# Objectivity and the Method of Arbitrary Functions

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**Abstract.** There is widespread excitement in the literature about the method of arbitrary functions: many take it to show that it is from the dynamics of systems that the objectivity of probabilities emerge. In this paper, I differentiate three ways in which a probability function might be objective, and I argue that the method of arbitrary functions cannot help us show that dynamics objectivise probabilities in any of these senses.

## 0. Introduction

Probabilities are ubiquitous in a number of special sciences, including statistical mechanics, population genetics, and many social sciences. Within these sciences, they seem to play a crucial role in the explanation and the prediction of empirical phenomena, and to provide constraints on what we ought to believe about these phenomena. This has motivated many to think that they must be *objective*, in some sense to be made precise. Are they, and if so, in what sense? There is a vast literature on these topics, a subset of which concerns what is known as the *method of arbitrary functions*.[1] This method will be my central concern in this paper.

The method of arbitrary functions has attracted a lot of attention, because it is widely believed to provide a reason for thinking that (at least some of) the probabilities in the special sciences are indeed objective, in virtue of the dynamics of the systems under study. More precisely, many take it to show that the dynamics of a system can serve to *objectivise* an underlying probability function; or in other words, that the objectivity of the probabilities can emerge from the dynamics of the system. Strevens (2011) for instance claims that the method allows us to

> ...find a basis for an outcome's [objective] probability in the properties of the physical dynamics that produce it. (p. 340)

[1] The method originates with von Kries (1886) and Poincaré (1896). For a history of the method, see von Plato (1983).

According to Myrvold (2016), the key idea of the method is that

> ...a probability distribution over initial conditions is transformed, via the dynamical evolution of the system, into a probability distribution [which is] an epistemic chance. (p. 592)

For Gallow (forthcoming),

> ...the chance distribution over outcomes is the result of time-evolving a suitable probability distribution along a suitable dynamical equation. [...] In a slogan: chance is a suitable probability distribution filtered through suitable dynamics.

All these authors agree: the method of arbitrary functions shows us that the objectivity (or chanciness) of a probability function emerges out of the system's dynamics. My aim in this paper is to argue that the method of arbitrary functions can do no such thing.

My argument begins by differentiating three ways in which a probability function might be thought to be objective. A probability function might be ontically (as opposed to epistemically) interpreted, if it represents a mind-independent phenomenon. If it is interpreted as a credence function, it might be objectively (as opposed to subjectively) evaluable, if disagreement about its values entail fault. Finally, it might be high-level robust (as opposed to chaotic) if the values it assigns to high-level phenomena do not depend much on the values it assigns to low-level phenomena.[2] In this paper, I show that authors in the literature on the method of arbitrary functions have sought to use the method to show that the dynamics of a system can establish the objectivity of probability functions in each of these senses. I then show that this is a mistake: the method does not show that the objectivity of probability functions stems from the system's dynamics, in any of the senses of objectivity that can be found in the relevant literature.

Let me make my thesis more precise. What I will argue is that, to the extent that the aforementioned probabilities are objective, they are not so in virtue of dynamics *qua* dynamics. Thus my argument will be consistent with the claim that the probabilities in question are objective; and it will even be consistent with the claim that the method of arbitrary functions can shed some light as to why they are objective (though for this latter claim, not in all senses of *objective*). But, I will show, this has nothing in particular to do with dynamics, rather at best, dynamics are an instance of a much more general way in which the probabilities in question could be objectivised. Thus my claim will be that the authors are wrong to think that the basis for the objectivity of probabilities lies in the dynamics (or mechanics) of

[2] There is also another sense of objectivity, according to which something is objective if it is perspective-invariant; if it is "the same from all angles". This sense of objectivity has been discussed in the literature on objectivity in general, as well as in the literature on the objectivity of probabilities in the form of the reference class problem. However, as far as I'm aware, it has not been discussed in the literature on arbitrary functions. Nonetheless I discuss it briefly in §4, where I show that the method does not establish the objectivity of probabilities in the sense of addressing the reference class problem.

the systems of study. It follows that, if authors are to argue that they are indeed objective, in any of the senses I have identified, they cannot appeal to the dynamics of the system in question.

My strategy is as follows. I begin by outlining the method in some detail (§1). Then, I take each of the senses of objectivity in turn. My strategy is the same for each of them. I begin by giving a fuller account of the sense of objectivity at hand. Then, I show that at least some authors in the literature believe the method of arbitrary functions to show that it is the system's dynamics that establish the objectivity of probabilities in that sense. Finally, I argue that they are wrong. To the extent that probabilities in the sciences are ontically interpreted (§2), objectively evaluable (§3), and high-level robust (§4), they are not so in virtue of the dynamics of the system. I conclude the paper by noting that, even though the method is inert when it comes to the objectivisation of probabilities through dynamics, it nonetheless has philosophical interest: I explain how (§5).

## 1. The Method of Arbitrary Functions

In this section, I outline the method of arbitrary functions. Suppose that you are faced with a wheel, painted with alternating black and red wedges of equal size, equipped with a stationary pointer. The wheel may be spun, and allowed to come at a stop with the pointer indicating either red or black. Suppose further that the mechanics of the wheel are deterministic, and that the outcome of a given trial is fixed by a single parameter, the initial speed of the wheel.

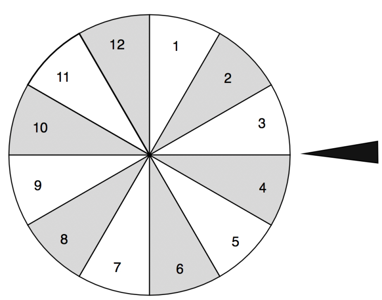


Figure 1: A roulette wheel

Let us represent this setup more precisely. Let the set $X$ represent the set of all initial speeds, the set $Y$ be the set of all the wedges on the roulette wheel,[3] and the set $Z$ be the set of all outcomes of interest—here, black and red. Now, let us define a surjective function $f : X \to Y$, which maps every initial speed to its associated outcome on the roulette wheel.[4] Next, we define a surjective function

[3] Alternatively, we could represent the setup with a set representing the wheel's possible angles of rotation.

[4] This map is a function because the dynamics of the wheel are deterministic: given that the wheel is spun at a particular initial speed, there is only one wedge that it could land on. This suggests that, in the event that someone wanted to extend the method of arbitrary functions to non-deterministic sciences, such as

$g : Y \to Z$, which maps every wedge of the wheel to its colour. Finally, we define a surjective function $h = g \circ f : X \to Z$, which maps initial speeds to the colour of the wedge that it yields. Note that these functions allow each subset of $Y$ and $Z$ to be associated with a subset of $X$, namely its preimage. For instance, $f$ associates $\{y_1\}$, the first wedge, with the set of all initial speeds that yield a landing on the first wedge, which we can call $X_1$.

