Fighting the Floating Correlations: Expectations and Complications in Extracting Statistical Correlations from the String Theory Landscape

Keith R. Dienes*, Michael Lennek†

Department of Physics, University of Arizona, Tucson, AZ 85721 USA

Abstract

The realization that string theory gives rise to a huge landscape of vacuum solutions has recently prompted a statistical approach towards extracting phenomenological predictions from string theory. Unfortunately, for most classes of string models, direct enumeration of all solutions is not computationally feasible and thus statistical studies must resort to other methods in order to extract meaningful information. In this paper, we discuss some of the issues that arise when attempting to extract statistical correlations from a large data set to which our computational access is necessarily limited. Our main focus is the problem of “floating correlations”. As we discuss, this problem is endemic to investigations of this type and reflects the fact that not all physically distinct string models are equally likely to be sampled in any random search through the landscape, thereby causing statistical correlations to “float” as a function of sample size. We propose several possible methods that can be used to overcome this problem, and we show through explicit examples that these methods lead to correlations and statistical distributions which are not only stable as a function of sample size, but which differ significantly from those which would have been naively apparent from only a partial data set.

*E-mail address: dienes@physics.arizona.edu
†E-mail address: mlennek@physics.arizona.edu
1 Introduction

Over the past few years, it has become increasingly clear that string theory gives rise to a very large number of vacuum solutions [1]. Because the specific low-energy phenomenology that can be expected to emerge from the string depends critically on the particular choice of vacuum state, detailed quantities such as particle masses and mixings — and even more general quantities and structures such as the choice of gauge group, number of chiral particle generations, magnitude of the supersymmetry-breaking scale, and the cosmological constant — can be expected to vary significantly from one vacuum solution to the next. Thus, in the absence of some sort of vacuum selection principle, it has been proposed that meaningful phenomenological predictions from string theory might instead be extracted statistically, through the discovery of statistical correlations across the huge “landscape” of string vacua [2]. Such string-derived correlations would relate different phenomenological features that are apparently unrelated in field theory, and would thus represent string-theoretic predictions that hold for the majority of string vacua.

Unfortunately, the space of possible vacua is extremely large, with some estimates putting the number of phenomenologically interesting vacua at $10^{500}$ or more [2]. Direct computational access to this large data set is therefore virtually impossible, and one is forced to undertake statistical studies of a more limited nature.

To date, there has been considerable work in this direction [2, 3, 4, 5, 6, 7, 8, 9]; for reviews, see Refs. [10, 11]. Collectively, this work has focused on different classes of string models, both closed and open, employing a number of different underlying string constructions and formulations. However, regardless of the particular string model or construction procedure utilized, any such statistical analysis can be characterized as belonging to one of three different classes:

- **Abstract studies:** First, there are abstract mathematical studies that proceed directly from the construction formalisms (e.g., considerations of flux combinations). Although large sets of specific string models are not enumerated or analyzed, general expectations and trends are deduced based on the statistical properties of the parameters that are relevant in these constructions.

- **Direct enumeration studies:** Second, there are statistical studies based on direct enumeration of finite subclasses of string models. Within these well-defined subclasses, one enumerates literally all possible solutions and thereby collects statistics across a large but finite tractable data set.

- **Random search studies:** Finally, there are statistical studies that aim to explore a data set which is (either effectively or literally) infinite in size. Such studies involve randomly generating a large but finite sample of actual string models and then analyzing the statistical properties of the sample, assuming the sample to be representative of the class of models under examination as a whole.
Indeed, all three types of studies have been undertaken in the literature. Certain difficulties are inherent to all of these approaches. For example, in each case there is the over-arching problem of defining a measure in the space of string solutions. We shall discuss this problem briefly below, but this is not the chief concern of the present paper and for simplicity we shall simply assume that each physically distinct string model is to be weighted equally in any averaging process.

By contrast, other difficulties are specifically tied to individual approaches. For example, the first approach has great mathematical generality but often lacks the precision and power that can come from direct enumerations of actual string models. Likewise, the second approach is fundamentally limited to classes of string models for which a full enumeration is possible — i.e., string constructions which admit a number of solutions which is both finite and accessible with current computational power.

For these reasons, the third approach might ultimately seem to have the best prospects for generating precise statistical statements about a relatively large string landscape. As has been recently shown, the problem of directly enumerating certain classes of string models is actually NP-complete [5]. This fact implies that our computational access to the string landscape will always be quite limited, which in turn suggests that random search studies may be more efficient for exploring the string landscape. Indeed, most large-scale census studies are of this type.

Although significant effort has been devoted to studying the algorithms and issues facing direct enumeration studies, relatively little effort has been invested in studying the issues facing random search studies. In this paper, we shall present some elementary observations concerning some of the potential pitfalls of such studies, and the methods by which they can be overcome.

Clearly, one fundamental difficulty is that one must assume that the sample set of string vacua is representative of the relevant class of string vacua as a whole. To attempt to ensure this, one typically generates these sample sets as randomly as possible from amongst the functionally infinite set of allowed solutions in the class. One therefore assumes that no bias has been introduced into this procedure. However, as we shall discuss in this paper, there is a unique alternative kind of bias which is nearly inevitable in random searches through the string landscape. Moreover, as we shall explain, this bias leads directly to the problem of “floating correlations”. This in turn leads to tremendous distortions in the statistical correlations that one would appear to extract through such studies.

In this paper, we shall begin by discussing the origins of this phenomenon. We shall then discuss various means by which it may be overcome. Finally, we shall present an explicit example drawn from studies of the heterotic landscape which illustrates that these issues, and their resolutions, can dramatically alter the magnitudes of the correlations that one would naively appear to extract from the landscape.
2 The problem of floating correlations

In general, there are many different construction formalisms which may be employed in order to build self-consistent string models. For example, closed string models may be constructed through orbifold techniques (with or without Wilson lines), or alternatively using geometric techniques (e.g., by specifying particular Calabi-Yau compactifications). There are also generalized conformal field theory techniques (such as those utilized in Gepner constructions), or special cases of these which involving only free worldsheet bosons or fermions with different boundary-condition phases. Similar choices exist as well for open strings, where one can have, e.g., intersecting D-branes, fluxes, and so forth [12]. Not all construction formalisms are distinct, and the sets of models which can be realized through each construction technique can often have significant overlaps.

Within each construction formalism, there are certain free parameters which one is free to choose; we shall collectively label these internal parameters \( \{x_i\} \). These may be compactification moduli, boundary-condition phases, Wilson-line coefficients, or topological quantities specifying Calabi-Yau manifolds; likewise they might be D-brane dimensionalities and charges, wrapping numbers, or intersection angles. We may also include among the set \( \{x_i\} \) the vevs of moduli fields and/or fluxes which are necessary for guaranteeing stable (or at least sufficiently flat) vacuum solutions. As long as these internal parameters are chosen to satisfy whatever self-consistency constraints are inherent to the relevant construction method (such as those stemming from conformal invariance and modular invariance in the case of closed strings, or anomaly and tadpole cancellations in the case of open strings), one is guaranteed to have constructed a bona-fide string model.

However, regardless of the particular construction formalism employed, one cannot generally define a given string model as being distinct from all others on the basis of an examination of these parameters \( \{x_i\} \). Rather, one must deduce the spacetime properties of the resulting string model in order to deduce whether this model is truly unique when compared with another. Such spacetime properties might include, for example, the gauge group, the number of spacetime supersymmetries, the entire particle spectrum, and the associated couplings. Collectively, we can describe these spacetime properties as belonging to a set of spacetime parameters \( \{y_j\} \). If any of these \( y \)-parameters are different for two candidate models, we say that the two candidate models are truly distinct — i.e., that we truly have two models. Of course, the parameters \( \{y_j\} \) are not independent of each other (as they might be in field theory), but are presumably correlated by the fact that they emerge from a given self-consistent string model. These are the types of correlations that one ultimately hopes to extract as string predictions from the landscape.

In general, each construction technique provides a recipe or prescription for starting with a self-consistent set of parameters \( \{x_i\} \) and generating a corresponding set of spacetime parameters \( \{y_j\} \). In other words, each construction formalism implicitly
provides us with a set of functions $f_j$ such that

$$y_j = f_j(x_i).$$

However, deriving the exact explicit form of such functions is a formidable task, and it is not always possible to extract these functions explicitly from the underlying construction method. What is important for our purposes, however, is that such functions represent the dependence of the spacetime $y$-parameters on the internal $x$-parameters.

