreted, while arguing that such misinterpretation is a major source of unfair criticism of frequentism. By and large I agree with this. Here I will present a view of frequentist probability and inference that, as I will argue, avoids many issues that critics have raised, and allows for a better understanding of how results of frequentist inference can be interpreted. I call this view “frequentism-as-model”. It is based on the idea (elaborated in [Hennig (2010)] and sketched in Section 3.1) that mathematical models are essentially different from “reality” and should be used and their results interpreted always keeping this difference in mind.

The insight that probability models are remote from reality was expressed by de Finetti (1974) (“probability does not exist”) and Box (1979) (“all models are wrong but some are useful”). Most statisticians would probably agree with the idea that probability models will not match how reality “really is”. But much communication and even thought about statistics is misleading in this respect, for example the idea that it is possible to check and ultimately fulfill statistical model assumptions (which implies that models can be true after all). Having de Finetti’s and Box’s quote in mind can be a useful reminder of the difference between models and reality, but I think they are themselves problematic. Box implies that the distinction between a true and a wrong model is a meaningful one, which I doubt, given that models operate on a different domain from observer-independent reality, see Section 3.1. De Finetti argued that, given that objective/frequentist probabilities do not exist, subjective (Bayesian) probability should be preferred, conveniently ignoring that using probability calculus to model personal uncertainty comes with the same modelling issue, namely that the model operates on a domain different from what is modelled. I will argue that this issue ultimately affects all probability modelling. Ultimately my position is pluralist and I do not claim general superiority of a frequentist over a Bayesian view, but I hold that there is a place and a use in science for frequentist probability models and inference, appropriately understood as frequentism-as-model.

Here are some influences and important ideas from the literature. Readers with a philosophical background may find similarities of my position with the “long run propensity” interpretation of probability by [Gillies (2000)] (Chapter 7). Gillies abandons the typically frequentist operational connection between probabilities and “real” infinite sequences, and just postulates that real propensities may behave like random numbers generated by probability models, which can be probabilistically falsified. Although his outlook is more traditionally realist than mine (see Section 3.1), I find this interpretation very much in line with how frequentist statisticians actually reason. [Hacking (1975)] suggested the term “aleatory” for probability interpretations such as frequentism and propensities, where probabilities refer to the data generation in the real world. [Tukey (1997)] re-interpreted the role of frequentist models in statistical inference as “challenges” that procedures have to face in order to show that they are worthwhile in a given situation. He recommended using “bouquets of alternative
challenges” to investigate how well a procedure may work in a real situation, including worst cases, rather than finding an optimal procedure for a single model. He wrote “(more honest foundations of data analysis) have to include not assuming that we always know what in fact we never know – the exact probability structure involved”, and further “almost nothing is harder than verifying a stochastic assumption, something I find hard to believe has ever been done in even a single instance.” Many different models can be compatible with the same situation, and it is often misleading to base an analysis on just one model.

Davies (2014)’s view of statistical models is similar: “The problem is how to relate a given data set and a given model. Models in statistics are, with some exceptions, ad hoc, although not arbitrary, and so should be treated consistently as approximations and not as truth. I believe there is a true but unknown amount of copper in a sample of water, but I do not believe that there is a true but unknown parameter value. Nor am I prepared to behave as if there were a true but unknown parameter value, here put in inverted commas as statisticians often do to demonstrate that they do not actually believe that there is such a thing.

Moreover the model has to be an approximation to the data and not to some true but unknown generating mechanism. There may well be some physical truth behind the data but this will be many orders of magnitude more complex than anything that can be reasonably modelled by a probability measure. Furthermore there will be a different truth behind every single observation. (…) The definition of approximation used in this book is that a model is an adequate approximation to a data set if typical data generated under the model look like the real data.” Again this implies that many different models can be adequate approximations of the same data. In line with Davies, in the following, the term “probability model” will denote a well defined family of distributions; sometimes this will refer to the special case of just a single distribution, i.e., both the set of Gaussian distributions \( \mathcal{N}(\mu, \sigma^2) : \mu \in \mathbb{R}, \sigma^2 \in \mathbb{R}^+ \), and a single distribution with fixed values for \( \mu \) and \( \sigma^2 \), will be called “model”.

A key issue in Davies’s concept is whether the real data to be analysed can be distinguished, in a sense to be defined by the choice of an appropriate statistic, from typical data sets generated by the model. I will refer to this as “compatibility logic”. It does not require any postulation of “truth” for the model. Although Davies stated that his approach is neither frequentist nor Bayesian, I interpret it as frequentist in the sense that models are taken as data generators that can in principle produce an arbitrarily large amount of data sets with resulting relative frequencies that roughly correspond to the modelled probabilities.

Whereas I mostly agree with Davies, my focus is more on interpretation and what we can learn from probabilistic modelling, and as opposed to Davies I can see an interpretation of Bayesian inverse probability logic that is in line with frequentism-as-model, see Section 5.2.

In Section 2 I define what I mean by frequentism-as-model. In Section 3 concerns the connection between frequentism-as-model and reality, first summarising my general view on mathematical models and reality, then explaining how sta-
tistical models can be useful dealing with reality without implying that they are true or even, in a well defined sense, close to being true (Davies’s “approximation” regards closeness to the observed data, not to any underlying truth). Section 4 treats statistical inference based on frequentism-as-model, including the role of model assumptions, robustness and stability, and the key concept of “interpretational equivalence”. In Section 5 I will explore Bayesian statistics in relation to frequentism-as-model. First I discuss the charges of proponents of epistemic interpretations of probability against frequentism, and to what extent frequentism-as-model can be useful in ways not covered by epistemic probabilities. Then I will explain how Bayesian inverse probability logic can be used in a way compatible with frequentism-as-model, in the spirit of what we have called “falsificationist Bayes” in Gelman and Hennig (2017). Compatibility logic (as inherent in, e.g., statistical tests) and Bayesian inverse probability logic will be compared in the light of frequentism-as-model. Section 6 has a concluding discussion.

2 What is frequentism-as-model?

In Section 2.1 I will explain frequentism-as-model. In order to understand its implications, the role of i.i.d. models and subject matter information is discussed in Section 2.2.

2.1 Frequentism-as-model as interpretation of probability

Frequentism-as-model is an interpretation of probability, by which I mean a way to connect the mathematical definition of probability to our concept and perception of reality.

To make distinctions clearer, “traditional” frequentism is an interpretation of probability as well, and this does in my view not include what is often seen as methods of frequentist inference such as tests and confidence intervals. These are normally motivated assuming a frequentist interpretation of probability, but holding a frequentist interpretation of probability does not make using these methods mandatory. An alternative to frequentism is epistemic probability, interpreted as expressing the level of uncertainty of either an individual or a group about events; often logical/objective and subjective epistemic interpretations are distinguished. Epistemic probability is an interpretation of probability, whereas I understand Bayesian statistics as statistics involving Bayes’ theorem and reasoning based prior and posterior probabilities. This is often, but not always, done based on an epistemic interpretation of probability, and is therefore not in itself an interpretation of probability. See Gillies (2000); Galavotti (2005) for overviews of interpretations of probability.

I do not think that the adoption of one interpretation of probability in one situation should preclude a statistician from using another interpretation in another situation. A statistician can be a frequentist when analysing a randomised trial to compare a new drug with a placebo, and adopt a logical epistemic approach when
computing a probability that an accused is really the murderer, and in this sense she can be a pluralist. However, if in a single analysis the implied interpretation of probability is not clear, results in terms of probability do not have a meaning.

Frequentism-as-model is a flavour of frequentism in the sense that a probability of an event $A$ refers to the limiting relative frequency for observing $A$ under idealised infinite repetition of a random experiment with possible outcome $A$. There are substantial problems with frequentism. It is hard to define what a “random experiment” is in reality. In the following I will generally use the term “experiment” for a situation that is modelled as potentially having several different outcomes to which probabilities are assigned. One of the central proponents of frequentism, von Mises (1939), tried to ensure the random character of sequences by enforcing relative frequency limits to be constant under certain place selection rules for extracting a subsequence from the infinite sequence of experiments. His and other attempts have been criticised by the lack of connection of both the hypothesised limit and the definition of admissible sequences to observed relative frequencies in finite sequences, which are the only ones we can observe (see, e.g., Hajek (2009); Diaconis and Skyrms (2018)).

Von Mises and most other frequentists claim that their conception is applicable to experiments allowing for lots of (idealised infinitely many, idealised identical) repetitions, but in fact in standard frequentist probability modelling such repetitions are modelled as identically and independently distributed (i.i.d.), meaning that a whole sequence of repetitions is modelled by a single probability model, modelling probabilities not only for the outcomes of a single replicate, but for combinations of outcomes of some or all replicates. Central results of probability theory such as the laws of large numbers and the central limit theorem apply to such models. Applying the traditional frequentist interpretation of probability to them would require whole independent and potentially infinite sequences to be repeated, which of course is impossible.