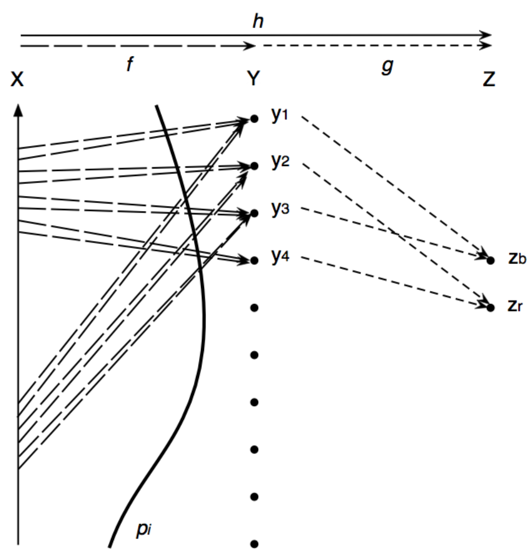


Figure 2: The setup

Now, we can introduce probabilities. Let us define a probability function $p_i$ on (a $\sigma$-algebra of) $X$, to give us the probabilities of (sets of) initial speeds. (I leave questions of interpretation to §2.) Note that this probability function will give us the probability of any measurable subset of $X$. So for instance, it will give us the probability of $X_1$, the set of all initial speeds that yield a landing on the first wedge; and the probability of $X_B$, the set of all initial speeds that yield a landing on a black wedge.[5] Note that, strictly speaking, this does not give us the probability of landing on a specific wedge, or the probability of landing on a wedge of a specific colour: it gives us the probability of the wheel being spun with an initial speed such that it will land on a specific wedge or colour. Finally, note that strictly speaking, the probability function $p_i$ does not ascribe values to individual initial speeds: it is its associated probability density function that does.[6] However, for economy of expression, I will

quantum mechanics, some amendments would have to be made. I leave this aside, though: along the other authors writing about the method of arbitrary functions, I concentrate on the probabilities that feature in the deterministic sciences.

[5] We are assuming that $f$ and $h$ are random variables; that is, that the preimages of elements of the algebras over $Y$ and $Z$ are measurable in the algebra over $X$.

[6] Indeed the probability density function is defined on elements of $X$, that is, over $X$; and its associated probability function $p_i$ is defined on measurable subsets of $X$, that is, over a $\sigma$-algebra of $X$. So $p_i$ takes *sets*

obscure this distinction in the rest of this paper and sometimes talk of $p_i$ as taking initial speeds as its argument—talking for instance of "the probability of an initial speed" or of "the probability function over initial speeds".[7]

We arrive at the method. The basic idea is that, from two relatively weak assumptions, we can derive that the probability of $X_B$ is roughly equal to that of $X_R$. The first assumption **(a)** concerns the mechanics of the system; that is, the shape of the functions $f$, $g$, and $h$. It can be divided into two parts. **(a$_1$)** We assume that $f^{-1}$, the inverse image of $f$, partitions $X$ into intervals such that velocities in adjacent intervals are mapped to adjacent wedges: the resulting partition is what Butterfield (2011) calls "filamentous" (p. 1083). Roughly, if you slightly increase the initial speed by which the wheel is spun, the wheel will land on the following wedge. **(a$_2$)** Furthermore, in sufficiently small (but not too small) regions of initial speeds, the relative size of the set of initial speeds mapped by $h$ onto the red outcome with respect to that of the set mapped by $h$ onto the black outcome is roughly constant: the system has a property which Strevens (2011) calls "microconstancy" (p. 346). In our roulette wheel, the ratio is $1/2$. The second assumption **(b)** concerns the probability function $p_i$ over sets of initial speeds. We assume that it is such that its associated density function does not vary too quickly, such that two very similar initial speeds get roughly the same probability density.

If these two assumptions are satisfied, it can be shown that the probability of $X_B$ is roughly equal to that of $X_R$.[8] This is illustrated by figure **3** below. Given a setup like that of figure **2**, which respects assumption **(a)**, take any function (that is, take an ***arbitrary function***) $p_i$ that respects assumption **(b)**, and it follows that $p_i(X_B) \approx p_i(X_R)$. This result is what is usually referred to as the "method of arbitrary functions". (I will sometimes speak as though $p_i(X_B)$ and $p_i(X_R)$ were equal, as opposed to roughly equal. This is merely for economy of expression—they are not.)

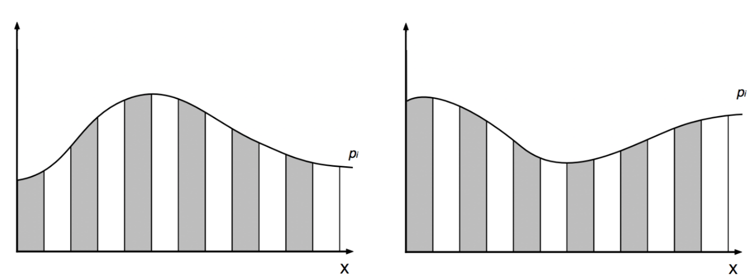


Figure 3: Two different probability functions over $\mathcal{A}$.

You can see that the two assumptions **(a)** and **(b)** pull in opposite directions. The more "filamentous"

of initial speeds as its argument, not initial speeds themselves.

[7] In addition, I have labelled the probability density function in figures **2**–**4**, "$p_i$". Again this is not strictly correct because elsewhere I take "$p_i$" to refer to its associated probability function. Nonetheless I remain loose for economy of expression.

[8] The initial proof appears in Poincaré (1896). See Butterfield (2011) for an overview of the technical results, and Engel (1992) for a more in-depth (and difficult!) exposition.

the partition of $X$ is, the more "wiggly" $p_i$ can be (see figure **4.1**). Conversely, the less "wiggly" $p_i$ is, the less "filamentous" the partition needs to be (see figure **4.2**).

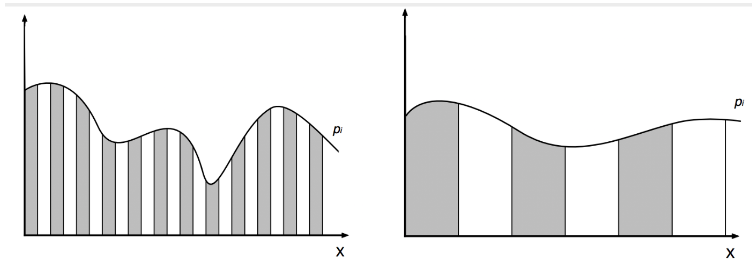


Figure 4: Assumptions **(a)** and **(b)** pull in different directions.