Although not much is generally known about such functions $f_j$, one thing is clear: these functions are not one-to-one. Rather, there exist numerous redundancies according to which different combinations of $\{x_i\}$ can lead to exactly the same $\{y_j\}$. In general, such redundancies exist because of a variety of factors. Sometimes, there are underlying identifiable worldsheet symmetries (often of a geometric nature, e.g., mirror symmetries) which cause two different constructions to lead to the same physical string model. In such cases, these redundancies are well-understood and can perhaps be quantified and eliminated from the model-construction procedure, but this process becomes extremely intractible and inefficient for sufficiently complicated models. In other cases, however, there may simply be redundancies in the chosen construction formalism such that different combinations of parameters can result in the same physical string model in spacetime. For example, it often happens that two unrelated sets of orbifold twists and Wilson lines can result in the same string model even when there is no apparent geometric connection between them. Regardless of the cause, however, the important point is that the mapping between the internal $x$-parameters and the spacetime $y$-parameters is not one-to-one. We therefore are faced with the situation sketched in Fig. 1.

This feature can have devastating consequences for a random search through the space of string models. Because any such search must be tied to a particular construction technique, one cannot simply survey the model space of self-consistent $\{y_j\}$; rather, one is forced to survey the parameter space $\{x_i\}$. This means that we do not have direct access to the model space in which each model is weighted equally; rather, we only have access to deformation of this model space in which models with multiple $x$-representations occupy a larger effective volume than those with fewer $x$-representations. We may refer to this deformed model space as a probability space, since each model in the probability space shall be defined to occupy a volume which is proportional to its probability of being selected through a generation of self-consistent $x$-parameters. This is illustrated in Fig. 2.

This can lead to three potential types of bias in a random model search. The first two are relatively obvious and straightforward to deal with:

- First, one may not be sampling the parameter space in a truly random way. Indeed, the selection of $x$-parameters may be skewed as the result of a systematic algorithmic or computational bias. However, this kind of bias is not the focus
Figure 1: Each string model-construction technique provides a mapping between a space of internal parameters (such as compactification moduli) and a physical model in spacetime. However, this mapping is not one-to-one, and there generally exists a huge redundancy wherein a single physical string model in spacetime (such as Model A in the figure) can have multiple redundant realizations in terms of internal parameters. For this reason, the space of internal parameters is usually significantly larger than the space of obtainable distinct models. The shaded region represents models which, though entirely self-consistent, are not realizable through the construction technique under study.

of this paper, and we shall assume that our computational algorithms provide a truly random sampling of model-construction parameters. (In any case, the methods we shall eventually be developing in this paper can compensate for a bias of this type as well.)

- Second, one might be oversampling models for which there exist multiple internal realizations. For this reason, it is necessary to ensure that one does not consider a given string model more than once in the random search process. In other words, each time a self-consistent set of $x$-parameters is generated, one must calculate the corresponding $y$-parameters and verify that these parameters do not match those of any other model which has previously been considered in the same sample. While conceptually straightforward, this requirement is computationally and memory intensive since it requires that any search procedure maintain a cumulative, readable memory of all models that have already been constructed in the sample. Indeed, we have found that this feature alone tends to provide the most severe limitations on the sizes of string model samples that can feasibly be generated.
Thus, while these types of bias are important, both can easily be addressed.

However, the third type of bias is more subtle and is the focus of this paper. In some sense, this problem is the reverse of the second problem itemized above: some models are relatively hard to generate in terms of appropriate \( \{x_i\} \). Of course, this would not be an issue if the redundancy indicated in Fig. 1 were relatively evenly distributed across the model space. Counting each model with a multiple redundancy only once would then eliminate all bias. However, it turns out that some models have redundancies which are greater than those of other models by many, many orders of magnitude. What this means in practice is that when one is randomly sampling the parameter space, one easily “discovers” models such as Model A in Fig. 1 while never finding models such as Model B. Thus, while models such as Model A are likely to be included in any random sample of string models, models such as Model B are almost certain to be missed. Indeed, in a typical search, we are not likely to have the computational power to probe even the full set of highly likely models. Thus we are almost certain to under-represent the relatively unlikely models, assuming we find such models at all.

This kind of disparity is of little consequence if all physical properties of interest are evenly distributed across the model space. For example, if we are interested in knowing what fraction of models have chiral spectra, this kind of disparity will be irrelevant if the chirality property is uncorrelated with the redundancy property. However, it is usually the case that the very same underlying features which create the hierarchy of redundancies for different string models also lead to uneven distributions with respect to their physical properties. For example, a given string construction method may easily yield a set of models with a given property (e.g., a gauge group of a given large, fixed rank), and yet be capable of yielding models that do not have that property (e.g., which exhibit rank-cutting) in some carefully fine-tuned circumstances. Thus, if we are generating a sample set of models, we are likely to miss those “rare” models until our sample size becomes extremely large.

The implications of this can be rather severe. If the physical property about which we are seeking statistical information happens to correlate with this redundancy, then
our statistical correlations or percentages will necessarily evolve (or “float”) as a function of the sample size. Even worse, because we cannot hope to approach a complete saturation of the model space and because we have little guidance as to the sizes or patterns of redundancy in our model-construction procedures, we cannot obtain meaningful statistics by generating more models and waiting for this floating process to become stable. We emphasize that this is a problem that must be faced regardless of our choice of model-construction technique and regardless of how carefully we construct randomized algorithms for model generation.

Thus, we may summarize this problem as follows. Each time we construct a self-consistent set of \( x \)-variables \( \{x_i\} \), we examine the corresponding \( y \)-variables to see if we have really constructed a new model that we have not seen before. If so, we add it to our sample set of models; if not, we disregard the set \( \{x_i\} \) and generate another. Very soon, we reach a stage at which models with some physical properties are “common”, and models with other physical properties are “rare”. However, it is a priori impossible to determine what percentages of models might be “common” and what percentages are “rare” on the basis of this sample set. The problem is that if we keep generating new candidate sets \( \{x_i\} \), we will tend not to generate any further models of the “common” variety because they will have already been generated. In other words, each additional distinct model that we generate has an increasing probably of being “rare”, which is why it is distinct from those that have already been constructed. Thus, rare properties tend to become less rare as the sample size increases, which causes our statistical correlations to float as functions of the sample size. Indeed, in most realistic situations, this problem can be further compounded by the fact that physically interesting properties such as spacetime SUSY, gauge groups, numbers of chiral generations, and so forth may be differently distributed across models with varying intrinsic probabilities of being selected. This too causes our statistical correlations to float as functions of the sample size.

This, then, is the problem of floating correlations. What is required is a means of overcoming this type of bias and extracting statistical information, however limited, from such a model search.

### 3 Modelling the model search: Drawing balls from an urn

It will help to develop a mathematical model for the process of randomly exploring the model space. Towards this end, let us begin by imagining a big urn filled with balls of different colors and compositions. For example, some of the balls are red, while others are blue; likewise, some of the balls are plastic, while others are rubber. Each ball shall correspond to a distinct string model. Thus, exploring the string model space through the random generation of string models becomes analogous to the act of drawing a ball from the urn, noting its properties, marking it for future identification, replacing the ball in the urn, mixing, and then repeating over and over. Of course, since we replace each ball after we have drawn it, each draw is independent.
If the model-generation method is truly random without any inherent biases, then each ball will have the same basic probability to be drawn regardless of its properties. We shall examine this case in detail first, and then consider more realistic cases where the model-generation method is biased.

Clearly, each draw from the urn need not result in a new ball because there is a possibility that we will draw a ball that has already been seen. Thus, after \( D \) draws from the urn, we will have found a number of models \( M(D) \) which we expect to be smaller than \( D \). Although \( M(D) \) is restricted to be an integer, it is straightforward to derive an expression for the expectation value \( \langle M(D) \rangle \). If we have a total of \( N \) different balls in the urn (so \( N \) distinct models are realizable by our construction method), then the probability of drawing any specific ball is simply \( P = 1/N \). Since we are exploring the model space randomly, the difficulty of finding a new ball will be related to how fully explored the model space already is. If we have already seen \( x \) distinct balls, then the probability that a new draw will yield a previously unseen ball is given by

\[
P_{\text{new}} = 1 - \frac{x}{N}.
\]

Given this, we can determine \( \langle M(D) \rangle \) recursively. If \( \langle M(D) \rangle \) is already known, then clearly

\[
\langle M(D + 1) \rangle = \left[1 - \frac{M(D)}{N}\right] \left[\langle M(D) \rangle + 1\right] + \left[\frac{M(D)}{N}\right] \langle M(D) \rangle
\]

\[
= \langle M(D) \rangle \left[1 - \frac{1}{N}\right] + 1. \tag{3.2}
\]

Here the first term on the first line reflects the contribution from the possibility that the next draw yields a new ball hitherto unseen, while the second term reflects the possibility that it does not; moreover, in passing to the second line we have replaced \( M(D) \) by \( \langle M(D) \rangle \). This recursion relation, along with the initial condition \( \langle M(0) \rangle = M(0) = 0 \), allows us to solve for \( \langle M(D) \rangle \) exactly:

\[
\langle M(D) \rangle = \sum_{j=0}^{D-1} \left(1 - \frac{1}{N}\right)^j = N \left[1 - \left(1 - \frac{1}{N}\right)^D\right], \tag{3.3}
\]

and for \( N \gg 1 \) we may approximate this as

\[
\langle M(D) \rangle \approx N(1 - e^{-D/N}). \tag{3.4}
\]

This has the basic behavior we expect; indeed, calculations along these lines have appeared more than a decade ago in Ref. \[13\]. When the model space is relatively unexplored, it is not difficult to find a new model, but as the model space becomes more explored it gets harder to find new models. The main feature to note here is that the total number \( N \) of distinct models appears both as a multiplicative factor
and in the exponent in this expression. This only happens when all models have an equal probability to be generated.