Frequentism-as-model deals with these issues in two ways. Firstly, it emphasises that probability is a model and as such fundamentally different from observed reality. Adopting frequentism-as-model means to think about an experiment or a situation as if it were generated by a frequentist process, temporarily, without implying that this really is the case. This means that it is not required to make a case that the experiment is really infinitely or even very often repeatable and will really lead to the implied relative frequencies. Particularly this means that all available observations can be modelled as i.i.d. without having to postulate that the whole sequence of observations can be infinitely repeated. Obviously, insisting on the fundamental difference between model and reality in this way raises the question how we can use such a model to learn something useful about reality, which I will discuss in Section 3.2. Von Mises and other frequentists already acknowledged that frequentist probabilities are idealisations. To me it seems, however, that in traditional frequentism this is only a response to critics, and that otherwise it does not have consequences regarding data analyses and their interpretations. Frequentism-as-model is different, as elaborated below.
Secondly, whereas it is not clear how to connect the limits of traditional frequentist infinite sequences to an observed finite amount of real data as any limit is invariant against arbitrary changes of any finite beginning of a sequence, i.i.d. models, as well as models with other fully specified dependence and non-identity structures, make probability statements about finite sequences of observations, and these can be checked against the actual observations. It can be found out if and in which sense the observed sequence would have been “typical” according to the specified model. In fact, whereas we may not be able to observe another real data sequence of the same length, and surely not arbitrarily many, we are able to generate an almost arbitrary number of repetitions of sequences from the model involving just the light idealisation that we would need a perfect random number generator. Even without a random number generator, in many situations probability theory can tell us what to expect if the model holds. This resonates with Gillies’s long run propensity interpretation of probability, where probabilities are not defined referring to infinite sequences, but rather as tendencies to observe in finite experiments a “typical” (i.e., large probability) outcome as specified by the model.

An issue with this is the definition of “typical”. For example, assuming an i.i.d Bernoulli model, a sequence that has 50 zeroes, then 50 ones, then 50 zeroes, than 50 ones again, and so on (called 50-50-sequence in the following), has the same probability as a randomly looking sequence of zeroes and ones that gives us no intuitive reason to doubt the i.i.d. assumption. On what basis can we claim that the i.i.d. model is adequate (to use Davies’s terminology) for the latter sequence but not for the 50-50-sequence? This requires subjective researcher input. The researcher needs to decide about the specific way, or ways, in which a sequence has to be typical according to the model in order for the model to be “adequate”. For example, the researcher may decide that the model is only useful for data in which the lengths of “runs” of zeroes or ones in a row are not longer to what is expected under the model, in which case the model will be ruled out for the 50-50-sequence (which can be formally done using the runs test, Lehmann (1986), p.176), whereas the irregular sequence will count as typical. A reason for such a decision may be that the researcher may think that sequences with long runs of the same outcome are better modelled by models involving positive dependence between the individual binary experiments, and that under such models it can be shown that the method of analysis the researcher would want to apply to an i.i.d. Bernoulli sequence will be misleading. This could for example be a confidence interval for the probability of observing a one. Keeping in mind the fundamental difference between model and reality, the researcher in this situation does not make a decision about which model she believes is in fact true. For such a belief, the subjective decision to distinguish between two sequences of same individual probability may look questionable. The actual decision is rather about case-dependent criteria regarding in terms of what model she wants to think when analysing the data, informed by the aim of data analysis as well as the characteristics of potential alternative models. Note though that if the researcher uses the runs test as decision
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rule, she will never be able to observe a 50-50-sequence whenever she uses the i.i.d. Bernoulli model, under which such sequences should actually be possible. This is an instance of the “misspecification paradox”, see Section 4.3.

Probability theory offers ways to help the researcher making such decisions, by for example addressing questions of the form “If indeed the model is as assumed, how likely is it to decide against it; or if a different model is true, how likely is it to not detect it and wrongly stick to the original one; and how bad would the consequences of such an erroneous decision be in terms of the probability of getting a misleading result of the final analysis?” The researcher may also decide to adopt a more robust final method of analysis in order to deal appropriately with a larger number of models between which the data cannot distinguish.

By always keeping in mind that reality is only treated temporarily as if generated by a certain model, not ruling out the possibility that it could be different (for which we may consider different models), and even taking this possibility into account when making further data analytic decisions, frequentism-as-model takes the fundamental difference between model and reality much more seriously into account than a standard frequentist analysis, in which a model is used and normally no longer questioned after some potential initial checks. This leads to interpretations of the final results that often seem to naively imply that the model is true, even if the researcher using such an approach may well admit, when asked explicitly, that reality actually differs from the model.

2.2 The role of i.i.d. models and subject-matter knowledge

Thinking about reality as if it were generated by a certain probability model has implications. A critical discussion of these implications is a way to decide about whether or not to adopt a certain model, temporarily, and on top of this it bears the potential to learn about the real situation. If a sequence is thought of as generated by an i.i.d. process, it means that the individual experiments are treated as identical and independent. De Finetti made the point that whatever can be distinguished is not identical. Treating experiments as identical in frequentism-as-model does not mean that the researcher believes that they are really identical, and neither do they have to be really identical in order to license the application of such a model. Rather it implies that the differences between the individual experiments are temporarily ignored or, equivalently, treated as irrelevant; analogously, using an independence model does not mean that the researcher believes that experiments are really independent in reality, but rather that she assesses potential sources of dependence as irrelevant to what she wants to do and how she wants to interpret the result. Of course, depending on what kind and strength of dependence can be found in reality, this may be inappropriate. On such grounds, the model can be criticised based on subject-matter knowledge, and may be revised. This allows for a discussion about whether the model is appropriate, on top of possible checks against the data, which may have a low power detecting certain deviations from an i.i.d. model.
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I.i.d. models, and their Bayesian counterpart, exchangeability models, play an important role in frequentism-as-model as well as in applied statistics in general. Of course many non-i.i.d. models are used such as regression where the response distribution is non-identical depending on the explanatory variables, and models for time series or spacial dependence. Even those models will normally have an i.i.d. element, such as i.i.d. residuals or innovations, be it conditional in a Bayesian setup. This is not an accident. Treating experiments as identical and independent (or having regular and fully specified dependence or non-identity structures that will usually involve an i.i.d. element) specifies how an observation from one experiment can be used for learning about another, which is the core aim of statistics. Statistics relies on models that allow us to use different observations to accumulate information. This particularly means that the statistician will need to make decisions to ignore certain potential irregular differences or dependencies in order to be able to do her job, regardless of whether her position is frequentism-as-model, traditional frequentism, or an epistemic Bayesian approach. Obviously i.i.d. models do not necessarily lead to convincing results, and ultimately it is a matter of experience and practical success in what situations the use of these models is appropriate. Note in particular that it is possible with a sufficiently large amount of data to distinguish, with large probability, i.i.d. data from data with a regular dependence structure such as ARIMA. It is however not possible to distinguish i.i.d. data from arbitrarily irregular structures of dependence or non-identity. A model that states that the first observation is random but conditionally on it all further observations are fixed with probability one can never be ruled out based on the data alone, and subject-matter arguments are required to make a reasonably regular model plausible.

At the stage of analysis, both checking of adequacy based on the data and understanding and discussion of the subject matter background are of crucial importance. The charge of an ignorance of subject-matter information that Bayesians often raise against frequentists does not affect frequentism-as-model. On the flipside, frequentism-as-model obviously relies on subjective decisions of the researchers, so that a statistician holding it cannot proudly claim that analyses are fully objective, as some frequentists and objective Bayesians like to do, which is questionable anyway (Gelman and Hennig (2017)).

As an example for subject-matter arguments, in a much-cited and influential study that prompted a Lancet editorial (Lancet (2005)) with the title “The end of homeopathy”, Shang et al. (2005) carried out a meta-analysis of eight studies comparing homeopathy with a placebo using a standard meta-analysis model with a random study effect assumed i.i.d. They found that there is no evidence that the odds ratio between “homeopathy works” and “placebo works” is different from one and concluded (not inappropriately) that “this finding is compatible with the notion that the clinical effects of homeopathy are placebo effects.”

Here are two implications of the i.i.d. model for the random study effect. Firstly, modelling it as identically distributed implies that the differences between studies are attributed to random variation, which basically means that knowledge
of known differences between the studies is ignored for modelling. This particularly includes differences between different modes of applying homeopathy. For example, there were only two studies used in the meta-analysis in which homeopathy was applied in the classical way with individual repertorisation, which is seen as the only proper way of applying homeopathy by many homeopaths. Modelling the study effect of these studies as coming from the same distribution as all the other study effects may be justifiable if there is an a priori belief that homeopathy basically is a placebo and the differences between application modes are irrelevant, but it is not suitable for convincing skeptical supporters of classical homeopathy who would be willing to accept a fair study.

Secondly, an expected value of 0 of the random effect is not assumed for every single study, but over all studies, with a potentially arbitrary large between-studies variance. This implies that the effective sample size for estimating the odds ratio is not the number of individual patients treated in all studies (several thousands), but the number of studies, \( n = 8 \), and \( n = 2 \) for classical homeopathy only, which the authors may use to justify their decision not to make a modelling difference between different modes of applying homeopathy, selected by Shang et al. (2005) by a study quality criterion. The power of the resulting test is therefore very low, and consequently the confidence interval that the authors give for the odds ratio is very wide. This argument applies to meta-analyses with a random effect generally; in the literature one can sometimes find the informal remark that such analyses require a large number of studies, see Kulinskaya et al. (2008). On top of that it could be discussed whether there may be reasons for assuming systematic bias over all studies, which in the model would amount to a nonzero expectation of the random effect.