With a grasp of the method of arbitrary functions, we are in a position to ask: what is the philosophical significance of this result? In particular, can this result help in establishing the objectivity of probabilities in the special sciences?

## 2. Objectivity as Ontic Interpretation

In this section, I consider whether the method of arbitrary functions can help establish that some probabilities in the sciences are objective, in the sense that they are *ontically interpreted.*

What is objectivity in this sense? I begin by spelling out what an interpretation is. Imagine that there are two people in a room. We might represent these two people using a set, $A = \{a_1, a_2\}$, each element standing for a person. Now, suppose that we are interested in the height of these people in centimetres. We might represent that using a function $h : A \to \mathbb{R}$, which assigns a real number to each element of $A$, giving the height in centimetres of each person represented as an element of $A$. In that case, we say that the function $h$ *represents* the height of the two persons. Conversely, imagine that one is presented with a function $h : A \to \mathbb{R}$. On its own, $h$ is a purely mathematical object, and does not describe anything in the world. However, we might *interpret* that function. What that means, is that we specify what the elements of the set $A$ on which $h$ is defined represent, and what $h$ says of them. Interpreting $h$ as a height function thus requires identifying each element of the set $A$ with a person in the room, and understanding $h$ as giving the height in centimetres of each of those people. Understood in this way, representation and interpretation are *duals.* Give me a fact and I can represent it using a function, give me a function and I can interpret it as expressing a fact.[9]

We can divide interpretations of functions in two groups: the *epistemic* ones, and the *ontic* ones. We say that an interpretation is epistemic just in case the function under that interpretation describes an agent's epistemic, intentional attitude.[10] We say by contrast that it is ontic if a function under

[9] This formulation is awkward, but the hope however is that this snappy phrase, problematic as it may be, gives the reader some initial insight into what I take interpretation to be.

[10] Technically, there may be interpretations of probability that represent intentional attitudes that are not epi-

it describes something non-epistemic; that is, mind-independent. A snappy way to think about this distinction is that ontically interpreted functions are about the external world, whereas epistemically interpreted functions are (at least partly) about the mind. To illustrate, take the function $h : A \to \mathbb{R}$. The height interpretation of this function described above is ontic: the height of the people in the room is unconnected to anyone's epistemic attitudes. By contrast, an interpretation of the function $h$ as the height that some agent Alice believes the people in the room to have is epistemic.

Like other functions, probability functions can be used to describe the world, and as such, can be interpreted. The question of interpretation is that of what these functions represent in the world. So, take a probability function $p : \mathcal{A} \to \mathbb{R}$, where $\mathcal{A} = \{\Omega, \{H\}, \{T\}, \emptyset\}$ and $p(\{H\}) = p(\{T\}) = 1/2$. What it means to interpret this function, is to specify what the elements of $\mathcal{A}$ represent, and what $p$ says of them. One way to interpret this function is as follows. A coin has been flipped ten times. The elements of the algebra correspond to the different possible outcomes of the coin flip: coin lands heads, and coin lands tails (and the trivial events). The probability function $p$ describes the proportion (that is, the finite frequency) of these outcomes having obtained. Namely, it says that the coin has landed half the time on heads and half the time on tails in this sequence of flips. Here, $p$ has been ontically interpreted. Another way to interpret $p$ is as follows. A coin is about to be flipped, and Alice is wondering how it will land. The elements of the algebra correspond to the propositions that the coin will land heads, and that the coin will land tails (and the trivial propositions). The probability function $p$ describes the confidence Alice has in the truth of each such proposition: it says that she is equally confident that the coin will land heads and tails. We say that $p$ represents her ***degrees of belief***, or ***credences***, in these propositions; so here, it has been epistemically interpreted.

It is customary in the literature to call the ontic interpretations of probability "objective",[11] and the epistemic ones "subjective".[12] Many think that probabilities in the sciences should be objective in this sense: the (physical) sciences describe the world and not our minds. Moreover, these probabilities play an explanatory role: they are used for instance in our best statistical mechanical explanation of why milk dissolves in coffee. But they could not play this role, the thought goes, if they described the mind and not the (mind-independent) system at hand.[13] Can the method of arbitrary functions help establish that probabilities are objective in this sense? Many in the literature think that it can. Some claim this directly. Abrams (2012) purports to use the method of arbitrary functions to "outline a new

stemic. For instance, we can imagine a probability function interpreted as giving an agent's degree of desire that a proposition be true. Thus not all possible mind-dependent interpretations are subjective. But I don't think that these are relevant to the matter at hand; so I leave them aside.

[11] A number of different objective interpretations of probability are discussed in the literature, including finite frequency (as above), hypothetical frequency, propensity, and indeterministic chance. For an overview, see Hájek (2019). Since my point applies to all interpretations indiscriminately, I will not discuss their particular characteristics in any detail.

[12] The main subjective interpretation is the Bayesian one, described above.

[13] For a forceful defence of this point, see Albert (2000).

interpretation of probability, one which is objective in the sense that the resulting probabilities are constituted only by facts about states of the world, without regard to epistemic factors such as belief or justification" (p. 344). Rosenthal (2012) argues that the method can "yield an objective interpretation of probability in the sense of providing truth conditions for probability statements that are independent of our state of mind and our state of information" (p. 225). Werndl (2015) argues that the method "shows how stable ontic probabilities (i.e., probabilities that are real features of the world) can arise out of deterministic equations" (p. 223).

But it is interesting to note that many other authors are implicitly committed to this view. Central to the debate about the compatibility of determinism and objective probability[14] is a sceptical argument; roughly: what can it mean to say that the objective probability of a coin landing heads is $1/2$, if it is already determined by the laws of nature that it will? The sense of objectivity that is relevant here is that of ontic interpretation. Indeed, the question is that of what these probabilities could represent that is not epistemic.[15] So, to think that progress can been made in the compatibilist camp amounts to thinking that progress can be made in devising an ontic interpretation of probability. And indeed, many mention the method of arbitrary functions in connection with the compatibilism debate, including Butterfield (2011), Frigg (2016), **?**, and Hájek (2019). But if I am right that the method can provide no help when it comes to ontically interpreting probabilities, these authors are mistaken in thinking that there is a connection between the method and the compatibilism debate.