Unfortunately, as we have discussed in Sect. 2, it will typically be the case that different models will have different probabilities of being generated. Indeed, as discussed in Sect. 2, what we are exploring randomly is typically not the model space, but rather the probability space.

In order to account for this, let us now modify the above analysis by imagining that each ball in the urn has a different intrinsic probability of being drawn from the urn, and that this probability depends on its composition. For example, we may imagine that plastic balls are intrinsically smaller or lighter than rubber balls, and thus have a smaller cross section for being selected when we reach into the urn. In general, we shall let \( p_i \) denote the relative intrinsic probability that a ball in population \( i \) will be selected on a random draw, and we shall let \( N_i \) denote the sizes of these populations. For example, if \( N_{\text{plastic}}/N_{\text{rubber}} = 1/3 \) but \( p_{\text{plastic}}/p_{\text{rubber}} = 1/4 \), then a plastic ball will be 1/12 as likely to be drawn from the urn as a rubber ball. We shall assume that all of the balls with a common composition \( i \) share a common intrinsic probability \( p_i \) of being selected, but we shall make no assumption about the number of such populations. Also note that only ratios of the different \( p_i \) shall matter, so there is no need to normalize the \( p_i \) in any particular fashion.

As illustrated in Fig. 2, each model occupies an equal volume in the model space but only a rescaled volume in the probability space; the probabilities \( p_i \) describe these rescalings. Indeed, the total volumes of the model and probability spaces can be defined as
\[
\Omega_{\text{model}} = \sum_i N_i, \quad \Omega_{\text{prob}} = \sum_i p_i N_i.
\] (3.5)

Of course, with this definition \( \Omega_{\text{prob}} \) will scale with the overall normalization of the \( p_i \), but this will not be relevant in the following. What is important, however, is that the probability space will be different from the model space if all of the \( p_i \) are not identical. Thus, the volume relations amongst populations with different \( p_i \) will be different in the two spaces. However, by construction, the volume relations amongst models with the same \( p_i \) will be the same in both the model and probability spaces.

We are now in a position to address what will happen as this model space is explored. By definition, the probability of drawing a ball from a given population is

\*We emphasize that in the case of actual string model-building, models do not have an intrinsic \( p_i \) except in the presence of a particular model-generation technique. Thus, the \( p_i \)'s are associated not only with a given set of models, but also with a specific model-generation technique. As a practical matter, however, one must always have a formalism through which to generate models, so it is sufficient to associate the \( p_i \)'s with the models themselves, as we have done with this ball/urn analogy. We also note that even if there is a bias within the model-generation technique, so that the parameter space in Fig. 1 is not explored truly randomly, this effect can also be incorporated within the probabilities \( p_i \) so long as each parameter combination is explored at least once. Thus, the methods that we shall be developing for overcoming production biases can overcome this type of bias as well.
directly related to the volume occupied in the probability space by that particular population. For a given population \( i \), this probability is \( \hat{P}_i = p_i N_i / \Omega_{\text{prob}} \). However, the probability of finding a new, previously unseen ball within the population \( i \) will also depend on the number \( x_i \) of distinct balls from population \( i \) which have already been found:

\[
\hat{P}_{\text{new}}^{(i)} = \frac{p_i}{\Omega_{\text{prob}}} (N_i - x_i).
\] (3.6)

Here the notation \( \hat{P} \) (rather than \( P \)) indicates that the probability \( \hat{P} \) in Eq. (3.6) is entirely unrestricted, i.e., there is no prior assumption that the draw will even select a model from the \( i \)-population. Using this equation, we can follow our previous steps to calculate the expected number of distinct \( i \)-models \( \langle M_i(D) \rangle \) that will be found after \( D \) draws from the urn. Our recursion relation takes the form

\[
\langle M_i(D+1) \rangle = \langle M_i(D) \rangle \left[ 1 - \frac{p_i}{\Omega_{\text{prob}}} \right] + \frac{p_i N_i}{\Omega_{\text{prob}}},
\] (3.7)

and with the initial condition \( \langle M_i(0) \rangle = M_i(0) = 0 \) we find the solution

\[
\langle M_i(D) \rangle = N_i \left[ 1 - \left( 1 - \frac{p_i}{\Omega_{\text{prob}}} \right)^D \right] \approx N_i \left( 1 - e^{-D p_i / \Omega_{\text{prob}}} \right).
\] (3.8)

Note that the prefactor no longer matches the factor in the exponential.

Eqs. (3.6) and (3.8) give us a general sense of when different populations of models will be found. The populations with the largest \( p_i \) will begin to be explored first simply because they have the larger probabilities of being selected. This will then increase \( x_i \), which in turn makes it more difficult to find new models in this population. Subsequent new models that are found will then start to preferentially come from populations with \( p_j < p_i \). Indeed, only when

\[
p_j = \left( \frac{N_i - x_i}{N_j - x_j} \right) p_i
\] (3.9)

will the probability of drawing a new model from the model space of population \( j \) be equal to that of drawing a new model from the model space of population \( i \). Thus, the exploration of model spaces with smaller \( p_i \) will always lag the exploration of spaces of models with larger \( p_i \).

Note that Eq. (3.8) describes the growth of the individual quantities \( \langle M_i(D) \rangle \) as functions of the number \( D \) of draws. For this purpose, any selection from the urn counts as a draw. However, for some purposes, it is also useful to define the restricted draw \( d_i \) which denotes the number of times a ball from population \( i \) is drawn (again regardless of whether this ball has previously been seen). Each time \( D \) increases by one, we can be certain that one and only one of the \( d_i \) increases by one because our
probability populations are disjoint. However, the expectation value $\langle d_i \rangle$ will be given by

$$\langle d_i \rangle = \frac{p_i N_i}{\Omega_{\text{prob}}} D .$$

(3.10)

Using $\langle d_i \rangle$, we can therefore rewrite Eq. (3.6) in the form

$$\langle M_i(D) \rangle = N_i \left[ 1 - \left( 1 - \frac{1}{N_i} \right)^{\langle d_i \rangle} \right] \approx N_i \left( 1 - e^{-\langle d_i \rangle / N_i} \right) .$$

(3.11)

Of course, as expected, these results have the same forms as Eqs. (3.3) and (3.4) since the use of the restricted draw $\langle d_i \rangle$ allows us to consider each population as truly separate in the drawing process.

Even at this stage, we have still not completely modelled the string model-exploration process. This is because we cannot assume that the physical characteristics of a given model (such as its degree of supersymmetry, the rank or content of its gauge group, the chirality of its spectrum, or its number of fermion generations) are correlated in any way with its probability of being drawn. Or, to continue with our analogy of the balls in the urn, even though the plastic balls may be smaller or lighter than the rubber balls (thereby giving the plastic balls a smaller intrinsic probability $p_i$ of being drawn than the rubber balls), the physical characteristics of the string model may correspond to a completely independent variable such as the color of the ball. Some balls may be red and some balls may be blue, and we have no reason to assume that all red balls are plastic or that all blue balls are rubber. In the following, therefore, we shall continue to let the composition $i$ of the balls represent their probabilities of being selected, but we shall also let the color $\alpha$ of the ball (red, blue, etc.) denote its physical characteristics. This is consistent with the conventions in Fig. 2, where different colors/shadings denote different physical characteristics while size rescalings denote different probabilities of being drawn.

Note that while the different probability populations are necessarily disjoint, the physical characteristic classes need not be disjoint at all. For example, two classes $\alpha$ and $\beta$ may have a partial overlap, such as would occur if characteristic $\alpha$ denotes the presence of an $SU(3)$ gauge-group factor while $\beta$ denotes the presence of $\mathcal{N} = 1$ spacetime SUSY; alternatively, one class may be a subset of another, as would occur if $\alpha$ denotes the presence of $SU(3)$ while $\beta$ denotes the presence of the entire Standard-Model gauge group. All that is required in our formalism is that each class correspond to a set of models exhibiting a well-defined set of particular physical characteristics.

Given this, the populations will generally fill out a population matrix $N_{\alpha i}$. Moreover, given this population matrix, it is then straightforward to determine the average expected numbers of distinct models with particular sets of physical characteristics:

$$\langle M_{\alpha}(D) \rangle = \sum_i N_{\alpha i} \left[ 1 - \left( 1 - \frac{p_i}{\Omega_{\text{prob}}} \right)^D \right] \approx \sum_i N_{\alpha i} \left( 1 - e^{-Dp_i / \Omega_{\text{prob}}} \right) .$$

(3.12)
Thus, as we draw balls from the urn and note their physical properties, we expect the numbers of distinct models exhibiting particular physical properties to grow as a sum of weighted exponentials, where each exponential is weighted by its own population size $N_{\alpha i}$ and where the “time constant” for each exponential is related to a unique probability fraction $p_i$.