Obviously \( n = 8 \) is not sufficient to check the i.i.d.-assumption for the random study effect at any reasonable power. Rather than having any connection to an observable “truth”, the random effect is a convenient modelling device where individual study effects cannot be ignored, but the implications above are usually ignored.

Given this, it looks very dubious that the Lancet (2005)’s issue editorial concluded: “Surely the time has passed for selective analyses, biased reports, or further investment in research to perpetuate the homeopathy versus allopathy debate.” Regarding the purely statistical evidence, the study itself could as well be used to argue that there are not yet enough studies.

In principle such discussions can also take place regarding the traditional frequentist use of models, but frequentism-as-model re-frames these discussions in a helpful way. I makes us aware of the implicit meaning of the model assumptions, which can be sued to discuss them. And it emphasises that what has to be decided is not the truth of the model, but rather a balance between the capacity of the model to enable learning from the existing observations about future observations or underlying mechanisms on one hand, and on the other hand taking into account all kinds of peculiarities that may be present in the real situation of interest, but that may be hard to incorporate in such a model - even though they may have
more or less strong impact on the results.

A key aspect is that rather than thinking in binary terms about whether a model is true or not, frequentism-as-model implies that the connection between observed reality and models can be assessed gradually as more or less close. It is neither impossible nor inadmissible to conceive a situation as a random experiment that cannot or only a very limited number of times be repeated, and to use a model in the sense of frequentism-as-model for it. This would imply to think of the situation as hypothetically repeatable, and of certain outcomes having a certain tendency, or, in standard philosophical terms, propensity to happen, which would materialise in case of repetition, but not in reality.

Therefore frequentism-as-model does not bar single-case probabilities as von Mises’s flavour of frequentism does. But it is more difficult for the person who puts up such a model to convincingly justify it compared to a situation in which there are what is interpreted as “replicates”. Convincing justification is central due to the central role of communication and agreement for science in the philosophy outlined above. Whatever the model is used for, it must be taken into account that hypothesised values of the single-case probability cannot be checked against data. This changes if the experiment is embedded in a set of experiments that are not directly seen as repetitions of each other, but for which an assumption is made that their results are put together from systematic components and an error term, and the latter is interpreted as i.i.d. repetition. This is actually done in standard frequentist regression analyses, in which the $x$ is often assumed fixed. For a given set of explanatory variables $x$ a distribution for the response $y$ is implied, despite the fact that the random experiment can in reality not be repeated for any given $x$ in situations in which the researcher cannot control the $x$. Insisting on repeatability for the existence of traditional frequentist probabilities would imply that nothing real corresponds to the distribution of $y$ for fixed $x$, and there is no way to check any probability model. The unobservable i.i.d. error term constructs repeatability artificially, but I have never seen this mentioned anywhere. From a frequentism-as-model perspective this needs to be acknowledged, and dependent on the situation it may be seen as appropriate or inappropriate, once more using information from the data and about the subject matter background, but there is nothing essentially wrong or particularly suspicious about it, apart from the fact that it turns out that our possibilities to test model assumptions are quite generally more limited than many might think, see Section 4.3.

3 Frequentism-as-model and reality

Given the separation between reality and model in frequentism-as-model, using such methods to address real problems requires justification. Before this is addressed in Section 3.2 I will summarise my general ideas about how mathematical models related to reality in Section 3.1.
3.1 Mathematical models and reality

I wrote [Hennig (2010)] mainly out of the conviction that the debate about the foundations of statistics often suffers from the lack of a general account of mathematical models and their relation to reality.

On the frequentist side often a rather naive connection between the models and reality is postulated, which is then often criticised by advocates of epistemic probability, implying that because this does not work, probability should better be about human uncertainty rather than directly about the reality of interest. But modelling human uncertainty is still modelling, and similarly naive ideas of how such models relate to “real” human uncertainty are problematic in similar ways. More about this in Section 5.1.

In [Hennig (2010)] I have argued that mathematical models are thought constructs and means of communication about reality. I have distinguished “observer-independent (or objective) reality”, “personal reality” (the view of reality constructed by an individual observer and basis for their actions), and “social reality” (the view of reality constructed by communication between observers). I have argued from a constructivist perspective that implies that observer-independent reality is not accessible directly for human observers, and that, if we want to talk about reality in a meaningful way, it makes sense to refer to personal and social reality, which are accessible by the individual, a social group, respectively, rather than only to observer-independent reality. Observer-independent reality manifests itself primarily in the generally shared experience that neither an individual observer nor a social system can construct their reality however they want; we all face resistance from reality. This resonates with Chang (2012)’s “Active Scientific Realism”: “I take reality as whatever is not subject to one’s will, and knowledge as an ability to act without being frustrated by resistance from reality. This perspective allows an optimistic rendition of the pessimistic induction, which celebrates the fact that we can be successful in science without even knowing the truth. The standard realist argument from success to truth is shown to be ill-defined and flawed.”

In [Hennig (2010)] I interpret mathematical models as a particular form of social reality, based on the idea that mathematics is a particular form of communication that aims at being free of ambiguity, and that enables absolute agreement by means of proofs. Science is seen as a social endeavour aiming at general agreement about aspects of the world, for which unambiguous mathematical communication should be obviously useful. This comes to the price that mathematics needs to unify potentially diverging personal and social observations and constructions. There is no guarantee that by doing so mathematical models come closer to observer-independent reality; in fact, this view sees the domain of mathematics as distinct from the domain of observer-independent reality. The connection is that the constructs of personal and social reality are affected by “perturbation” (or, as Chang puts it, “resistance”) from observer-independent reality, and it can be hoped that such perturbation, if observed in sufficiently related ways by different individuals
and social systems, can be represented in mathematical modelling, which then allows to analyse the represented reality using the machinery of mathematics and logic. There is also repercussion of communication, including mathematics, on social and personal constructions of reality. Mathematical modelling has an effect on the world view and dealings with the world of social systems and individuals. We can hope for this effect to be beneficial, but this may not always be the case, because important subtleties and differences between individual and social realities may be lost in mathematics.

The constructivism I adhere to is these days rather unpopular in philosophy, and what Chang refers to as “standard realism” in his quote above has won some ground. However, given the rather tedious controversy that philosophers have about the objective “existence” of long run frequentist or single-event chances and propensities (see Eagle (2019)), it seems as attractive as ever to me to treat them as a thought construct and to look at how they are used and how they relate to observations in order to circumvent the enormous amount of problems that come with postulating the objective “existence” of such a thing.

3.2 How is frequentism-as-model useful?

From Section 3.1 it follows that the separation between reality and model is a general feature of mathematical models, not an exclusive one of frequentism-as-model, although frequentism-as-model acknowledges it explicitly. The question “how is frequentism-as-model useful, and how is it connected to reality?” is therefore connected with the general question how mathematical models can be useful, even if “wrong” in Box’s terms. The way to use mathematical models is obviously to take the mathematical objects that are involved in the model as representations of either perceived or theoretically implied aspects of reality; and then to use this to interpret the results of using the models.

In order for the model to be useful, does not reality have to be “somehow” like the model? According to the constructivist view in Section 3.1 reality is accessible only through personal perception and communication, so the best we can hope for is that the model can correspond to how we perceive, communicate, and think about reality. Regarding frequentism-as-model, this concerns in the first place the concept of i.i.d. repetition, see Section 2.2. As follows from there, i.i.d. repetition is a thought construct and not an objective reality; however, even apart from probability modelling it is easy to see how much of our world-view even before any science relies on the idea of repetition, be it the cycles of day and night and the seasons, or be it the expectation, out of experience, of roughly the same behaviour or even roughly the same distribution of behaviours when observing any kind of recurring process as diverse as what a baby needs to do to attract the attention of the parents, look, size, and edibility of plants of the same species, or any specific industrial production process. Objections against the truth of “i.i.d” can be easily constructed for observations in all of these examples, yet analysing them in terms of i.i.d. models often gives us insight, new ideas, and practical
competence to deal with these processes, be it with additional ingredients such as explanatory variables, regular dependence structures, or seasonal components. Ultimately the success of probability modelling has to be evaluated empirically, relative to researchers’ aims, and obviously the result will sometimes, but not always, be positive. I remain agnostic regarding to what extent positive results confirm that “objective reality really is like that”, to which there ultimately is no answer beyond what occurs in personal and social reality.

A major general reason for using probability models is that they allow us to quantify random variation. It is not an accident that probability theory is a relatively young branch of mathematics. Unlike most historically older mathematical modelling, probability modelling is essentially not only about what was, is, or will be observed, but also about what could have been observed but was not and/or will not be observed. The latter does not and will not exist as observation. Connecting this to observations is tricky and in my view a major reason why probability models are so controversial and problematic. But they allow us to deal with a concept that was hardly acknowledged when probability theory emerged, and became hugely influential, namely the distinction between a meaningful pattern and meaningless variation.