Am I right, then, in thinking that the method can provide no such help? Or could it in fact at least make progress on establishing an ontic interpretation of the claim that red and black outcomes of a wheel spin are equally likely? To start, note that the method is really a mathematical theorem. So, in order for it to say anything about the world (and thus have philosophical significance), the mathematical objects that comprise it must be interpreted. The functions $f$, $g$, and $h$ are interpreted straightforwardly, as representing the dynamics and structure of the roulette wheel. But what about $p_i$? I will not answer this question here. Rather, I will simply note that whichever interpretation is supplied to $p_i$ is exogenous to the method of arbitrary functions; and furthermore that $p_i$ is the only probability function mentioned throughout. It follows that the method is completely inert, as far as interpretation is concerned: it cannot help establish the objectivity of the equiprobability of red and black outcomes. This is all the more the case given that the method does not even make claims about the probabilities of red and black outcomes themselves: instead, it makes claims about the probabilities of sets of initial speeds that will yield these outcomes. To illustrate, let us assume that $p_i$ is interpreted as giving frequencies. The method then says that, given **(a)** the mechanics and structure of the wheel, and **(b)** that the frequencies of initial speeds do not vary too quickly, the sets of initial speeds that correspond to red and black outcomes occur with roughly equal frequency. It should be clear that the

[14] For an overview, see Frigg (2016).

[15] Note for instance that there is no tension between determinism and objective Bayesianism (see §3 for a discussion of this sense of objectivity).

method has played no interpretive role.

The three authors cited in §0 believe that, contrary to what I have said so far, the method of arbitrary functions does make claims about the probability of landing on a red wedge; as opposed to merely making claims about the probability of the initial speed being such that the wheel will land on a red wedge. In order for this be true, the method must be supplemented. A new probability function, call it $P$, must be defined on (a $\sigma$-algebra over) $Z$, ascribing probabilities to the two possible colour outcomes of wheel spins, red and black. The method of arbitrary functions must then be connected to this new function by fixing its values. What we want is for the probability of landing on a black wedge to be equal to the probability of the wheel having been spun with a black-yielding initial speed; that is $P(z_B) = p_i(X_B)$, and similarly for the red outcome. We now have a second probability function, whose interpretation is not exogenously fixed. Could the method so-supplemented then help with interpretive projects?

There are two options. The first is that $P$ is given the same interpretation as $p_i$; more precisely, $P$ is stipulated to say of $z_B$ what $p_i$ says of $X_B$. So, on this option and assuming that $p_i$ was interpreted as giving frequencies, the (supplemented) method of arbitrary functions tells us that, if the frequencies of initial speeds do not vary too quickly, then the two colour outcomes occur with equal frequency. But the method has done no interpretative work here: the interpretation of $p_i$ was supplied exogenously, and the interpretation of $P$ was simply stipulated to be identical. This leaves us with the second option, according to which $P$ is not stipulated to have the same interpretation as $p_i$. But what interpretation does it then have? The method has fixed its value but has said nothing of its interpretation; which would again have to be supplied exogenously. So, either way, the method of arbitrary function has been of no interpretative help.

In sum, the method of arbitrary functions cannot help establish the objectivity in this first sense of probability distributions over outcomes; that is, it cannot help establish that they can be ontically interpreted. It follows, as I explained earlier in this section, that it is irrelevant to the compatibilism debate.

## 3. Objectivity as Correct Evaluability

In this section, I consider whether the method of arbitrary functions can help establish that some probabilities in the sciences are objective, in the sense that they are *objectively evaluable*, on the basis of the system's dynamics.

What is objectivity in this sense? Consider two agents, Beth and Carl. They are both considering the proposition that a particular spin of our roulette wheel will yield a red outcome, and they possess the same evidence. Suppose that they disagree, which is to say that they have incompatible credences in this proposition: Beth's is 1/3, and Carl's is 1/2.[16] Can they simply agree to disagree, or must it

[16] I am assuming throughout that credences are probabilistic: I have defined them as epistemically interpreted

instead be that at least one of them is wrong? In particular, is it the case that Carl is right, that $1/2$ is in some sense the objectively correct credence to have in this proposition?

This question has been thoroughly debated. Some think that there is no single objectively correct credence to have in this proposition; among them are the *Radical Subjective Bayesians*, according to whom every credence function over an algebra of propositions is as good as any other.[17] Others disagree. This includes the *Objective Bayesians*, who think that, given a fixed body of evidence, there is a unique objectively correct credence function—which one is a matter of dispute, though a Principle of Indifference, recommending a uniform distribution over elementary propositions, is often invoked.[18] It also includes the proponents of bridge principles between objectively interpreted probabilities and credences, according to whom credences about objectively probabilistic propositions should correspond to the objective probabilities of the events, though credences in other propositions are unconstrained. The classic chance-credence bridge principle is Lewis' Principal Principle (1980); and the classic frequency-credence bridge principle is proposed by van Fraassen (1983). So according to the proponent of the Principal Principal, all credences in chancy propositions are objectively evaluable, and all credences in non-chancy propositions are only subjectively evaluable.

This suggests a second sense in which a probability function can be objective, based on how much disagreement is allowed between agents. Note firstly that this sense of objectivity applies to epistemic attitudes, and as such, only applies to probabilities interpreted as credences (that is, probabilities that are subjective in the sense discussed in §2). Note secondly that this sense of objectivity is not an all-or-nothing affair. The subjectivist thinks that credences are less objective than what the proponent of the Principal Principle thinks. In turn, the proponent of the Principal Principle thinks that credences are less objective than what the objectivist thinks. Thus we arrive at a more precise definition of this sense of objectivity. Let us say that credences are increasingly *objectively evaluable* as they are subject to increasingly stringent norms of correctness, or in other words, as they allow for a decreasing amount of disagreement. Let us say, derivatively, that norms prescribe certain credences as *objectively correct*, when they dictate that disagreement in these credences entail that at least one agent is wrong. Thus the more objectively correct credences a (true) norm dictates there are, the more objectively evaluable will credences be. Let us illustrate this. According to the objectivist, credences are maximally objectively evaluable: any disagreement between agents entails that at least one has an objectively incorrect credence. According to the proponent of the Principal Principle, credences are somewhat objectively evaluable: disagreement about chancy propositions is disallowed, but other disagreement is allowed. And according to the radical subjectivist, credences are not objectively evaluable at all: all disagreement

probability functions. I am also assuming of course that the agent has no direct observational evidence about the outcome of the spin; so these credences may be thought of as priors. Finally, I am assuming that the agent has precise (as opposed to imprecise) credences: we are dealing with a single probability function, and not a set thereof.

[17] This tradition begins with de Finetti (1937).

[18] For a prominent defence of objective Bayesianism, see Williamson (2010).

is allowed.