Clearly, each different physical characteristic $\alpha$ can be expected to have its own unique pattern of growth for $\langle M_\alpha(D) \rangle$ as a function of $D$. However, it may occasionally happen that two different physical characteristics $\alpha$ and $\beta$ will nevertheless give rise to quantities $\langle M_\alpha(D) \rangle$ and $\langle M_\beta(D) \rangle$ which share the same overall behavior as functions of their arguments, with at most only an overall rescaling between them. In such cases, we shall say that $\alpha$ and $\beta$ are in the same \textit{universality class}. It is straightforward to see that if

$$\frac{N_{\alpha i}}{N_{\beta i}} = \frac{N_{\alpha j}}{N_{\beta j}} \quad \text{for all } (i, j),$$

i.e., if the $\alpha$-row of the population matrix is a multiple of the $\beta$-row, then $\alpha$ and $\beta$ will be in the same universality class. Indeed, in such cases, the $\alpha$-characteristic need not be correlated with the probability deformations, but the $\alpha$- and $\beta$-model spaces nevertheless experience identical deformations. Phrased slightly differently, this means that although the $\alpha$-model subspace experiences a non-trivial deformation in passing to the corresponding probability space, the $\beta$-model subspace experiences exactly the same deformation.

It turns out that Eq. (3.13) is not the most general condition which guarantees that $\alpha$ and $\beta$ are in the same universality class, since we can also have situations in which there exist (subsets of) intrinsic probabilities $p_i$ such that $p_i/p_k = p_j/p_\ell$. In such cases, we do not need to demand the strict condition in Eq. (3.13), but rather the more general condition

$$\frac{N_{\alpha i}}{N_{\beta k}} = \frac{N_{\alpha j}}{N_{\beta \ell}} \quad \text{for all sets } (i, j, k, \ell) \text{ for which } \frac{p_i}{p_k} = \frac{p_j}{p_\ell}.$$

We shall therefore take this to be our most general definition for when two physical characteristics $\alpha$ and $\beta$ are in the same universality class. However, it is easy to see that even when $p_i/p_k = p_j/p_\ell$ has no solutions with $i \neq k$ and $j \neq \ell$, there will always exist the trivial solution when $i = k$ and $j = \ell$. In this case, Eq. (3.14) reduces back to Eq. (3.13).

Regardless of the relations between the different physical characteristics $\alpha$, the fundamental problem that concerns us can be summarized as follows. As we construct model after model, we can keep a running tally of $M_\alpha(D)$ for each relevant physical characteristic $\alpha$ (or for each relevant combined set of characteristics $\alpha$). Equivalently, this information may be expressed as $M_\alpha(d_\alpha)$, where we express the number of models $M_\alpha$ as a function of $d_\alpha$, the numbers of draws which have yielded an $\alpha$-model regardless of whether that model has not previously been seen. Ultimately, on the
basis of this information, our goal is to determine correlations between these sets of characteristics across the entire landscape — i.e., we wish to determine the values of ratios such as \( N_\alpha / N_\beta \), where \( N_\alpha \equiv \sum_i N_{\alpha i} \). However, we now see that we face two fundamental hurdles:

- First, it is not possible in practice to determine \( N_\alpha / N_\beta \) from the \( \langle M_\alpha (D) \rangle \) or \( \langle M_\alpha (d_\alpha) \rangle \) because we do not have prior information about the partial population matrix \( N_{\alpha i} \) or the individual probabilities \( p_i \), both of which enter into Eq. (3.12). Indeed, even if we were willing to do a numerical fit and had sufficient statistical data with which to conduct it, we do not even know the number of distinct exponentials which enter into the sums in Eq. (3.12), and it is always possible to improve accuracy (and thereby dramatically change the resulting best-fit values for the \( N_{\alpha i} \)) simply by introducing additional exponentials into the sum.

- Second, even if we could solve the mathematical problem of extracting \( N_\alpha / N_\beta \) from \( \langle M_\alpha (D) \rangle \) or \( \langle M_\alpha (d_\alpha) \rangle \), we do not know to what extent \( \langle M_\alpha (D) \rangle \) or \( \langle M_\alpha (d_\alpha) \rangle \) can be taken to approximate the exact, discrete integers \( M_\alpha (D) \) or \( M_\alpha (d_\alpha) \) that are actually measured. Clearly, we expect that this approximation should become very good as we explore sufficiently large portions of the corresponding entire model spaces, but we cannot a priori determine when this approximation might actually be valid because we do not know the absolute populations \( N_{\alpha i} \) of these spaces.

Thus, it would clearly be an error to assume that \( N_\alpha / N_\beta \) can be identified as \( M_\alpha (D) / M_\beta (D) \) for any particular value of \( D \) (unless, of course, we have already saturated the model space, with \( D \gg N_{\alpha,\beta} \)). Indeed, if we were to make this error, we would find that our proposed ratio \( N_\alpha / N_\beta \) would “float” — i.e., it would evolve as a function of \( D \). This is, ultimately, the problem of floating correlations. This behavior is illustrated in Fig. 3, which shows the results of an actual simulation in the simple case in which there are only two populations in each variable (\( \alpha \) and \( i \)) and where the population matrix \( N_{\alpha i} \) is diagonal. Even in this dramatically simplified case, we see that our observed ratios of models float dramatically as a function of sample size, reaching the true value only when the full model space has been reached.

Having described the problem in mathematical terms, we shall now propose a solution. The solution is relatively simple in principle, but its proper implementation is somewhat subtle. We shall therefore defer a discussion of its implementation to the next section.

We shall begin by concentrating on the simplest case in which the population matrix \( N_{\alpha i} \) is diagonal. In this case, all physical properties of interest are perfectly correlated with the different probability deformations. Thus, we can identify the \( \alpha \)-population with some value \( i \), the \( \beta \)-population with some value \( j \), and so forth.

Our goal is to generate a value representing \( N_\alpha / N_\beta \) for some pre-determined (sets of) physical characteristics \( \alpha \) and \( \beta \). However, all we can do is make repeated draws
Figure 3: Results of an actual numerical simulation showing how a correlation can “float” as the model space is explored. For this simple example, we have assumed only two disjoint populations of models with different intrinsic probabilities and likewise assumed only two physical characteristics (denoted “good” and “other”). Moreover, we assumed a complete correlation between probabilities and physical characteristics, so that our $2 \times 2$ population matrix is diagonal. We assumed a model space consisting of 310,000 models, one third of which are designated “good”; likewise, this simulation was run repeatedly with different probability ratios $\gamma \equiv \frac{p_{\text{good}}}{p_{\text{other}}}$ reflecting the intrinsic bias of our model-generation procedure. Despite all of these simplifying assumptions, we see that our correlations float very strongly as a function of the sample size of distinct models found, with the ratio of the numbers of “good” models to total models approaching the true value ($=1/3$) only when the total model space is explored. We also see that our statistics from this random simulation do not follow any semblance of smooth behavior until we have examined at least 20,000 distinct models, representing approximately 6% of the total model space.
from the urn, slowly developing tallies $M_{\alpha,\beta}(D)$ or $M_{\alpha,\beta}(d_{\alpha,\beta})$ of the distinct models in these respective classes. As we continue in this process of drawing from the urn, it becomes increasingly difficult to find new, hitherto-unseen models in each class. Indeed, viewing these classes as entirely separate, we see that the probability that each model selected from class $\alpha$ or class $\beta$ will not have previously been seen is

$$P_{\text{new}}^{(\alpha)}(d_{\alpha}) = 1 - \frac{M_{\alpha}(d_{\alpha})}{N_{\alpha}}, \quad P_{\text{new}}^{(\beta)}(d_{\beta}) = 1 - \frac{M_{\beta}(d_{\beta})}{N_{\beta}}. \quad (3.15)$$

These probabilities can be taken as measures of how fully a given model space is explored. Therefore, rather than attempt to identify

$$\frac{N_{\alpha}}{N_{\beta}} \neq \frac{M_{\alpha}(D)}{M_{\beta}(D)} \quad (3.16)$$

for any single value of $D$, our solution is to instead identify

$$\left| \begin{array}{c} \frac{N_{\alpha}}{N_{\beta}} = \frac{M_{\alpha}(d'_{\alpha})}{M_{\beta}(d'_{\beta})} \\ P_{\text{new}}^{(\alpha)}(d'_{\alpha}) = P_{\text{new}}^{(\beta)}(d'_{\beta}) \end{array} \right| \quad (3.17)$$

for two different draw values $d'_{\alpha}$ and $d'_{\beta}$ which are chosen such that their respective production probabilities are equated. Note that while the quantity $d'_{\alpha}$ will correspond to a certain total draw count $D'$, the quantity $d'_{\beta}$ will generally correspond to a different total draw count $D''$. In other words, we do not extract the desired ratio $N_{\alpha}/N_{\beta}$ by comparing $M_{\alpha}(D)$ and $M_{\beta}(D)$ at the same simultaneous point in the search process; rather, we compare the value of $M_{\alpha}$ measured at one point in the search process (i.e., after $D'$ total draws) with the value of $M_{\beta}$ measured at a different point in the process (i.e., after $D''$ total draws). As indicated in the condition in Eq. (3.17), these different points are related by the fact that they correspond to points at which the corresponding $\alpha$- and $\beta$-model spaces are equally explored. This then completely overcomes the biases that result from the fact that the different model spaces are generally being explored at different rates.