What can be done in order to support useful and avoid misleading results of probability modelling interpreted as frequentism-as-model? Below are some questions that need to be addressed. These questions are not very surprising, and statisticians, be they traditional frequentists, Bayesians, or something else, may think that they address these anyway. What makes frequentism-as-model different is that a standard approach would be to take these issues into account when setting up and justifying the model, and then all further analysis and interpretation is done conditionally on the model being true. In frequentism-as-model there is no such thing as a true model, meaning that the issues below need to be kept in mind, and they still play a role when running model-based analyses and interpreting the results.

1. **Is the model compatible with the data?** I will discuss in more detail in Section 4.

2. **How does the model relate to subject matter knowledge?** The model should be informed by perceptions and ideas (subject matter knowledge, which is not necessarily “objective”) regarding the real process to be modelled, such as specific reasons for dependence, non-identity, or also for or against particular distributional shapes such as symmetry. Just to give an example, dependence by having the same teacher and communication between students makes the use of an i.i.d. model for analysing student results for students from the same class or even school suspicious, and it can be controversially discussed to what extent introducing dependence only in form of an additive random effect addresses this appropriately.

3. **What is the aim of modelling?** Probability models are used in different
ways, and this is important for how to set them up. Although the aim of modelling is not always ignored in traditional frequentist or Bayesian analysis, there is a general tendency to think that the ultimate aim is always to find the true model, and if this (or at least a model as “close” as possible to it) is found, everything “relevant” that can be known is known. This is fundamentally different from the attitude implied by frequentism-as-model. 

Cox (1990) lists three different classes of modelling aims, namely “substantive models”, which model causal mechanisms that the researcher is interested in, “empirical models”, which try to aid inference about relations between variables such as effect sizes, without making an attempt at reconstructing how the relations work in any detail, and “indirect models”, where models are not directly connected with a real situation. This encompasses the use of models for deriving or comparing methodology, but also, in a real situation but only indirectly serving the main aim of analysis, things such as the imputation of missing values. The classes may be mixed or intermediate in some specific situations.

This has methodological consequences. Substantive models may have aspects that are there for representing the researcher’s view but may refer to unobservables, in which case they cannot or in the best case very indirectly be tested against the data. A substantive model may even be there to communicate a researcher’s view without the ambition to fit any data. It may at times represent scenarios that are seen as best or worst cases, intentionally not modelling what is seen as most realistic. Parsimony may be a strong concern if the model is used to communicate an idea, a weaker concern if the model is used for prediction, and no concern at all if the model is used to simulate data for exploring potential future variation in a complex system. In some cases the major use of the model is learning from discovering its lack of fit and the exploration of reasons.

At first sight it seems central for of empirical models to fit the data. But also for such models the specific modelling aim is important, because deviations of the data from what would be typical for the model are only a problem to the extent that they raise the danger of misleading results, which depends on what kind of result the researcher is after, e.g., modelling integer number data by a normal distribution is not normally a problem if inference about means is required, at least not as long as the i.i.d. assumption is implied and the distributional shape is not strongly skew or with large outliers, both of which can happen with continuous data as well.

A major indirect use of models is the investigation of properties of inference methods by theory or simulation. For example, maximum likelihood estimators can be seen as inspired by probability models that are interpreted in a frequentism-as-model sense: “Imagine a real situation as modelled, then the ML estimator makes the data most likely”. The major aim of modelling there is to find a good method for estimation. Even many Bayesians use
models with an implicit frequentist interpretation in methodological work in order to investigate the quality of their methods in a situation with a constructed known truth. It could be argued that in order for such work to be applicable to objective reality, objective reality has to be as the model specifies, but if we accept that “all models are wrong” in objective reality, there is no better alternative than studying the behaviour of method under such a model. Simulations and theory will seem more relevant if the used models seem “realistic”, but as long as the aim is not to address a very specific application, models used in this way are not checked against data. This only can happen when the resulting methods are applied, see Section 4.3.

Furthermore, for some modelling aims there are methods that can more or less directly measure how well the aim is fulfilled. A major example is cross-validation for prediction quality. If prediction is the major modelling aim, good cross-validation results can be seen as dominating other concerns such as fit of the existing data or correspondence with subject matter knowledge. Breiman (2001) argued that if prediction quality is a major aim, the approach to model the data generating process is often inferior to using non-probabilistic algorithmic models; and he believes that prediction quality is almost always of dominating importance. I agree with Breiman that probability modelling is certainly not mandatory, which runs counter to the naive idea that finding the true model is the ultimate aim of statistics. On the other hand, Breiman (2001) and much work from the machine learning community seem to reduce aims of modelling to prediction quality as all-dominating issue, which neglects the role of models for communication and building understanding, even though it is probably valid to state that prediction quality is very often at least implied by the aim of analysis.

Also there are doubts to what extent prediction quality for future events can be reliably assessed. For example, there is the possibility that the process of interest in the future may change, and a transparent model with solid subject matter justification may be more easily adapted, as Hand (2006) argues. Cross-validation and other methods for assessing prediction quality imply i.i.d. repetition, which may be problematic, see above.

Another example for direct measurement of what is of interest is the objective function of $k$-means clustering, which directly measures the quality of the approximation of any data point by the closest cluster centroid (more generally, often loss functions can be constructed that formalise the aim of analysis). If achieving a good performance in this respect is the aim of analysis (for example if clustering is used for data set size reduction before some other analysis, and all clustered objects are replaced by the cluster centroid in a final analysis), $k$-means clustering is a suitable method even in situations that are very different from the model for which $k$-means is maximum likelihood, namely normal distributions with same spherical covariance matrix in all clusters but different cluster means. Statisticians have argued that $k$-
means often does not work well if this assumption is violated (e.g., Vermunt (2011) in a discussion of Steinley and Brusco (2011)), but this relies on the view that reconstructing the true model is required, which may deviate from what is needed in practice.

(4) How stable are conclusions against modelling differently? The ultimate answer to the question how we can be sure that a model is appropriate for the situation we want to analyse is that we cannot be. Breiman (2001) mentioned the “multiplicity of good models” in line with Tukey (1997); Davies (2014), and different “good” models may lead to contradicting conclusions. On top of that, research hypotheses may be operationalised in different ways, using different measurements, different data collection and so on. There is no way to be sure of a scientific claim backed up by a statistical result based on the naive idea that the model is true and the method is optimal. In order to arrive at reliable conclusions, science will need to establish the stability of the conclusions against different ways of operationalising the problem. This is basically the same way in which we as individual human beings arrive at a stable concept of the world, as far as it concerns us; after lots of experiences, looking at something from different angles in different situations, using different senses, communicating with others about it etc., we start to rely on our “understanding” of something. In the same way, every single analysis only gives us a very restricted view of a problem, and does not suffice to secure stability, see the discussion of “interpretative hypotheses” in Section 4.1. Currently there is talk of a “reproducibility crisis” in a number of disciplines (Fidler and Wilcox (2018)). I think that a major reason for this is that no single analysis can establish stability, which is all too conveniently ignored, given that researchers and their funders like to tout big meaningful results with limited effort. Even if researchers who try to reproduce other researchers’ work have the intention to take the very same steps of analysis described in the original work that they try to reproduce, more often than not there are subtle differences that were not reported, like for example data dependent selection of a methodology that the reproducer then uses unconditionally. Any single analysis will depend on researchers’ decisions that are rarely fully documented, could often be replaced by other decisions that are not in any way obviously worse, and may have a strong impact on the result. Having something confirmed by an as large as possible number of analyses investigating the same research hypothesis of interest in different ways is the royal road to increase the reliability of scientific results. In this I agree with the spirit of Mayo (2018)’s “severity”, and it also resonates with her “piecemeal testing”; a scientific claim is the more reliable, the harder researchers have tried to falsify it and failed. Frequentism-as-model surely does not license claims that anything substantial about the world can be proved by rejecting a single straw man null hypothesis.
4 Frequentism-as-model for statistical inference

Adopting frequentism-as-model as interpretation of probability does not imply that classical methods of frequentist inference, tests, confidence intervals, or estimators have to be used. It may also be combined with Bayesian inference, see Section 5.2. However, the classical methods of statistical inference have valid frequentism-as-model interpretations, and I believe that understanding these interpretations properly will help to apply the methods in a meaningful and useful way. Therefore I disagree with recent calls to abandon significance testing or even frequentist inference as a whole (Wasserstein et al. (2019)). In Section 4.1 I introduce the key concept of “interpretational equivalence”. Section 4.2 is about the frequentism-as-model interpretation of these methods. Section 4.3 is devoted to the issue of model assumptions for frequentist inference, and how these are important given that they cannot literally be fulfilled.