A clarification must be made about what is meant by a credence being "correct" or a disagreement being "allowed". Let us take the Principal Principle as an example. I have said above that it establishes (if true) that credences towards chance propositions are objectively evaluable, or that disagreement about these propositions is not allowed. This does not entail that agents are irrational if they have the incorrect credence, or if they disagree. For suppose that the chance of some proposition $A$ is $1/2$, but that neither of two agents know this. Suppose furthermore that both of them have perfectly rational credences, in the sense that they have perfectly conditionalised their priors on their available evidence. In this case, the two agents are (internally) rational. But this does not entail that they are correct, given the norm established by the Principal Principle. For according to that norm, if one of them has a credence in $A$ that is not $1/2$, that agent has an objectively incorrect credence; that is, they have a credence that does not correspond to the chance. So, agents cannot simply "agree to disagree": if they have different credences about $A$, at least one of them has an incorrect credence.[19] Note that we can know that credences are objectively evaluable, without knowing which is objectively correct. If we know that the Principal Principle is true, and that $A$ is a chancy proposition, we know that there is a correct credence to have about $A$, though we might not know which one, if we do not know what the chance of $A$ is. To conclude on this point: objective evaluability is not a matter of (internal) rationality;[20] rather, it is a matter of there being (externally) correct credences.

What would it take for the method of arbitrary functions to show that dynamics objectivise probabilities, in this sense of objective? It would require for the method to show that the system's dynamics provide a reason for thinking that credences are objectively evaluable; that is, that some credal values in some propositions are objectively correct. In the case we have been considering, it would require providing a reason for thinking that Carl has the objectively correct credence. Can the method show that the dynamics of a system provide a reason for $1/2$ being the correct credence to have in the wheel stopping on heads? There are two ways they could do this. The first is that they could help establish that the (mind-independent) chance or frequency of heads is $1/2$. Together with a bridge principle, it would follow that an agent's credence in heads ought to be $1/2$. But there is a sense in which the dynamics themselves would not directly be constraining credences: they would be operating on frequencies/chances, and the bridge principle would do the work of credence constraint. They would not establish a new norm of objectivity, but would instead feed into an already existing norm. I will discuss

[19] Suppose that Jean's credence in $A$ is $1/2$ and that Joan's is $26/50$. It may be that, for all practical purposes, their disagreement does not matter, in the way that it would if their credences were very far apart. But this is not the relevant sense for our purposes: what matters is that they cannot both be strictly correct.

[20] Nor is it a matter of practical concerns. There may be some very good reasons for scientists to adopt knowingly incorrect credences, for instance if they are close-enough for all practical purposes and more easily tractable. So it may be that the agent should adopt these credences, but that does not make them correct in the sense I have been describing: they should adopt them, *despite* the fact that they are incorrect, because they have other virtues (such as tractability) which are practically more important in that context.

the role of the method in constraining the values of objectively interpreted probability functions in §4, and the consequences this has for the constraining of credences in §5; but in this section I concentrate on the direct objectivisation of credences.

This brings us to the second option, namely that the method could somehow show that the dynamics of the wheel constrain credences directly. Some have thought that it could do this; that is, that the dynamics of the wheel could generate a reason to think that a credence of $1/2$ in red is objectively correct. This would be attractive, because it would supply a validation, rooted in empirical features of the world, of the intuition shared by many that agents should have this credence in red. The main proponent of this view is Myrvold (2012). He claims that "the dynamics of the system lead all reasonable credences" to a credence of $1/2$ in red (p. 80). Furthermore, he claims that it makes sense to talk, "in cases of disagreement about [the credence to have in red], about one value being more correct than another" (p. 80). So, according to him, the dynamics of the system give us a reason for thinking that there is an objectively correct credence to have in red. In other words, the dynamics of the system objectivise credences, in the sense that they make them (more) objectively evaluable.

How does Myrvold purport to establish this claim? I won't rehearse the precise details of his view here, but simply outline its general structure. This will turn out to be enough to show that he is mistaken in his claims about the power of dynamics to render probabilities objectively evaluable. What then is his view? Firstly, he suggests that we should interpret $p_i$ as an agent's credences in initial speeds, following Savage (1973). Then, he claims that "applying the dynamics of the system to this credence function yields" a probability function $P$ over the set of colour outcomes (p. 79), which gives the "epistemic chance" of these outcomes. He borrows the term "epistemic chance" from Schaffer (2007), who claims that they are subjectively interpreted probabilities which are "objectively informed, and may wear scientific credentials" (p. 137). Thus for Myrvold, the function $P$ represents the credences that an agent ought to have, given the dynamics of the wheel. Finally, he claims that $P$, together with a principle bridging epistemic chances and credences, can constrain the agent's credences $P_i$ in colour outcomes.[21] Now, there are several question marks surrounding aspects of this view, such as: What does it mean to "apply" dynamics to a credence function? Where does the interpretation of $P$ come from? In particular, where does the normative force of $P$ over an agent's credences come from? How could $P$ have normative force over $p_i$ when $P$ was defined partly in terms of $p_i$?

We don't need to answer these questions to see that Myrvold's proposal will not succeed in achieving his aim: on his proposal, it is not the dynamics that provide the constraint on credences. For where resides the normativity on his view? It resides in a newly defined function $P$ and on a bridge principle according to which the agent's credences must conform to $P$. But, by the time this can even be stated,

[21] Myrvold uses different nomenclature to refer to the specific functions; and talks about the epistemic chances, not of colour outcomes, but of the possible angles of orientation of the wheel after it has been spun. I have adapted Myrvold's claims for consistency within this paper, and nothing of philosophical significance hangs on this.

the method of arbitrary functions has already done all its work: indeed as we have seen, what the method shows is that $p_i(X_B)$ and $p_i(X_R)$ are roughly equal given particular assumptions. This must be assumed in order to define the function $P$, and in turn to outline the bridge principle. So, whatever new reason Myrvold may provide us for having objectively evaluable credences, this reason cannot be the system's dynamics: it must instead be something additional.

This criticism of Myrvold's view suggests that the dynamics of the wheel could only constrain credences if the normativity came from, not an additionally defined function $P$ together with a bridge principle, but from the properties of $p_i$ itself. In what follows, I present such a view, which I show is illuminating in various ways, but does not succeed either in achieving Myrvold's aim. Let me present my view with the help of an example, the *urn case*. Consider a set of six balls, numbered 1 through 6, placed in an urn. Let $F = \{1, 2, 3, 4, 5, 6\}$ be the sample space of possible outcomes of a ball pick, and $\mathcal{F}$ be the power set of $F$. Let us define a probability function $p$ over $\mathcal{F}$. Suppose that the even-numbered balls are coloured in blue, and the odd-numbered balls are coloured in white. Let us define a random variable $f : F \to F'$, where $F' = \{W, B\}$ is a two-element set that corresponds to white and blue outcomes. Thus $f$ is a coarse-graining map. We have the mathematical resources now to ask, what is the probability of pulling a white ball: what is the value of $p(f^{-1}(\{W\}))$?