Of course, in the process of randomly generating string models, we cannot normally control whether a random new model is of the $\alpha$- or $\beta$-type. Both will tend to be generated together, as part of the same random search. Thus, if $D'' > D'$, our procedure requires that we completely disregard the additional $\alpha$-models that might have been generated in the process of generating the required, additional $\beta$-models. This is the critical implication of Eq. (3.17). Rather than let our model-generating procedure continue for a certain duration, with statistics gathered at the finish line, we must instead establish two separate finish lines for our search process. Of course, these finish lines are arbitrary and must be chosen such their respective $\alpha$- and $\beta$-production probabilities are equated. However, these finish lines will not generally coincide with each other, which requires that some data actually be disregarded in order to extract meaningful statistical correlations.
Thus far, we have been describing the situation in which we seek to obtain statistics comparing only two different groups of physical characteristics $\alpha$ and $\beta$. In general, however, we might wish to compare whole sets of physical characteristics $\{\alpha, \beta, \gamma, \ldots\}$. Our procedure then requires that we establish a whole host of correlated finish lines, one for each set of physical characteristics, and use Eq. (3.17) to make pairwise comparisons.

Given Eq. (3.15), the result in Eq. (3.17) follows quite trivially from the condition that $P^{(\alpha)}_{\text{new}}(d'_{\alpha}) = P^{(\beta)}_{\text{new}}(d''_{\beta})$. The simplicity of this statement may even seem to be a tautology, and indeed the difficulty in extracting the desired correlation ratio $N_{\alpha}/N_{\beta}$ is now reduced to the practical question of determining when the probability condition in Eq. (3.17) is satisfied. This will be the focus of the next section. However, the important point is that we can overcome all of the biases inherent in the model-generation process by focusing on the probabilities for generating new distinct models, and by comparing the numbers of models which have emerged at different points in the model-generation process — points at which these respective production probabilities are equal.

As indicated above, Eq. (3.17) has been derived for the simple case in which the population matrix $N_{\alpha i}$ is diagonal. However, as long as $\alpha$ and $\beta$ are in the same universality class, it turns out that this result also holds for the more general case in which our populations $\alpha$ and $\beta$ are non-trivially distributed across different probabilities $p_i$. This statement is proven in the Appendix, and as we shall see below, this case actually covers a large fraction of physically interesting characteristics. Thus, even in this case, we can overcome the biases inherent in the model-generation process by focusing on the probabilities for generating new distinct models at different points in the model-generation process.

4 Equating probabilities, and the uses of attempts/model

The fundamental task that remains is to develop a method of measuring the restricted probabilities $P^{(\alpha, \beta)}_{\text{new}}(d_{\alpha, \beta})$ which appear in Eq. (3.17), or at least to develop a method of determining when these probabilities are equal. At first glance, it might seem that this should be a relatively simple undertaking. Since we naturally generate data such as $M_{\alpha, \beta}(d_{\alpha, \beta})$ in the course of our model search, it might seem that we could determine the individual model-production probabilities simply by taking a derivative:

$$P^{(\alpha)}_{\text{new}}(d_{\alpha}) \equiv \frac{dM_{\alpha}(d_{\alpha})}{dd_{\alpha}}.$$  (4.1)

Unfortunately, it turns out that taking such a derivative is computationally unfeasible. The reason is that whereas the theoretical expectation value $\langle M_{\alpha}(D) \rangle$ is a smooth, continuous function, the actual “measured” quantities $M_{\alpha}(D)$ are necessarily discrete, jumping from integer to integer at unpredictable values of $D$ or $d_{\alpha}$. Of course, one could perhaps extract $\langle M(D) \rangle$ by repeating the same model-generation
process over and over and averaging the results, but this is computationally expensive and redundant — hardly an efficient solution for a problem which has only arisen in the first place because our computational power is already stretched to the maximum extent.

Indeed, the overall problem is that the production probabilities \( P^{(\alpha,\beta)}(D) \) — which are the only true legitimate measure of the degree to which a model space is explored — fail to be a computationally practical measure because they are extremely sensitive to the difference between \( M_{\alpha}(D) \) and \( \langle M_{\alpha}(D) \rangle \). What we require, by contrast, is an alternative measure of the extent to which a given model space is explored, a measure which may be only approximate but which is less sensitive to the difference between \( M(D) \) and \( \langle M(D) \rangle \) and which can therefore be implemented in an actual automated search through the model space.

To get an idea how to proceed, let us begin by considering the simplified case in which the population matrix \( N_{\alpha i} \) is actually diagonal. In this case, all physical properties of interest are perfectly correlated with the different probability deformations, so that we can identify the \( \alpha \)-population with some value \( i \), the \( \beta \)-population with some value \( j \), and so forth. Thus we expect \( \langle M_{\alpha}(D) \rangle \) (or equivalently \( \langle M_i(D) \rangle \) for some \( i \)) to follow Eqs. (3.8) and (3.11). Given these equations, it then follows that

\[
\frac{N_i}{N_j} = \frac{M_i(D')}{M_j(D'')} \quad (4.2)
\]

only when we satisfy the balancing condition

\[
\frac{D'p_i}{\Omega_{\text{prob}}} = \frac{D''p_j}{\Omega_{\text{prob}}} \quad (4.3)
\]

or equivalently

\[
\frac{\langle d'_i \rangle}{N_i} = \frac{\langle d''_j \rangle}{N_j} \quad (4.4)
\]

Since we do not know the values of the \( p_i \) or the \( N_i \), it is not possible to determine the balanced pairs of values \( (D', D'') \) or \( (\langle d'_i \rangle, \langle d''_j \rangle) \) using these equations. However, since Eq. (4.3) implies Eq. (4.2), we can multiply each side of Eq. (4.3) by \( N_i/\langle M_i(d'_i) \rangle \) or \( N_j/\langle M_j(d''_j) \rangle \) respectively to obtain the equivalent balancing condition

\[
\frac{\langle d'_i \rangle}{\langle M_i(d'_i) \rangle} = \frac{\langle d''_j \rangle}{\langle M_j(d''_j) \rangle} \quad (4.5)
\]

Unlike Eq. (4.3), this balancing equation is easy to interpret and implement in a computer search since \( \langle d_i/M_i(d_i) \rangle \) is nothing but the expectation value of the ratio of ‘attempts’ to ‘models’, where ‘attempts’ refers to the total number of \( i \)-models drawn and ‘models’ refers to the total number of actual distinct \( i \)-models drawn. Thus, we can view our balancing condition as one which equates cumulative attempts
per model, where our attempts are restricted to those which yielded a model (whether distinct or not) in the appropriate class.

Of course, it may initially seem that attempts/model is no better than production probabilities since they are both essentially equivalent when the population matrix is diagonal or has rescaled rows. However, the important point is that since attempts/model does not involve a derivative of $M(D)$, this quantity is actually less sensitive to the difference between $M(D)$ and $\langle M(D) \rangle$ than the production probabilities $P_n^{(a)}$. Thus, we may replace

$$\frac{\langle d'_i \rangle}{\langle M_i(d'_i) \rangle} \rightarrow \frac{d'_i}{M_i(d'_i)}$$

in Eq. (4.5) without seriously damaging our ability to extract the desired ratio $N_i/N_j$ (or $N_\alpha/N_\beta$). This fact is illustrated in Fig. 4.