4.1 Interpretational equivalence, stability, and robustness

The question how stable our conclusions are as raised in Section 3.2 does not only depend on models, methods, and the data, but also on the conclusions that are drawn from them, i.e., the interpretation of the results by the researcher. A helpful concept to clarify things is “interpretational equivalence”. I call two models “interpretationally equivalent” with respect to a researcher’s aim if her subject-matter interpretation of what she is interested in is the same regardless of which of the two models is true. For example, if a researcher is interested in testing whether a certain treatment improves blood pressure based on paired data before and after treatment, she will in all likelihood draw the same conclusion, namely that the treatment overall does not change the blood pressure, if the distribution of differences between after and before treatment is symmetric about zero, be it a Gaussian or a $t_2$-distribution, for example. For her research aim, the precise shape of the distribution is irrelevant. Comparing two models with expectation zero, one of which is symmetric whereas the other one is not, this is not so clear; the treatment changes the shape of the distribution, and whether this can be seen as an improvement may depend on the precise nature of the change. For example, consider the model

$$0.99\mathcal{N}(\mu^*, \sigma^2) + 0.01 * \delta_{100},$$

(1)

$\delta_x$ being the one-point distribution in $x$. The researcher may think of $\delta_{100}$-distribution as modelling erroneous observations, in which case $\mu^* = 0$ makes the model interpretationally equivalent to $\mathcal{N}(0, \sigma^2)$, whereas for $\mu^* = -\frac{1}{\sqrt{0.99}}$, despite expectation zero, the researcher will interpret an average effect of lowering the blood pressure, which the model implies for a subpopulation of $99\%$ of people.

The importance of interpretational equivalence is that it allows to discuss what deviations of a temporarily assumed model should be handled in which way. If conclusions are drawn from a method involving certain model assumptions, it would be desirable, and conclusions can be seen as stable, if the probability would
be roughly the same that the researcher arrives at the same conclusion from the same data if data are actually generated by an interpretationally equivalent model other than the assumed one. If this is not the case, it would be of interest if the models can be told apart with good probability by the data, in which case the data can be used to decide on which one of these models to base a data analysis. This also applies to the decision between a specific parametric and a more general but potentially less precise nonparametric approach, see Section 4.3.

The concept also clarifies the issue that large samples reject point null hypotheses too easily. Considering a $\mu = 0$ null hypothesis, a model with $\mu \neq 0$ but a very small absolute value will lead to a rejection of $\mu = 0$ with a large probability for large $n$. This is a problem if and only if models with, say, $|\mu| < \epsilon$, $\epsilon > 0$ are considered interpretationally equivalent to the model with $\mu = 0$, in other words, if a difference as small as $\epsilon$ is considered substantially irrelevant. If this can be specified, the question whether values of $\mu$ with $|\mu| < \epsilon$ are in the corresponding confidence interval, or what severity the test achieves ruling out $|\mu| < \epsilon$, see Mayo (2018), is more relevant than whether a test of $\mu = 0$ is significant.

Being concerned about interpretational equivalence of models is an entry point for robust methods, which are designed for dealing with the possibility that a nominal model is violated in ways that are hard to diagnose but may make a difference regarding analysis. Robustness theory is about limiting changes in results under small changes of the modelled processes. An issue with standard robustness theory is that it is often implied that “contamination” of a distribution should not have an influence on the results, whereas in practice the contamination may actually be meaningful. To decide this is a matter of assessing interpretational equivalence. Frequentism-as-model acknowledges that desirable behaviour of a statistical method is not just something “objective” but depends on how we interpret and assign meaning to differences between distributions. Robustness is helpful where the reduced sensitivity of robust methods to certain changes in the data or model takes effect where the changes are indeed interpreted as meaningless relative to the aim of analysis; for example there needs to be a decision whether in model (4) what correspond to the quantity of interest is rather the mean of the distribution, or rather the $\mu^*$. In some applications it is inappropriate to use a method of which the results may not change much when replacing up to half of the data, namely where the resulting processes are considered as interpretationally very different, but in any case comparing the results of such a method with a more “sensitive” non-robust one may allow for a more differentiated perception of what is going on than does every single method on its own.

4.2 Significance tests, frequentist inference
Statistical hypothesis tests have become very controversial (e.g., Wasserstein et al. 2019), but the interpretation of tests from the position of frequentism-as-model is rather straightforward. They address to what extent models are compatible with the data in the sense formalised by the test statistic. For simplicity, I mostly call
models “compatible” or “incompatible” with data, but in fact compatibility is of course gradual, as expressed by \( p \)-values. Obviously a non-rejection cannot be an indication that the model is in fact true, but it means the absence of evidence in the data that reality is different in the way implied by the test statistic, making it impossible to claim such evidence. Sometimes tests, particularly two-sided tests, are criticised because “point null hypotheses are usually scientifically implausible and hence only a straw man” (e.g., [Rice et al. (2020)]), but then a parametric model allowing for any parameter value cannot be “really true” anyway, so that for the same reason for which the point null hypothesis (\( H_0 \)) is “implausible”, the whole parametric family is implausible as well, but that does not stop inference about it from being informative and useful, see Section 3.2. What a test can do is neither to confirm \( H_0 \) as true, nor to allow to infer any specific alternative in case of rejection. A two-sided test should be run if compatibility of the data with \( H_0 \) is a possibility of interest; even then it may not be the method of choice if the sample size is too large, see Section 4.1.

Often significance tests are presented implying that a rejection of the \( H_0 \) is meaningful whereas a non-rejection is not. Above I have explained the meaning of a non-rejection in frequentism-as-model; the meaning of rejection is a more complex issue. Obviously, rejection of the \( H_0 \) does not imply that there is evidence in favour of any particular model that is not part of the \( H_0 \). On the positive side, a rejection of the \( H_0 \) gives information about the “direction” of deviation from the \( H_0 \), which can and mostly should be substantiated using confidence intervals as sets of compatible parameter values, and particularly data visualisation for exploring how this plays out without relying on the specified model.

Consider a one-sample t-test of the \( H_0 : \mu = 0 \) regarding \( \mathcal{N}(\mu, \sigma^2) \) against \( \mu > 0 \). The test statistic \( T \) is the difference between the sample mean and \( \mu = 0 \) standardised by the sample standard deviation. Sticking to model-based thinking while not implying that \( \mathcal{N}(\mu, \sigma^2) \) is true, rejection of the \( H_0 \) can be interpreted as providing evidence in favour of any distribution of the two samples for which a larger value of \( T \) is observed with larger probability than under \( H_0 \), compared with the \( H_0 \). We do not only have evidence against \( H_0 \); we also learn that the problem with \( H_0 \) is that \( \mu = 0 \) is most likely too low, which is informative.

In many situations in which the one-sample t-test is used, the user is interested in testing \( \mu = 0 \) or rather its interpretational meaning, but not in the distributional shape \( \mathcal{N}(\mu, \sigma^2) \), which just enables her to run a test of \( \mu = 0 \). If in fact there were a true distribution, which of course we can consider, in the “as if”-model world, which isn’t \( \mathcal{N}(\mu, \sigma^2) \) with any specific \( \mu \) or \( \sigma^2 \), it is not clear in a straightforward manner what would correspond to \( \mu \). It may be the expected value, but in a model such as (1) it may well be seen as the \( \mu^* \) that governs 99% of the observations. In any case, there are distributions that could be considered as interpretationally equivalent to the actual \( H_0 \). Any distribution symmetric about zero is in most applications of t-tests interpretationally equivalent to the \( H_0 \), meaning that if indeed a distribution different from \( \mathcal{N}(\mu, \sigma^2) \) but still symmetric about \( \mu = 0 \) were true, it would be desirable that the probability to reject \( H_0 \) would be as low as if
$H_0$ were true.

One can then ask how likely it is to reject the $H_0$ under a model that is not formally part of the $H_0$, but interpretationally equivalent. For the one-sample t-test, assuming existing variances, many distributions interpretationally equivalent to $\mathcal{N}(\mu, \sigma^2)$ are asymptotically equivalent as well, and results in Cressie [1980] and some own simulations indicate that it is very hard if not impossible even in finite samples to generate an error probability of more than 7% for rejecting an interpretationally true $H_0$ with a nominal level of 5%, even if the underlying distribution is skew and $\mu$ is taken to be the expected value, meaning that testing at a slightly smaller nominal level than the maximum error probability that we want to achieve will still allow for the intended interpretation.

But the situation becomes much worse under some other deviations from the model assumptions, particularly positive dependence between observations, which increases the variation of $T$ and may lead to significant $T$ and increased type I error probability very easily, also in situations that are interpretationally equivalent to the $H_0$. Consider an example in which the expected change of the turnover generated by a salesperson after attending a sales seminar is of interest in order to evaluate the quality of the instructor. If all salespersons in the sample attend the sales seminar together, they may learn something useful from talking with each other about sales strategies, even if what the instructor does itself is useless.