Suppose that we interpret $p$ as an agent's credences, and suppose that the agent does not know which unobserved ball is of which colour. More precisely, she doesn't know the shape of $f$, so that she does not know that $\{1, 3, 5\} = f^{-1}(\{W\})$. In other words, she does not know that "a ball numbered either 1, 3, or 5" and "a white ball" refer to the same outcome. Because she does not know that these two expressions corefer, she may have different beliefs about them. For instance, if her credences are such that $p(1) = p(3) = p(5) = .1$, then her credence in their disjunction will be $p(\{1, 3, 5\}) = .3$. But supposing that she has also heard from a source that she considers reliable that the white and blue outcomes are equiprobable, her credence in the white outcome might be $p(\{W\}) = .5$. This is no other than a *Frege case*: an agent has two incompatible beliefs about a single state of the world, because she considers this state of the world under two distinct descriptions that she does not realise corefer (Frege, 1892).

The problem raised by Frege cases in general is this: even though it is hard to say what an agent has done wrong in these cases (after all, it doesn't seem to be a failure of rationality that an agent not know which unobserved ball is of which colour), there is a sense in which an agent should not have two distinct credences in one and the same state of affairs. It is notoriously difficult to accommodate for Frege cases in a Bayesian framework:[22] if "a ball numbered 1, 3, or 5" and "a white ball" are represented by a single element of the algebra, it is impossible to represent the agent as having different credences in these possibilities; if they are represented by different elements, then the algebra misrepresents the world. How to accommodate these cases in a Bayesian framework is a problem for another paper, I want only here to remark the following. If an agent has different credences in $\{1, 3, 5\}$ and $\{W\}$, there

[22] See for instance Chalmers (2011).

is some sense in which her credences are objectively incorrect. If two agents had the same credences in $\{1, 3, 5\}$, but had different credences in $\{W\}$, at least one of them would be wrong in the sense described earlier in the section: they could not permissibly "agree to disagree" about the credence to have in $\{W\}$.[23]

Let us transpose what I have said about the urn case to the method of arbitrary functions, as presented in §1. Firstly, let us interpret $p_i$ as an agent's credence function over initial speeds. Now, suppose that $h$ and $p_i$ satisfy constraints **(a)** and **(b)**, such that the agent's credence in $X_B$, namely the set of initial speeds such that the wheel spun at one of those speeds will land on a black wedge, is roughly $1/2$. Suppose furthermore that the agent's credence that the wheel will land on a black wedge is $1/3$, maybe because she has heard from a source that she considers reliable that there are brakes under the red wedges which makes landing on them more likely. The agent is involved in a Frege case: she has different credences in two propositions that are extensionally equivalent, namely the proposition that the wheel is spun such that it will land on a black wedge, and that the wheel will land on a black wedge.[24] But those credences cannot both be correct. If two agents have the same credences about initial speeds, they also ought to have the same credences about colour outcomes. Or in other words, an agent's credences in initial speeds determines the credences to have in colour outcomes, that is, the objectively correct credences. So have we not, through the method of arbitrary functions, found a reason *in the world* for constraining an agent's credences? Has the method of arbitrary functions not shown us that the dynamics of a system can serve to objectivise the credences an agent has about that system, in providing a new way to make them objectively evaluable?

I think it has not. In the discussion above, the norm to which we have appealed to establish the objective evaluability of credences makes no reference to the dynamics of the wheel. Instead, the norm

[23] It follows from this that all cases of agents having non-extremal credences in empirical propositions are involved in a Frege case. Suppose that Julian's credence that it will rain tomorrow is $3/4$. Suppose furthermore that his credence that it will rain on the next rainy day is 1. Assuming that it is indeed true that it will rain tomorrow, the expressions "tomorrow" and "the next rainy day" corefer. So, Julian is making a Frege mistake here: he is assigning two different credences to the same proposition. It is unclear what to do about this fact, especially as it pertains to the specific issue of the method of arbitrary functions. It may however have interesting implications for the more general question of credal disagreement and the aims of credences, but that is another paper.

[24] As discussed above briefly, there are questions about how to represent this agent's epistemic state. If her credences are represented by the function $p_i$ on $X$, then she must be represented as assigning two different credences to the same proposition $X_B$. But of course this is impossible, because $p_i$ is a function: by definition, it takes an argument to a single value. There are two options to avoid this. The first is to use two different credence functions to represent the "guises" under which she considers the single proposition. The second is to enlarge her algebra, such that the two extensionally equivalent propositions are represented by two different elements. Which option to pursue is far beyond the scope of this project however. For my current purposes, it suffices to note that agents are susceptible to these Frege cases. How to represent that adequately is a further matter.

is much more general: it prescribes not having different credences in a unique state of affairs. What we did above, was take as a starting point the fact that agents have particular credences in sets of initial speeds (so say, in $X_B$), and then use this norm to insist that agents should have particular credences in colour outcomes (in the wheel landing on a black wedge). But now we can see that the dynamics of the wheel *qua* dynamics have played no role in this. This is because the dynamics have already played their role by the time the norm can even be defined: the dynamics fix the value of $X_B$, which is assumed when the norm is stated.

One might object by claiming that fixing the value of $X_B$ is an essential part of establishing that credences towards the black outcome are objectively evaluable. And furthermore the method plays a crucial role in this: it shows that, if agents have credences in individual initial speeds that respect particular constraints, they ought to have credences in sets of initial speeds such that $p_i(X_B) \approx p_i(X_R) \approx 1/2$. Does that not show after all, that the dynamics of the wheel can serve to objectivise credences? It does not. To see this, it suffices to look at the analogue claim within the urn case. The structure of the balls in the urn are such that, if an agent has credences $p(1) = p(3) = p(5) = .1$ in each of these individual balls being drawn, she ought to have credence $p(\{1, 3, 5\}) = .3$ in their disjunction. But the urn case is not dynamical. So, dynamics are not involved in establishing this conclusion. Instead, it is another feature that the urn case and the wheel case have in common, which is involved: the fact that we are interested in a system at various levels of grain. I explore granularity in more depth in the next section.

To conclude on objective evaluability: I have shown in this section that the method of arbitrary functions cannot help establish that the dynamics of a system can objectivise credences, in the sense of making them more objectively evaluable. To the extent that it does establish the objective evaluability of particular credences, it does so in virtue of a very general norm, according to which it is incorrect to have two conflicting beliefs about a single state of affairs. This suggests that Frege cases in general, and not the method of arbitrary functions in particular, should be studied if one wanted to establish the objective evaluability of probabilities in the sciences. And to the extent that it fixes which credences to have in $X_B$ and $X_R$, it does so in virtue of comprising a coarse-graining map, not in virtue of representing a dynamical system.