Figure 4: Results of a numerical simulation involving the same setup as in Fig. 3 but now with $N_{\text{good}}/N_{\text{all}}$ extracted through Eq. (4.7) and plotted as a function of the value of $d'_{\text{good}}/M_{\text{good}}(d'_{\text{good}}) = d'_{\text{other}}/M_{\text{other}}(d'_{\text{other}})$. As we see, use of this method enables us to extract the correct value $N_{\text{good}}/N_{\text{all}} = 1/3$ with considerable accuracy even for relatively small values of attempts/model, regardless of the value of the bias $\gamma$. We also ran similar simulations in which each of the models in the model space was subjected to an additional arbitrary probability deformation; as long as the condition in Eq. (3.14) was enforced, the resulting plot remained essentially unchanged.
This result is valid for the case when the population matrix is diagonal. However, it is straightforward to see that these results also hold for any \( \{\alpha, \beta, \ldots\} \) which are in the same universality class [as defined in Eq. (3.14)]. Because the probability spaces corresponding to models with each of these characteristics have identical deformation patterns, we can repeat the above derivation and find that attempts/model continues to be a fairly accurate measure parametrizing the degree to which a given model space is explored. As it turns out, many physical characteristics of interest \( \{\alpha, \beta, \ldots\} \) have the property that they share identical probability deformations for a given model-construction formalism, and are thus in the same universality class. Thus, for these characteristics, attempts/model can be used in place of production probabilities in extracting population ratios:

\[
\frac{N_\alpha}{N_\beta} = \frac{M_\alpha(d'_\alpha)}{M_\beta(d'_\beta)} \frac{d'_\alpha}{M_\alpha(d'_\alpha)} \frac{d'_\beta}{M_\beta(d'_\beta)}.
\]

(4.7)

Indeed, we can “experimentally” verify whether our chosen physical characteristics \( \alpha \) and \( \beta \) are in the same universality class by calculating the ratio \( N_\alpha/N_\beta \) as a function of the chosen number of attempts/model using this relation, and verifying that this ratio does not experience any float as a function of attempts/model. The absence of any float indicates that the physical characteristics \( (\alpha, \beta) \) are in the same universality class. We shall see explicit examples of this situation in Sect. 5.

One important cross-check is to verify that

\[
\frac{N_\alpha}{N_\beta} = \frac{N_\alpha}{N_\gamma} \cdot \frac{N_\gamma}{N_\beta}
\]

(4.8)

for all \( (\alpha, \beta, \gamma) \) in the same universality class, where each of these fractions is individually extracted through Eq. (4.7). Since Eq. (4.8) is not guaranteed to hold on the basis of the definition in Eq. (4.7), its validity provides an important check on any results we obtain.

It is important to note that this procedure only yields a set of relative abundances of the form \( N_\alpha/N_\beta \) within the same universality class. This is usually the best one can do. However, it is occasionally possible to convert this information to absolute proportions of the form \( N_\alpha/N_{\text{all}} \). For example, if the characteristics \( \{\alpha, \beta, \gamma, \ldots\} \) are non-overlapping, all in the same universality class, and happen to span the entire space of possible physical characteristics, then \( N_{\text{all}} = N_\alpha + N_\beta + N_\gamma + \ldots \) and we can therefore extract the absolute probabilities:

\[
\Omega_\alpha = \frac{N_\alpha}{N_{\text{all}}} = \frac{N_\alpha}{N_\alpha + N_\beta + N_\gamma + \ldots} = \left(1 + \frac{N_\beta}{N_\alpha} + \frac{N_\gamma}{N_\alpha} + \ldots\right)^{-1}.
\]

(4.9)

Alternatively, we can sometimes avoid this procedure by simply letting \( \overline{\pi} \) denote the complement of \( \alpha \) (i.e., the characteristic that a given model does not contain the
characteristic associated with $\alpha$) and calculate $N_\alpha/N_\beta$. If $\alpha$ and $\beta$ happen to be in the same universality class, then our result for $N_\alpha/N_\beta$ will be stable and $\Omega_\alpha$ can then be extracted through Eq. (4.9) where we identify $N_\beta = N_\beta + N_\gamma + \ldots$. Calculating $\Omega_\alpha$ for one member $\alpha$ of a given universality class will then enable us to obtain $\Omega_\beta$ for every other member $\beta$ of the class. However, we stress that this relies on the assumption that $\alpha$ and $\beta$ are in the same universality class, a situation which is not guaranteed to be the case.

Finally, of course, we may face the most general situation in which two physical characteristics $\alpha$ and $\beta$ are not in the same universality class. In such cases, even the ratios $N_\alpha/N_\beta$ determined through Eq. (4.7) will float as a function of the number of attempts/model. Indeed, as mentioned above, this provides a test (indeed, the only viable test) of whether two physical characteristics $\alpha$ and $\beta$ are truly in the same universality class. However, even if $\alpha$ and $\beta$ are not in the same universality class, it may nevertheless be possible to extract individual absolute probabilities $\Omega_\alpha$ and $\Omega_\beta$ through Eq. (4.9) if $\alpha$ and $\beta$ are in the same universality classes as $\alpha$ and $\beta$ respectively. We can then indirectly calculate the relative probability $N_\alpha/N_\beta = \Omega_\alpha/\Omega_\beta$.

Even when $\{\alpha, \beta, \gamma\}$ are not in the same universality class, the cross-check in Eq. (4.8) must continue to hold. However, each individual fraction will not be stable as a function of attempts/model unless it is determined indirectly through $\Omega_{\alpha,\beta,\gamma}$. For example, let us imagine that $\alpha$ and $\beta$ are in the same universality class but $\gamma$ is in a different universality class. In this case, we can use Eq. (4.7) to obtain each of the fractions in Eq. (4.8), but the two factors on the right side of Eq. (4.7) will float as a function of attempts/model, constrained only by the requirement that their product be fixed. However, determining these factors through their absolute $\Omega$-probabilities will enable stable results to be reached.

We see, then, that our solution to the problem of floating correlations involves more than simply tallying the populations of different models generated in a random search — it also requires information about how they were generated, and in particular how many attempts at producing a distinctly new model are required before a given such model is actually found. While this represents new data which might not otherwise have received any special attention, we see that it is this new ingredient which enables us to evaluate the degree to which a given model space has been explored. Moreover, it is relatively easy to keep track of this information during the model-generation process.

In this connection, it is important to note that attempts/model can also have additional important uses beyond Eq. (4.7). For example, attempts/model can be used as a measure of the extent to which a given model space has been explored — even in the presence of an unknown model-generation bias. Thus, use of attempts/model can allow comparisons between model spaces of different (unknown) sizes. This property is illustrated in Fig. 5, which shows that use of attempts/model can completely eliminate the effects of differing model-space volumes. However, it is clear from these plots
Figure 5: *Top figures:* Two different ways of presenting the same simulation results. In each graph, we have plotted the results from three simulations in which the number of models designated “good” (according to a pre-defined characteristic) is one-third of the total. However, these simulations differ in the total size of the model space, with total model-space volumes taken to be $\Omega$, $\Omega/5$, and $\Omega/10$ where $\Omega = 310000$. Each simulation also incorporated a model-generation probability bias $\gamma \equiv p_{\text{good}}/p_{\text{other}} = 1/3$. We see that the observed ratio $N_{\text{good}}/N_{\text{all}}$ floats in each case, but plotting these results as a function of sample size (as in the left figure) does not allow us to separate the effects of the bias from the effects of the different volumes. However, plotting these results in terms of attempts/model (as in the right figure) enables us to completely eliminate the effects of the differing model-space volumes. *Bottom figure:* The results of the same simulation, but now with differing bias ratios $\gamma$. We see that unlike the effects of differing volumes, the effects of bias cannot be overcome simply by considering model spaces at similar levels of exploration.
that the existence of model-generation bias can continue to make a determination of $N_{\text{good}}/N_{\text{other}}$ impossible until the model space is nearly fully explored. Indeed, we see that the amount of model space which must be explored in order to overcome the bias depends on the value of $\gamma$.

5 A heterotic example

In this section, we shall illustrate the above ideas and their implementation in an actual example drawn from the heterotic string landscape. As we shall see, the use of these ideas leads to correlations that differ markedly from those which would have naively been apparent from only a partial data set.

The models we shall examine are all four-dimensional perturbative heterotic string models with $N = 1$ spacetime supersymmetry, formulated through through the free-fermionic construction [14]. In the language of this construction, worldsheet conformal anomalies are cancelled through the introduction of free fermions on the worldsheet, and different models are realized by varying (or “twisting”) the boundary conditions of these fermions around the two non-contractible loops of the worldsheet torus while simultaneously varying the phases according to which the contributions of each such spin-structure sector are summed in producing the one-loop partition function. For the purposes of our search, all worldsheet fermions were taken to be complex with either Neveu-Schwarz (anti-periodic) or Ramond (periodic) boundary conditions. However, we emphasize that alternative but equivalent languages for constructing such models exist. For example, we may bosonize these worldsheet fermions and construct “Narain” models [15, 16] in which the resulting complex worldsheet bosons are compactified on internal lattices of appropriate dimensionality with appropriate self-duality properties. Furthermore, many of these models have additional geometric realizations as orbifold compactifications with randomly chosen Wilson lines; in general, the process of orbifolding is quite complicated in these models, involving many sequential overlapping layers of projections and twists.

A full examination of these statistical correlations for such $N = 1$ string models will be presented in Ref. [17]. Indeed, many of the techniques behind our model-generation techniques and subsequent statistical analysis are similar to those described in Ref. [9]. However, our goal here is merely to provide an example of how certain statistical correlations float, and how stable results can nevertheless be extracted.