What is required in order to achieve a reliable conclusion is just what was discussed in Section 3.2. The general question to address is whether a rejection of $H_0$ could have been caused by something interpretationally equivalent to the $H_0$, in which case the conclusion could not be relied upon. The questions to ask are: (1) is there any evidence in the data that such a thing may have happened, (2) is there any subject matter knowledge that suggests this (e.g., salespersons may have learnt from each other rather than from the seminar), (3) does the meaning of the test statistic correspond to what is of interest, and (4) is it feasible to run further analyses, with the same or other data, that can confirm the conclusion? (4) is important because even the best efforts to use (1) and (2) will not be able to remove all doubt. Models are always conceivable that can cause trouble but cannot be distinguished from the nominal model based on the data, and thinking about the subject matter may miss something important. (3) is in my view a very important issue that is not usually appreciated. Tests are usually derived using optimality considerations under the nominal model, but this does not imply that they are optimal for distinguishing the “interpretational $H_0$” from the “interpretational alternative”. In many cases at least they make some good sense, but a model like (1) may lead them astray. The full interpretational $H_0$ and the interpretational alternative will normally be too complex to derive any optimal test from them, but the form of the test statistic itself suggests what kinds of distributions the test actually distinguishes. This should be in line with what is interpreted as the difference of interest between $H_0$ and the alternative. At least under a point $H_0$, the distribution of any test statistic can be simulated (if the $H_0$ is a set of distributions, parametric bootstrap can be used, if potentially involving a certain bias).
Assuming that rejection happens for large values of the test statistic, it implicitly defines the interpretational $H_0$ and the interpretational alternative distinguished by the test as the set of distributions for which the test statistic is expected low, and the set of distributions for which it is expected larger, respectively. In many situations a nonstandard test statistic may be better suitable than what is optimal under a simple nominal model (see Hennig and Lin (2015) for an example).

Bayesians often argue against frequentist inference by stating that the probabilities that characterise the performance of frequentist inference methods such as test error and confidence interval coverage probabilities are pre-data probabilities, and that they do not tell the researcher about the probability for their inferences to be true after having seen the data. One example is that a 95%-confidence interval for the mean of a Gaussian distribution in a situation in which the mean is constrained to be larger than zero may consist of negative values only, meaning that after having seen the data the researcher can be sure that the true value is not in the confidence interval. This is a problem if the coverage probability of the confidence interval is indeed interpreted as a probability for the true parameter value to be in the confidence interval, which is a misinterpretation; the coverage probability is a performance characteristic of the confidence interval pre-data, assuming the model. From the point of view of frequentism-as-model, this is not a big problem, because there is no such thing as a true model or a true parameter value, and therefore a probability for any model to be true is misleading anyway, although it can be given a meaning within a model in which the prior distribution is also interpreted in a frequentism-as-model sense, see Section 5.2. In practice, if in fact a confidence interval is found that does not contain any admissible value, this means that either the restriction of the parameter space that makes all the values in the confidence interval impossible can be questioned, or that a truly atypical sample was observed. Davies (1995) defined “adequacy regions” that are basically confidence sets defined based on several statistics together (such as the mean or median, an extreme value index, and a discrepancy between distributional shapes; appropriately adjusting confidence levels), that can by definition in principle rule out all distributions of an “assumed” parametric family, meaning that no member of the family is compatible with the data given the combination of chosen statistics. Normally confidence intervals are interpreted as giving a set of truth candidate values assuming that the parametric model is true; but if that assumption is dropped from the interpretation and it is just a set of models compatible with the data defined by parameter values within a certain parametric family, it is possible that the whole family is not compatible with the data.

Overall I suspect that the biggest problem with interpreting standard frequentist inference is that many of the people who use it want to make stronger claims, and want to have bigger certainty, than the probability setup that they use allows. An example for this is the ubiquity of implicit or explicit claims that the null hypothesis is true in case that it was not rejected by a test. Interpreting the results of classical frequentist inference assuming the truth of the model will generally lead to overinterpretation. According to frequentism-as-model, classical frequentist in-
ference allows compatibility and incompatibility statements of models with data. As far as the models are used to make substantive statements, such compatibility assessment is of course of interest. What exactly can be learnt from this depends on the analysed situation as well as on what the researcher sees as interpretationally equivalent in that situation. From the point of view of frequentism-as-model, research about the expected variation of results under interpretationally equivalent models would be desirable, acknowledging that interpretational equivalence may mean different things in different applications. Deciding on a parametric model and from then on ignoring that other models can be compatible with the data as well will easily lead to overinterpretation.

4.3 Model assumptions

A standard statement regarding statistical methods is that these are based on model assumptions, and that the model assumptions have to be fulfilled for a method to be applied. This is misleading. Model assumptions cannot be fulfilled in reality, because models are thought constructs and operate on a domain different from observer-independent reality. For this reason they cannot even be approximately true, in a well defined sense, because no distance between the unformalised “underlying real truth” and the model can be defined, although, following Davies, the approximation notion can be well defined comparing observed data to a model.

What is the role of model assumptions then? There are theorems that grant a certain performance of the method, sometimes optimal, sometimes just good in a certain sense, under the model assumptions. The model assumptions are not required for applying the method, but for securing the performance achieved in theory. The theory can be helpful to choose a method, and some theory leads to the development of methods, but the theoretical performance can never be granted in reality. This does not mean that the performance will be bad whenever the model assumptions are not fulfilled. In fact, some aspects of the model assumptions are usually almost totally irrelevant for the performance, such as the application of methods derived from models for continuous data to data that is rounded to, say, two decimal places. Interpretational equivalence is an important concept also in this respect, because optimally methods would not only distinguish what is deemed relevant to distinguish by the researcher under the model assumptions, e.g., a certain $H_0$ and its nominal alternative, but they would lead to largely the same results for interpretationally equivalent distributions that do not fulfill the model assumptions. As far as this is the case, the corresponding model assumption is irrelevant.

It makes sense, at first sight, to think that the method will be appropriate if the assumed model is a good model, i.e., if reality looks very much like it. This itself can be modelled, meaning that one can look at the performance of a method in a situation in which the assumed method does not hold, but a somewhat similar model, which can be formalised using dissimilarity measures between distributions. This has been considered in robust statistics (Hampel et al. (1986), “qualitative
robustness” in particular) with the unsettling result that the performance of some classical statistical methods can decline strongly in arbitrarily small neighbourhoods of the assumed model. As a consequence, more robust methods have been developed, i.e., methods with a more stable performance in close neighbourhoods. This is surely valuable progress, but on one hand it does not change the fact that models can be set up that cannot be rejected by any amount of data, and that can annihilate the performance of any method, e.g., using irregular dependence and/or non-identity structures, so full safety cannot even be had in the modelled world, let alone the real one. On the other hand, the situation is not quite as hopeless as this may suggest. Usually bad results about standard methods are worst case results, very often regarding extreme outliers, and it is often plausible, from checks, visualisation, or subject matter considerations, that the worst case does not occur. To argue against detrimental dependence or non-identity structures is much harder, and the best that can be done in this respect is looking for conceivable reasons for trouble, using a suitable method if no such reason comes to mind, and then hope that the world will behave when acting based on the result.

Science makes mistakes and is hopefully open-minded enough to correct itself in case of bad outcomes.

Mayo (2018) (Sec. 4.8-4.11) and others argue that model assumptions can and should be tested. Whereas the truth of the model cannot be secured, much can indeed be found out about whether a model is adequate for a purpose. But model checking has a number of issues. The first one is that if a certain model assumption turns out to be compatible with the data, it does not mean that other models that may lead to different results are ruled out. Misspecification testing cannot secure robustness of results against models that cannot be excluded.

A second issue is that if a method is used conditionally on passing a model misspecification test for checking a model assumption, the distribution of the data that eventually go into the method becomes conditional on passing, and this will normally violate the model assumptions, even if they were not violated before. Particularly it will make observations dependent even if they were not dependent before, because if for example \( n - 1 \) observations are on the borderline for passing the misspecification test, the \( n \)th observation has to fit well for passing. I called this “goodness-of-fit (or misspecification) paradox” in Hennig (2007). In many situations this will not change error probabilities associated with the model-based method strongly, meaning that in case the model was true before misspecification testing, not much harm is done.

On the other hand, in case that the model assumption is violated in a problematic way, the distribution conditionally under passing a misspecification test will often not make the method work better, and sometimes even worse than before testing; keep in mind that just because the misspecification test does not reject the model assumption, it does not mean that it is fulfilled. Shamsudheen and Hennig (2019) reviewed work investigating the actual performance of procedures that involve a misspecification test for one or more model assumptions before running a method that is based on these model assumptions, looking at data that originally
fulfilled or did not fulfill the model assumption. Occasionally a combined procedure was investigated in which, in case of the violation of a model assumption, a method with lighter assumptions is used. The philosophy of such work, which is in line with frequentism-as-model, is that the key question is not whether the model assumptions are really fulfilled, but what the performance of such combined procedures is in several situations, compared to both the model-based and the less model-based (often nonparametric) test. The results are mixed, depend on the specific combinations of tests, and surprisingly many authors advise against misspecification testing based on their results (e.g., Fay and Proschán (2010): “The choice between t- and Wilcoxon-Mann-Whitney decision rules should not be based on a test of normality”). Whereas formally requiring lighter model assumptions, the use of nonparametric methods only pays off if the probability to arrive at a conclusion that is interpretationally in line with a modelled truth is better than for a competing parametric method for most models compatible with data and existing knowledge; traded off potentially against the expected sizes of “interpretational differences”, as far as these can be specified. This is not always the case.