**4. Objectivity as Robustness**

In this section, I consider whether the method of arbitrary functions can help establish that some probabilities in the sciences are objective, in the sense that they are high-level *robust*. What does this mean?

Authors who take this line on the method of arbitrary functions begin with an observation: roulette wheels of the kind we have been considering systematically land on a red wedge and on a black wedge with roughly equal probability. (Let us leave the implied probability function uninterpreted for now.) This raises the question: how can that be? What is it, in the world, that makes this true? The reply given by the authors who take this line is that these probability values are high-level *robust*. What this means is that there may be variation in the probability values of lower-level outcomes (that is, the probabilities over individual initial speeds), but the probability values of high-level outcomes (that is, probabilities over $X_B$ and $X_R$)[25] are invariant under such variations. And the method of arbitrary functions is invoked to establish this claim. Indeed, the method shows that, given that the dynamics of a roulette wheel have very specific characteristics (they satisfy condition **(a)**), any probability function $p_i$ over individual speeds (so long as it satisfies condition **(b)**) will ascribe roughly the same probability of $1/2$ to the higher-level red and black outcomes. Thus the method explains why probabilities are invariant: they are high-level robust, because of the wheel's dynamics. Many authors think furthermore that invariance is a kind of objectivity. Thus according to them, the dynamics of the wheel make the probabilities in colour outcomes objective. In the rest of this section, I will examine and reject this view.

The view that the probabilities' robustness can be explained by the wheel's dynamics is a popular one. It can be found prominently in Butterfield (2011), who writes that these high-level probabilities "are robust in a vivid sense: the whole point of the method of arbitrary functions is that they are invariant under a choice of a density function from a wide class" (p. 1087). He claims that this hints that they are "objectively correct" (p. 1084). In a similar vein, Rosenthal (2012) "views objective probability as a high-level phenomenon that arises in deterministic contexts which are structured in a particular way" (p. 227). Abrams (2012) remarks that these "mechanistic" probabilities which are defined in terms of "causal structures" (p. 344) are "robust in the sense that it's difficult to alter [probabilities in lower-level outcomes] in such a way as to alter [probabilities in higher-level outcomes] to a significant degree" (p. 370). Finally, Strevens (2011) takes the method of arbitrary functions to help with "finding a basis for an outcome's probability in the properties of the physical dynamics that produce it." (p. 339).

There is clearly something true in the vicinity of what these authors claim. But, there are also two falsehoods, each either stated or implied by at least one author. These falsehoods are: (1) that robustness is a kind of objectivity; and (2) that the dynamics of the wheel play any role in establishing robustness. In the rest of this section, I expand on and refute each of these claims. It will follow that the method of arbitrary functions cannot help establish that the dynamics of a system objectivise probabilities, in this third sense.

Let me begin with the first claim, (1). The method of arbitrary functions can be used to show that the higher-level properties of a system do not depend sensitively on its lower-level properties: the probabilities of colour outcomes do not depend sensitively on the probabilities of individual initial speeds. Thus the probabilities of colour outcomes may be called high-level *robust*. But this kind of robustness is not a form of objectivity. To see this, contrast our roulette wheel with what is known as

[25] The reader who prefers the probabilities to be defined literally on colour outcomes (and not merely on the set of initial speeds that yields these colour outcomes) can read the claims in this section as if the method has been extended in the way described at the end of §2.

a *chaotic* system, a system such that a small variation in initial conditions leads to a large variation in outcome state. By definition, many of the chaotic system's outcome properties are not robust: they depend very sensitively on the system's initial properties. But, I think it should be clear that these properties are in no way less objective than those of the roulette wheel. They may be less stable, and more difficult to determine, but they are not less objective. It follows that claim (1) is false.

Why then might someone have believed that robustness is a form of objectivity? The strongest reason I can think of is this. Robustness is a kind of invariance: the higher-level properties are invariant under changes in lower-level properties, if assumptions **(a)** and **(b)** are respected. Thus high-level robustness might also be called *lower-level invariance.* And furthermore, at least some kinds of invariance are forms of objectivity; such as (for example) perspective-invariance: something is objective in that sense if it is the same from all perspectives; and subjective otherwise.[26] Unfortunately, I have shown that the kind of invariance that the method of arbitrary functions is widely accepted to establish, lower-level invariance, is not a kind of objectivity. But the observation that robustness is a kind of invariance can nonetheless play a dual role: on the one hand it can help explain why (1) is held despite being false; and on the other hand it can suggest a potential strategy for arguing that the method can help with establishing objectivity, namely to identify a kind of invariance that the method does establish, besides lower-level invariance, and which does constitute a kind of objectivity.

Before concluding on (1), I want to explore and reject one way in which this strategy might be pursued. The hypothetical frequency interpretation, among other interpretations, suffers from what is known as the *reference class problem.*[27] In a nutshell, the problem is this. The probability that a particular patient will get lung cancer depends on whether we consider him as an element of the set of all patients, or of the set of all smoker patients, or of the set of all heavy smoker patients, and so on. Thus what the probability values are depends on the reference class against which we consider this patient. As such, the probability that he will get cancer is reference-class-sensitive, and not reference-class-invariant. Furthermore, it is plausible to think that reference-class invariance is a kind of objectivity: the mention of the reference class is a mention of the modeller's particular perspective on the patient.[28] Could the method of arbitrary functions help with the reference class problem, and thus establish objectivity in the associated sense? We would have to interpret the function $p_i$ as giving the probability of initial speeds given a particular reference class $i$. Furthermore, it would have to be the case that, no matter the reference class $i$, the shape of $p_i$ respected assumption **(b)**, that is, did not vary too quickly. If these two constraints were satisfied, it would follow that the probability of red and black outcomes

[26] This view is championed by Nozick (1998).

[27] There is a vast literature on this problem. The classic discussion begins with Reichenbach (1949), whose PhD was, according to Butterfield (2011), on the method of arbitrary functions! For an insightful contemporary treatment, see Hájek (2007).

[28] This is suggested for instance by La Caze (2016), who writes that there is a sense in which hypothetical frequencies are not objective, namely, they "are not divorced from considerations of personal factors" (p. 358), in part because of the reference class problem.

are roughly equal, no matter the reference class; and thus that the colour-outcome probabilities are objective in the associated sense. But this cannot be the case. For suppose that $p_1$ gives the probability for each initial speed that the wheel will be spun at that speed, and suppose furthermore that the chosen reference class is that which contains only this particular wheel spin. Then, $p_1$ will ascribe probability 1 to the speed at which the wheel is in fact spun, and probability 0 to all the other speeds, thus violating assumption **(b)**. It is therefore not true (and this may seem completely evident) that the probabilities of colour outcomes are roughly equal, for all reference classes.