Towards this end, we shall restrict our attention to a simple question: with what probabilities do certain gauge-group factors appear in the total (rank-22) gauge group of such $N = 1$ string models? To address this question, we randomly constructed a set of $\approx 3.16$ million distinct models in this class. This set of models is 25 times larger than that examined in Ref. [9], and thus represents the largest set of distinct heterotic string models which have ever been constructed to date. We emphasize that the distinctness of these models is measured, as discussed in Sect. 2, on the
basis of their resulting physical characteristics in spacetime and not on the basis of
the internal worldsheet parameters from which they are derived.

One feature which is immediately apparent from such models is that while $U(1)$
and $SU(2)$ gauge-group factors are fairly ubiquitous, $SU(3)$ gauge-group factors are
relatively rare. Indeed, if we restrict our attention to the first 1.25 million models
that were generated in this set, we find that more than 90% of these models exhibit at
least one $U(1)$ or $SU(2)$ gauge-group factor, while less than $\approx 50\%$ of these models
exhibit an $SU(3)$ gauge-group factor. Thus, we have what appears at first glance
to be a striking disparity: $SU(3)$ gauge-group factors appear to be significantly less
likely to appear than $U(1)$ or $SU(2)$ gauge-group factors, at least in this perturbative
heterotic corner of the landscape.

However, an alternative explanation might simply be that our model-construction
technique (in this case, one involving free worldsheet complex fermions with only peri-
odic or anti-periodic worldsheet boundary conditions) may have certain inherent
tendencies to produce models with $U(1)$ or $SU(2)$ gauge-group factors more easily
than to produce models with $SU(3)$ gauge-group factors. Indeed, even though this
construction technique may ultimately be capable of producing more models with
$SU(3)$ gauge-group factors than $U(1)$ or $SU(2)$ gauge-group factors (thereby causing
the $SU(3)$ models to occupy a larger relative volume of the associated model space),
it may simply be that the models with $SU(3)$ gauge-group factors may be more dif-
ficult to reach and thus occupy a smaller volume within the associated probability
space. If this is true, then we cannot hope to reach any conclusion about the rel-
ative abundances of $U(1)$, $SU(2)$, and $SU(3)$ gauge-group factors on the basis of a
straightforward census of the models we have generated.

Again, we emphasize that this is not a problem unique to the free-fermionic con-
struction. Literally any construction procedure will have an intrinsic bias towards
or against certain string models, yet this need not have anything to do with the ul-
timate statistical properties across the corresponding model spaces. Thus, since we
can examine at best only a necessarily finite sample of models, it is clear that we are
not able to extract any meaningful information from a census study of a finite model
sample alone.

One clue that we are indeed dealing with a model-construction bias in this exam-
ple comes from examining the percentage of models exhibiting an $SU(3)$ gauge-group
factor as a function of the number of distinct models we generated at different points
in our search. This data is plotted in Fig. 6 for the first 1.25 million models, and it
is immediately clear that the percentage of models with $SU(3)$ gauge-group factors
floats rather significantly as a function of the sample size. This implies that models
with $SU(3)$ gauge-group factors occupy a smaller relative volume within the proba-
bility space of models than within the true model space itself. We emphasize that
this need not have been the case: it could have turned out that gauge groups were
uniformly distributed among the populations of models with different probabilities
of production. However, by examining gauge-group correlations as a function of the

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number of models generated, we now have clear evidence that this is not the case. Therefore, before we can draw any conclusions concerning the relative probabilities of specific gauge-group factors for such string models, we must compensate for this distortion of the probability space relative to the model space.

![Graph showing percentage of distinct four-dimensional \( N = 1 \) supersymmetric heterotic string models exhibiting at least one \( SU(3) \) gauge-group factor, plotted as a function of the number of models examined for the first 1.25 million models. We see that as we generate further models, \( SU(3) \) gauge-group factors become somewhat more ubiquitous — i.e., the fraction of models with this property floats. This implies that models with \( SU(3) \) gauge-group factors occupy a smaller relative volume within the probability space of models than within the true model space itself. One must therefore correct for this distortion of the probability space relative to the model space before any conclusions concerning the relative probabilities of specific gauge-group factors can be drawn.]

At first glance, it might seem from Fig. 6 that the proportion of models with \( SU(3) \) gauge-group factors appears to be saturating somewhere near 50% or 60%. However, we must remember that the true size of the full model space is unknown. This means that even though the proportion of models with \( SU(3) \) seems to be floating very slowly, it is difficult to judge how long this floating might continue if we were able to examine more models. Even a small degree of floating could accumulate into a large change in the apparent frequency of \( SU(3) \) gauge factors. Moreover, we
are generally concerned with correlations — i.e., relationships between two or more different variables. For example, we might be concerned with a correlation between the appearance of an $SU(3)$ gauge-group factor and the appearance of an $SU(2)$ gauge-group factor or a $U(1)$ gauge group factor. Since each individual gauge-group factor may experience its own degree of floating, the net float of the correlation can be quite strong even if these individual floats are rather weak.

In order to address these difficulties, we can therefore employ the methods outlined in the previous section. For example, we can let $\alpha$ represent the physical characteristic that a string model contains a $U(1)$ gauge-group factor, $\beta$ represent the same for $SU(2)$, $\gamma$ the same for $SU(3)$, and so forth. If these $\alpha$, $\beta$, and $\gamma$ characteristics are in the same universality class [as defined in Eq. (3.14)], we can use Eq. (4.7) directly to extract $N_\alpha/N_\beta$, $N_\beta/N_\gamma$, and so forth. Indeed, calculating these ratios as a function of attempts/model, we can verify whether $\alpha$, $\beta$, $\gamma$ are truly in the same universality class. Moreover, even when these characteristics are not in the same universality class, we can use the method outlined in Eq. (4.9) to obtain absolute probabilities $\Omega_\alpha$ when $\alpha$ and $\pi$ are in the same universality class. In such cases, we can then convert all of our final information to the same absolute scale $\Omega_\alpha$.

Our results are shown in Table 1. For each listed gauge-group factor, we list the percentage of models containing this factor at least once (tallied across our sample consisting of the first 1.25 million distinct four-dimensional $\mathcal{N} = 1$ heterotic string models we generated) as well as the percentage to which this sample result ultimately “floats”, as extracted through Eqs. (4.7) and (4.9). Although not directly evident from the entries in this table, it turns out from our analysis that each of these group factors is in the same free-fermionic universality class, at least as far as we can determine numerically. Moreover, we were able to verify (again within numerical error) that $\alpha$ and $\pi$ are in the same universality class for the case when $\alpha$ represents the $SU(5)$ characteristic. It was through this observation that we were able to convert the relative probabilities $N_\alpha/N_\beta$ into the absolute probabilities $\Omega_{\alpha,\beta}$ quoted in Table 1.

As is evident from Table 1, the effects of such floating can be rather significant, resulting in relative percentages $\Omega_\alpha$ which often differ significantly from the percentages which are evident in only the finite sample set. Perhaps the most significant example of this can be found in the relation between the $SU_5$ and $SU_4$ columns in Table 1. At relatively low levels of exploration, one would easily conclude that $SO(2n)$ groups (such as $SU_4 \sim SO_6$) are more common than $SU(n \geq 3)$ groups, since every $SO(2n)$ group has a higher probability of occurring than the corresponding $SU(n)$ group of the same rank. However, when the full model space is extracted, it is clear that actually the reverse is true: the ‘$SU$’ groups actually dominate the model space even though they do not dominate the probability space. Indeed, the apparent paucity of ‘$SU$’ groups in our finite sample indicates nothing more than their difficulty of construction — a feature which is completely unrelated to their overall abundance within this class of string models. We see, then, that the issue of floating correlations can be rather important in any attempt to obtain statistical correlations through...
Table 1: Percentage of four-dimensional $\mathcal{N} = 1$ supersymmetric heterotic string models containing various gauge-group factors at least once in their total gauge groups. Here $SU_{>5}$ indicates the appearance of any $SU(n > 5)$ factor, while $SO_{>10}$ indicates any $SO(2n)$ group with $n \geq 6$ and $E_{6,7,8}$ signifies any of the 'E' groups. For each gauge-group factor, the 'sample' column indicates to the percentages of models exhibiting this factor across our sample of more than one million distinct models in this class. By contrast, the $\Omega_\alpha$ column lists the corresponding values to which these percentages would “float”, as extracted through Eqs. (4.7) and/or (4.9). It is clear that correcting for such probability deformations can result in abundances which are markedly different from those which appear within a finite sample.

| group | finite sample | extracted $\Omega_\alpha$ |
|-------|--------------|---------------------------|
| $U_1$ | 99.94        | 95.6                      |
| $SU_2$ | 97.44        | 98.2                      |
| $SU_3$ | 47.84        | 97.6                      |
| $SU_4$ | 51.04        | 29.5                      |
| $SU_{>5}$ | 7.36        | 41.6                      |
| $SU_{>5}$ | 6.60        | 1.72                      |
| $SO_8$ | 13.75        | 1.53                      |
| $SO_{10}$ | 4.83        | 0.21                      |
| $SO_{>10}$ | 2.69        | 0.054                     |
| $E_{6,7,8}$ | 0.27        | 0.023                     |

We emphasize that although the procedures outlined in the previous section are fairly robust, there can be numerous numerical/computational difficulties which can cloud or obscure these results. For example, we found that it was much more difficult to extract information concerning the $SU(3)$ gauge-group factor than for almost any other factor. We attribute this to the fact that the $SU(3)$ gauge-group characteristic is predominantly distributed amongst models with extremely small intrinsic probabilities $p_i$ in this construction, making it difficult to reach significant penetration into this set with sufficiently large values of attempts/model. Moreover, as we have stressed in Eq. (4.6), the actual numbers of attempts/model, just like the actual numbers of models generated, are only approximations to their mathematical expectation values. When attempting to extract correlations between models whose $p_i$ are of hierarchically different sizes, these numerical issues can become severe. These numerical issues must therefore be dealt with on a case-by-case basis when attempting to extract correlations from the landscape.