Shamsudheen and Hennig (2019) showed a theoretical result that presents combined procedures in a somewhat more positive light. They looked at the overall performance of combined methods in a setup where datasets could be, with a certain probability \( \lambda \), generated by the assumed model, and with probability \( 1 - \lambda \) by a distribution that could cause trouble for the model-based method (see Section 5.2 for using such a “Bayesian” setup in connection with frequentism-as-model). Under certain assumptions for the involved methods they showed that for a range of values of \( \lambda \) the combined procedure beats both involved tests, the model-based one and the one not requiring the specific model assumption, regarding power, even if not winning for \( \lambda = 0 \) and \( \lambda = 1 \), which is what authors of previous work had investigated.

The surprisingly pessimistic assessment of misspecification testing by authors who investigated its effect may be due to the fact that most available misspecification tests test the model assumption against alternatives that are either easy to handle or very general, whereas little effort has been spent on developing tests that rule out specific violations of the model assumptions that are known to affect the performance of the model-based method strongly, i.e., leading to different conclusions for interpretationally equivalent models, or same conclusions for models that are interpretationally very different. Such tests would be specifically connected to the model-based method with which they are meant to be combined, and to the assessment of interpretational equivalence.

Furthermore, as already mentioned, not everything can be tested on data. E.g., many conceivable dependence structures do not lead to patterns that can be used to reject independence. For example, it may occasionally but not regularly happen that one observation determines or changes the distribution of the next one. Think of psychological tests in which sometimes a test person discusses the test with another participant who has not yet been tested, and where such communication can have a strong influence on the result.
Frequentism-as-model could inspire work that looks at performance of methods involving model assumptions and misspecification testing under all kinds of models that seem realistic, including Bayesian combinations of different models with different probabilities. Generally, regarding methodological research, a researcher adhering to frequentism-as-model will always be interested in the performance of method that are supposedly model-based under situations in which the model assumptions are violated, knowing that model assumptions can never be relied upon, and that looking at other models is the only way to investigate what happens then. This is in the tradition of robust statistics, but with less focus on worst cases and more focus on interpretational equivalence for deciding what outcome would actually be desirable. The cases that are really the worst ones are hopeless under any approach, and even the best statistics cannot always save the day.

5 Frequentism-as-model and Bayesian statistics

Most Bayesians interpret their probabilities epistemically, and this is incompatible with frequentism-as-model. This does not imply that the epistemic interpretation is in my view in any way wrong, but I do not agree with the claim of some, including de Finetti, that epistemic probabilities make frequentist probabilities superfluous, see Section 5.1. Frequentism-as-model as interpretation of probability is not committed to specific methodology such as tests and confidence intervals. It is also compatible with Bayesian methods, as long as they are interpreted accordingly, see Section 5.2.

The distinction between compatibility logic (i.e., asking whether certain probability models are compatible with the data as addressed by tests and confidence intervals) and Bayesian inverse probability logic, in which the central outcomes are the posterior probabilities resulting from conditioning the prior on the data, is not fully in line with the distinction between epistemic and aleatory probability, but currently a matter of hot debate. The 2016 ASA-Statement on p-values (Wasserstein and Lazar (2016)) has a single positive message on p-values, besides a number of negative ones: “p-values can indicate how incompatible the data are with a specified statistical model.” Wasserstein et al. (2019) go further and ask to “abandon statistical significance”, opening a Special Issue of The American Statistician containing a bewildering variety of alternative proposals. Many of the authors argue from an inverse probability logic, but this has problems that in my view are similarly severe. Section 5.3 compares the two logics.

5.1 Epistemic probability

The term “epistemic probability” refers to interpretations of probability that explain them as rational measures of uncertainty of a claim. They can roughly be distinguished in objectivist (logical) and subjectivist epistemic probabilities, respectively (see Gillies (2000); Galavotti (2005) for an overview). According to epistemic interpretations of probability, a priori existing probability assignments
are modified by the evidence. Mostly this is done using Bayes’s theorem, and some people identify epistemic probability with Bayesian statistics, although there are epistemic approaches that are not Bayesian, and Bayesian statistics can be combined with non-epistemic interpretations of probability, see Section 5.2.

The frequentist interpretation of probability is often rejected by advocates of epistemic probabilities for its obvious problems to establish an existence of frequentist probabilities in the observer-independent world. De Finetti and other advocates of subjectivist epistemic probability hold that whereas objective probability does not exist in the real world, degrees of belief of a person do exist, and are therefore more worthy targets of probability modelling. Given that I do not locate frequentist probabilities in the observer-independent world, why would I not go all the way to a subjectivist position? The reason is the following. Personal degrees of belief may exist or not. In any case, probability modelling is still modelling as discussed in Section 3.1. It operates on a domain different from what is modelled, and as there are issues with the connection of frequentist probabilities to real phenomena involving frequencies, there are issues of similar gravity with the connection of epistemic probabilities to subjective degrees of belief. I just mention two of them.

1. **Empirically observed behaviour of persons violates the probability axioms** (e.g., Kahneman et al. (1982)), so arguably epistemic probabilities do not model existing degrees of belief as far as they are observable. Defenders of epistemic probability argue that what empirical probabilities model is not how human behaviour is, but how degrees of uncertainty should be. But mixing probability axioms with otherwise unconstrained prior probability assessments produces a strange compromise of normative and empirical reasoning that looks artificially constructed rather than really existing in any conceivable sense. The only credible claim for existence is probably that a Bayesian researcher can say that she consciously adopts the resulting probabilities.

2. **As is the case with frequentist probabilities, epistemic probabilities are used in a simplifying and idealising way.** As elaborated in Section 2.2, frequentists need to rely on i.i.d. models not because they believe that the modelled process is really i.i.d., but rather in order to construct the kind of repetition that makes model-based learning from data possible. For the same reason, epistemic Bayesians normally rely on exchangeability (or a generalised concept for more complex situations, as was discussed for i.i.d. in Section 2.2). But when applied to real degrees of belief, the exchangeability assumption seems counterintuitive. In particular, once a process is assessed as exchangeable by an epistemic Bayesian, using standard Bayesian reasoning there is no way to learn anymore that the exchangeability assumption is not in line with the process to be modelled. I would think it rational, even if initially there is no reason to think that the order of observations matters regarding the probability of a sequence, to change that assessment if for example in
a binary process 50 ones, then 50 zeroes, then 50 ones, then 23 zeroes are observed. Surely this should convince the subjectivist that observing a zero next is now more likely than it was in the middle of a run of ones! But if initially runs were assessed to be exchangeable, this is not possible.

The point that I want to get across here is not that subjectivist epistemic probability involving exchangeability is in any way “wrong” or “useless”. In fact I am a pluralist, I can see areas where epistemic probability can be of use, and I accept the necessity of simplification and idealisation. The point is rather that most if not all criticism that subjectivists have about frequentism corresponds to trouble that exists within their own approach as well, which has to do with the fact that modelling does not match reality, be it aleatory or epistemic. Given that this is so, and given the obvious difficulty in many cases to choose a prior distribution, it looks attractive to model the reality of interest directly as frequentism-as-model does, rather than a degree of belief about it.

Objectivist epistemic probability is overall not in a better position than subjectivist epistemic probability. The issue with exchangeability is the same, except that subjectivists at least have a reference, namely the person holding the probability, who could take responsibility for either choosing exchangeability or for specifying a particular pattern deviating from it. I have not seen any discussion of objectivist epistemic modelling involving the ability to deviate from exchangeable assessments if the data provides strong evidence against it.

Another advantage that subjectivists have over epistemic objectivists is that they are allowed to formalise existing but informal evidence as they see fit, whereas it is unclear how epistemic objectivists could incorporate it. This corresponds to the open license that frequentism-as-model grants the researcher to incorporate subjective assessments of the situation in their models compared to traditional frequentists or propensity theorists. In practice almost everyone does it, but many do not admit it.

Overall I think that frequentism-as-model has something relevant to offer that is not covered by the major streams of epistemic probability, and that for major arguments that critics have against frequentism, corresponding arguments exist against epistemic probability. I do not deny that epistemic probability has its uses, but frequentism-as-model treats processes that the researchers think of as “random” more directly, avoiding the thorny if sometimes useful issue of specifying and justifying a prior.

5.2 Frequentism-as-model and falsificationist Bayes

Epistemic probabilities model a personal or “objective” degree of belief, not the data generating process as such, and therefore they cannot be checked against and falsified by the data. See Dawid (1982) for a discussion of “calibration”, i.e., agreement or potential mismatch between predictions based on epistemic Bayesian probabilities and what is actually observed. Gelman and Shalizi (2013) argued that Bayesian statistics should allow for checking the model against the data,
and interpret Bayesian models as modelling data generating processes rather than epistemic uncertainty. This is in line with a lot of applied Bayesian work in which the posterior distribution of the parameter is interpreted as encoding probabilities for a certain parameters being true descriptors of the underlying data generating process. This allows to check the model against the data and potentially to revise it. Gelman and Hennig (2017) called it “falsificationist Bayes”.