Thus we arrive at claim (2), according to which the system's dynamics play a crucial part in establishing the robustness or level invariance of the colour probabilities. My argument against this claim is a generalisation of an argument I already presented in §3. When applying the method of arbitrary functions to our roulette wheel, we interpreted the functions $f$ and $h$ as giving the dynamics or mechanics of the wheel. But the way in which the function is interpreted cannot play a role in determining what the method establishes; what plays a role is the more general point that $f$ and $g$ are coarse-graining functions. This is because, as I insisted in §2, the method is a mathematical theorem which holds independently of how its components are interpreted. Whether this function is interpreted as describing the mechanics of a system, or as describing the system's structure, is irrelevant. To see this more vividly, it suffices to notice that the method of arbitrary functions can be applied just as well to systems which are not dynamical, such as the urn example. In that case, the function from individual numbers to colours describes the (static) structure of the system of study. It follows that, contrary to popular opinion, whatever the method of arbitrary functions establishes, it does so independently of any system's dynamics (*qua* dynamics), or of any causal process.

Let me conclude on this section. The hope was that the method of arbitrary functions could help us show that the dynamics of a system can play a role in showing the system's probabilities to be objective, in a third sense of objective. But, in this section, that hope has been extinguished. Indeed, I have shown that the method of arbitrary functions can help establish that the probabilities in colour outcomes are robust. But I have also shown that robustness is not a form of objectivity, and that dynamics play no role in establishing robustness. (The fact that robustness is not a form of objectivity has dialectical implications for my paper. I claimed in §0 that I would show that the dynamics do not objectivise probabilities in any of the three senses of objectivity put forward by authors in the literature. For the first two senses, the properties discussed were indeed senses of objectivity, but the method did not show that dynamics gave rise to these properties. But for this third sense, it is for a deeper reason: because this sense of objectivity is not one after all—there is no objectivity in this sense.)

## 5. Conclusion

Let me begin this paper's conclusion with a summary of its negative claims. I differentiated between three ways in which a probability function might be thought to be objective. Firstly, it may be ontically interpreted; secondly, it may be objectively evaluable (if it is interpreted as a credence function); thirdly, it may be high-level robust. The first two are indeed different senses in which a probability function may be objective. In this paper, I showed that the method of arbitrary functions cannot help show that probabilities can be objective in the first sense, and that it cannot establish that dynamics *qua* dynamics give rise to objectivity in the second sense. Then, I argued that, although the method of arbitrary functions does show that high-level probabilities are robust, this is not a sense of objectivity, nor are dynamics involved in establishing this fact. So there was a widespread hope, in the philosophical literature on probabilities, that the method of arbitrary functions could help show that probabilities in the sciences are objective in virtue of the system's dynamics, in one of three senses of objective. In this paper, I showed that this hope must be extinguished: the method can do no such thing.

I should note that it is consistent with everything I wrote in this paper for there to be another putative sense of objective, beyond those discussed in this paper, such that the method of arbitrary functions could be used to show that dynamics can objectivise probabilities, in this new sense of objective. I cannot think of any such new sense however, nor have I encountered such a sense in the literature. But, for those authors who remain committed to the thought that it is in the wheel's dynamics that the objectivity of probabilities lies, this option remains open in principle. Such an author would have to identify and give an account of this new sense, and show that it emerges from systems' dynamics.

My dialectical strategy has been to identify senses of objectivity in the work of authors in the philosophy of probability. But it should be clear, and I have hinted at, why the three genuine senses of objectivity mentioned in this paper can be deemed important for science, thereby explaining the interest that philosophers of probability have in them. One might want probabilities to be ontically interpreted, on the grounds that science is about the world and not about what agents believe about the world. One might want them to be objectively evaluable, on the grounds that we cannot agree to disagree in science: there is instead a correct thing to believe. One might want them to be perspective-invariant (as briefly discussed in §4), on the grounds that the facts of science are true irrespective of the scientist's perspective on these facts. This suggests another task for the potential proponent of the new, dynamics-emergent sense of objectivity: to explain why this sense matters for science.

But even if there is no such new sense, it is untrue that the method is philosophically uninteresting. Its interest lies in the fact that it shows that particular kinds of dynamics are coarse-graining maps which yield robust probabilities over outcomes. In Butterfield's words, "the basic idea of the method of arbitrary functions [is] that intricate partitions of a sample space can wash out the peaks and troughs of an unknown density function, and secure robust probabilities" (p. 1090). Thus, the method shows that particular dynamics can be instances of coarse-graining maps that induce high-level robustness. The result is interesting, because conditions **(a)** and **(b)** are general enough that they apply to most actual roulette wheel spins. And Strevens (2011) claims, it can be applied beyond roulette wheel spins: a number of physical phenomena, such as coin flips, dice throws, and statistical mechanical phenomena might also exhibit these properties (though of course whether they do is an empirical matter). Thus, the method of arbitrary functions' value lies there: in showing that some robustness-yielding coarse-graining maps represent systems' dynamics. This value has been identified by the authors quoted in §4. But we must be careful not to take the infelicitous leap from this true claim to a false one about objectivity, or to another false one about the specialness of dynamics. This is what I have shown in this paper.

I will end this paper by making two remarks about what the method of arbitrary functions can be used to do, derivatively. I argued in §3 that the method cannot constrain credences directly, in providing a new norm of objective evaluability. However, it can play an indirect role, in the following way. As just explained, the method of arbitrary functions can be used to establish that the frequencies of colour outcomes in roulette wheel spins are uniformly distributed, given some assumptions about the frequencies of individual initial speeds and about the structure of the system. Now suppose, as was first advocated by van Fraassen (1983), that an agent ought to align her credences in outcomes to their frequencies. Then, it would follow that an agent's credences over colour outcomes ought to be uniformly distributed. Thus the method can provide an indirect constraint on credences: by telling us that the frequencies in colour outcomes are what they are, it tells us what our credences in these outcomes should be.

Finally, I want to point to a last indirect possible use of the method of arbitrary functions, which as far as I am aware, has never been discussed. It is widely accepted that the results of a scientific experiment are only valuable insofar as they can be recovered; that is, it is possible to repeat the experiment and get the same result. But, in many cases, replicating the exact initial conditions of an experiment is difficult. The method of arbitrary functions indicates which probabilistic experiments are such that we should not worry too much about replicating the exact initial conditions. It tells us that if the target phenomenon satisfies conditions **(a)** and **(b)**, the specific initial conditions do not matter in a large enough number of trials: the outcome probabilities remain constant. In other words, the method helps us establish that and when the result of a probabilistic experiment is robust. But again, it would be an error to mistake this practical usefulness for conceptual significance.

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