My interpretation of it is that probabilities in falsificationist Bayes are interpreted in the same manner as in frequentism-as-model. The parametric model is handled as if it describes the data generating process but can be dropped or modified if falsified by the data. The involved interpretation of probabilities is consistent if the parameter prior is interpreted as a model of a parameter generating process in the same way. Gelman and Shalizi (2013) stated that the prior distribution may encode “a priori knowledge” or a “subjective degree of belief”. This seems to mix up an epistemic interpretation of the parameter prior with an aleatory interpretation of the parametric model, and it is hard to justify using them in the same calculus. I believe that it would be better to refer to the parameter prior as an idealistic model of a process that generates parameters for different situations that are based on the same information, i.e., to interpret it in a frequentism-as-model way. This is in line with the fact that Gelman in presentations sometimes informally refers to the parameter prior as a distribution over parameters realistic in a distribution of different situations of similar kind in which datasets can be drawn. This is a very idealistic concept and it is probably hard to connect the setup of parameter generation precisely to real observations. The modelling will normally indeed rely more on belief and informal knowledge than on observation of replicates of what is supposed to be parameter generation, but as in Section 2.1 an arbitrary amount of data can be generated from the fully specified model, and can be compared with the observed data. Testing the parameter prior is hard. In a standard simple Bayesian setup, it is assumed that only one parameter value generated all observed data, so the effective sample size for checking the parameter prior is smaller than one, because the single parameter is not even precisely observed. Therefore sensitivity against prior specification will always be a concern, but see the discussion of single-case probabilities in Section 2.2. In any case, the parametric model can be tested in frequentist ways. In case the parameter prior encodes valuable information about the parameter that can most suitably be encoded in this way, Bayesian reasoning based on such a model is clearly useful, and open to self-correction by falsificationist logic. As for model assumptions testing followed by traditional frequentism methods (Shamsudheen and Hennig (2019)), it may be of interest to analyse the behaviour of Bayesian reasoning conditionally on model checking in case of fulfilled and not fulfilled model assumptions.

A benefit of such an interpretation could be that the prior no longer either has to be claimed to be objective or to model a specific person. It is a not necessarily unique researcher’s suggestion how to imagine the parameter generating process based on a certain amount of information, and can as such be compared with alternatives and potentially rejected, if not by the data, then by open dis-
discussion. As earlier, stability against different prior choices compatible with the same information can be investigated. Models may be set up not only to formalise most realistic processes, but also, depending on the application, worst or best case scenarios in order to explore what range of possibilities this generates. Such things are occasionally already done (see the Shamsudheen and Hennig (2019) example in Section 4.3), but currently such nonstandard Bayesian practice does not seem in line with the predominantly epistemic Bayesian philosophy. Frequentism-as-model is in my view a more fitting philosophy for such reasoning.

5.3 Compatibility logic vs. inverse probability logic

We have seen that the distinction between a frequentist and an epistemic interpretation of probability does not align perfectly with the distinction between compatibility logic and Bayesian inverse probability logic. Often posterior probabilities are interpreted as probabilities about where to find the true parameter value. The idea of a true parameter value is a traditional frequentist one. de Finetti (1974) argued that posteriors should be interpreted regarding observable quantities such as future observations and not regarding unobservables such as true parameter values that may well not exist, but according to Diaconis and Skyrms (2018), de Finetti’s Theorem implies that for a subjectivist, belief in exchangeability or a suitable generalisation of it implies belief in the existence of limiting relative frequencies, and therefore a limiting probability distribution that can be parametrised. This could be used to connect epistemic probability with compatibility logic, but advocates of epistemic probability do not seem to be very interested in this. In any case, a falsificationist Bayes perspective combined with a frequentism-as-model interpretation of probability licenses probabilistic statements about the parameter modelled as true.

A major difference between compatibility logic and inverse probability logic is that according to compatibility logic many models can be compatible with the data, and the compatibility of one model does not exclude or reduce the compatibility of another model. Inverse probability logic distributes an overall probability of one over the models modelled as possible, implying that a higher probability for one model automatically decreases the probability for the others. The models are competing for probability, so to say.

There are advantages and disadvantages of both approaches. Many Bayesians such as Diaconis and Skyrms (2013) have pointed out that p-values and confidence levels are regularly misinterpreted as probabilities regarding the true parameter, because these should be the ultimate quantities of interest in statistical inference, or so it is claimed. Inverse probability logic deals with combining different inferences such as multiple testing, which creates trouble for standard compatibility logic approaches, in a unified and coherent way. Frequentists are not only interested in compatibility, but also in estimation; finding a best model amounts to a competition between models. A Bayesian can argue that in this case a probability distribution over parameters provides better information about how the parame-
ters compare and to what extent one parameter value is more likely than others. Using confidence distributions (Xie and Singh (2013)), frequentists involving compatibility logic can give more detailed information about how parameters compare as well, but Bayesians hold that a proper probability distribution is more intuitive and less prone to misinterpretation.

On the other side, by allowing many models to be compatible with the data at the same time, compatibility logic is more obviously in line with the attitude that models are idealisations and not really true. Davies (1995) argued that if many models are valid approximations for the same real situation, inverse probability logic is inappropriate, because if for example $\mathcal{N}(0, 1)$ is a reasonable approximation of the truth, $\mathcal{N}(10^{-10}, 1)$ is a reasonable approximation as well, whereas according to inverse probability logic any two models compete for a part of the unit probability mass.

Furthermore, it may seem unfair to criticise tests and confidence intervals based on misinterpretations. Arguably many users do not only want to know the probability for certain parameter values to be true, which indeed tempts them to misinterpret confidence levels and p-values. But arguably they also want this probability to be objective and independent of prior assessments, which to make up they have a hard time. This combination is not licensed by any properly understood philosophy of statistics, and ultimately statisticians need to accept that their job is often not to give the users what they want, but rather to defy wrong expectations.

The role of the prior distribution in inverse probability logic is a major distinction between the two approaches. Bayesians argue that the prior is a good and very useful vehicle to incorporate prior information. Actually prior information enters frequentist modelling as well (see earlier sections), but the Bayesian prior is still an additional tool on top of the options that frequentists have to involve information. But the requirement to set up a prior can also be seen as a major problem with inverse probability logic, given that prior information does not normally come in the form of prior probabilities, and that it is actually in most cases very difficult to translate existing information into the required form. It is not an accident that a very large number of applied Bayesian publications come with no or very scarce subject matter justification of the prior, and in most cases the prior information is very clearly compatible with many different potential priors, with comprehensive sensitivity analysis rarely done. If the sample size is large enough for the prior to lose most of its influence, one may wonder why to bother having one. The question whether there is prior information that is meant to have an impact on the analysis and can be encoded convincingly in the form of a prior distribution is a key issue for deciding whether inverse probability or compatibility logic will be more promising in a given application of statistics.

Falsificationist Bayes combines the two by applying inverse probability logic within a Bayesian model, of which the compatibility with the data should also be investigated. Gelman et al. (1996) emphasise that the posterior distribution of the parameter is conditional on the truth of the model, and according to frequentism-as-model a researcher can interpret results temporarily as if this were the case,
without ignoring that this is just a thought construct and that many other models are compatible as well. This differs from the epistemic probability interpretation, where the “truth of the model” is not a matter of mechanisms in the real world, but rather of the degrees of belief of the analyst, and the possibility that the analyst’s degrees of belief are not properly reflected by the model is rarely discussed.

Frequentism-as-model allows to interpret the parameter prior in falsificationist Bayes in a way that requires neither to mix epistemic and frequentist meanings of probability, nor to demand that the prior corresponds to an in principle infinitely repeatable data generating process. The Achilles heel of this is that in a standard situation, based on a single realisation that is not even directly observable, the potential to check the parameter prior against the data is very weak. The prior still needs to be convincingly defended in other ways, and sensitivity analysis is certainly desirable.

6 Conclusion

It may seem to be my core message that models are models and as such different from reality. This is of course commonplace, and agreed by many if not all statisticians, although it rarely influences applied statistical analyses or even discussions about the foundations of statistics.

Here are some less obvious implications:

- The usual way of talking about model assumptions, namely that they “have to be fulfilled”, is misleading. The aim of model assumption checking is not to make sure that they are fulfilled, but rather to rule out issues that misguide the interpretation of the results. Combining model assumption checking and analyses chosen conditionally on the model checking results can itself be modelled and analysed, and depending on what exactly is done, it may or may not turn out to work well.

- There are always lots of models compatible with the data. Some of these can be favoured or excluded by plausibility considerations, prior information, or by the data, but some irregular ones have to be excluded simply because inference would be hopeless if they were correct.

- Model assessment based on the data involves decisions about “in what way to look”, i.e., what model deviations are relevant. Whether a model is compatible with the data cannot be decided independently of such considerations.

- Choosing a model implies decisions to ignore certain aspects of reality, e.g., differences between the conditions under which observations modelled as i.i.d. were gathered. These decisions should be transparent and open for discussion.

- Consideration of interpretational equivalence, i.e., what different models would be interpreted in the same or different way regarding the subject
matter, are important in order to investigate robustness and stability, i.e., to what extent different models compatible with the data would lead to results on the same data that have a different meaning.

- Frequentism-as-model is compatible with both compatibility and inverse probability logic. A key to decide which one to prefer is to ask whether the parameter prior distribution required for inverse probability logic can be used to add valuable information in a convincing way. The parameter prior itself can not normally be checked against the data with satisfactory power.

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