Given the results in Table 1, one might wonder why we did not quote joint probabilities for the composite Standard-Model gauge group $G_{SM} \equiv SU(3) \times SU(2) \times U(1)$ or the composite Pati-Salam gauge group $G_{PS} = SO(6) \times SO(4)$ in Table 1. The
reason is that these composite groups $G_{SM}$ and $G_{PS}$ do not appear to be in the same universality classes as their individual factors. This, coupled with the numerical difficulties of dealing with apparently small $p_i$, makes an analysis for these cases significantly more intricate. The results for these cases will be given in Ref. [17].

6 Discussion

In this paper, we have investigated some of the issues which challenge attempts to randomly explore the string landscape. We identified an important generic difficulty — the problem of “floating correlations” — and presented a method for overcoming this difficulty which is applicable in a large variety of cases. Moreover, we found that properly compensating for these floating correlations can lead to statistical results which differ, in many cases substantially, from the results which would have emerged from direct statistical examination of only a partial data set. We therefore believe that recognition of and compensation for these effects are absolutely critical, and must play a role in any future string landscape study which operates through a random generation of string models.

It is worth emphasizing that this entire difficulty ultimately stems from our underlying ignorance of the properties of the functions (discussed in Sect. 2) which map internal string-construction parameters into spacetime physical observables. If we had an explicit and usable representation for these functions, we could avoid this whole problem completely since we could analytically (or computationally) account for this kind of bias directly in our model-generating process. It is only because of the difficulty of analyzing such functions in a general way that we are forced into situations in which our model spaces experience such significant probability deformations. These sorts of concerns also fail to play a role in various field-theoretic analyses of the landscape [18].

It is also worth emphasizing that although we have focused in this paper on the specific problem of surveying string models in a way suitable for string landscape studies, the mathematical problem we have been dealing with is actually far more general, arising in all generic situations in which we seek to scan one space (such as the model space) while we only have direct computational access to a second space (such as the probability space) whose relations to the first space are generally unknown or difficult to analyze analytically. Thus, we expect our approach to this problem to have general applicability as well.

Despite these facts, there are still many issues which are left unresolved by our methods. Some of these issues are numerical and computational — for example, one must develop techniques of overcoming other sorts of numerical instabilities and fluctuations which transcend the bias issues we have been discussing, but which nevertheless can be significant. Other issues are more abstract and mathematical — for example, one must eventually develop new and efficient methods of generating and classifying string vacua. One also requires additional theoretical input into the all-
important question of determining which measure is ultimately the most appropriate for landscape calculations. Finally, other issues are more detailed and potentially intractible — for example, although we have given a procedure for extracting statistical correlations between physical observables in the same universality class, we have not provided any procedure for relating physical observables in different universality classes. Barring successful resolution, all of these are critical issues which will likely hamper future statistical studies of the string landscape.

There are also other challenges which are inherent to all attempts at statistical explorations of the string landscape, be they numerical or analytic, randomized or systematic. Although discussed elsewhere (see, e.g., Ref. [9]), we feel that they bear repeating because of their generality.

One of these has been termed the “Gödel effect” — the danger that no matter how many conditions (or input “priors”) one demands for a phenomenologically realistic string model, there will always be another observable for which the set of realistic models will make differing predictions. Therefore, such an observable will remain beyond our statistical ability to predict. (This is reminiscent of the “Gödel incompleteness theorem” which states that in any axiomatic system, there is always another statement which, although true, cannot be deduced purely from the axioms.) Given that the full string landscape is very large, consisting of perhaps $10^{500}$ distinct models or more, the Gödel effect may represent a very real threat to our ability to ultimately extract true phenomenological predictions from the landscape.

Another can be called the “bulls-eye” problem — the realization that since we cannot be certain how our low-energy world is ultimately embedded into a string framework, we do not always know physical characteristics our “target” string models should possess. For example, we do not know whether our world becomes supersymmetric as we move upwards in energy, or whether strong-coupling effects develop which completely change our perspective on microscopic physics. We do not know whether our world remains essentially four-dimensional as we move upwards towards the string scale, or whether there exist extra spacetime dimensions (large or small, flat or warped) which become evident at intermediate scales. Indeed, it is possible that nature might pass through many layers of effective field theories at higher and higher energy scales before reaching an ultimate string-theory embedding. Absence of knowledge concerning the appropriate string-theory embedding thereby limits our ability to identify which statistical information about the string landscape is the most important to extract.

A third challenge can be termed the “lamppost” effect — the danger of restricting one’s attention to only those portions of the landscape where one has control over calculational techniques. Ultimately, barring a complete classification of all consistent string vacua, there is always the danger that there exists a huge sea of unexplored string models whose properties are sufficiently novel that they would invalidate any statistical conclusion we might have already reached. This danger exists regardless of how detailed or comprehensive an analysis we may have just performed. Indeed,
at any moment in time, our knowledge of string theory and various constructions leading to consistent string models is, by necessity, quite limited. A decade ago, one would have considered the heterotic strings alone to have comprised the set of phenomenologically viable string models. The advent of the second superstring revolution has opened the doorway to studies of Type I strings, and recent realizations concerning flux vacua have led to new ideas concerning moduli stabilization. It is impossible to predict what the future might hold, and thus it might be argued that any statistical analysis of known vacua is at best premature.

Closely related to this is the problem of unknowns — even within a given string construction. For example, the methods we have been describing in this paper for sampling string models randomly can eventually allow us to evaluate, with some certainty, how large a volume of the probability space of models might still have been missed in our search. However, although such a statistical study might be able to place an upper bound on the volume that such unexplored models might occupy in the probability space, this does not translate into any bound on the corresponding volume that such models might occupy in the model space. Thus, as long as such models have sufficiently small intrinsic probabilities $p_i$, their total number can essentially grow without bound and yet remain unobservable.

Despite these observations, we are not pessimistic about statistical explorations of the landscape. Instead, we feel that efforts to take this exploration seriously are important and must continue. As string phenomenologists, we cannot hope to make progress without ultimately coming to terms with the landscape. Given that large numbers of string vacua exist, it is imperative that string theorists learn about these vacua and the space of resulting phenomenological possibilities. As already noted in Ref. [9], the first step in any scientific examination of a large data set is that of enumeration and classification. This has been true in branches of science ranging from astrophysics and botany to zoology, and it is no different here. However, before we can undertake this monumental enterprise, we will first need to develop an entire toolbox of statistical techniques and algorithms which are especially constructed for the task at hand. It is therefore our hope that the methods developed in this paper will represent one small but useful tool in this toolbox.

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Appendix A

Eq. (3.17) has been derived for the simple case in which the population matrix \( N_{\alpha i} \) is diagonal. However, as long as \( \alpha \) and \( \beta \) are in the same universality class, it turns out that this result also holds for the more general case in which our populations \( \alpha \) and \( \beta \) are non-trivially distributed across different probabilities \( p_i \). To see this, let us begin again with our probability condition

\[
P_{\text{new}}(D') - P_{\text{new}}(D'') = 0 .
\] (A.1)

For convenience, we shall write these expressions in terms of the total number of draws \( D \) rather than the individual counts \( d_{\alpha} \). In general, these restricted probabilities are given by

\[
P_{\text{new}}^{(\alpha)}(D') \equiv \frac{1}{\Omega_{\alpha}} \sum_i p_i \left[ N_{\alpha i} - M_{\alpha i}(D') \right]
\]

\[
P_{\text{new}}^{(\beta)}(D'') \equiv \frac{1}{\Omega_{\beta}} \sum_i p_i \left[ N_{\beta i} - M_{\beta i}(D'') \right]
\] (A.2)

where \( M_{\alpha i}(D) \) denotes the number of distinct \( \alpha \)-models already found in probability class \( p_i \) and where \( \Omega_{\alpha,\beta} \) are respectively the total probability-space volumes of the \( \alpha \)- and \( \beta \)-models:

\[
\Omega_{\alpha} \equiv \sum_j p_j N_{\alpha j} , \quad \Omega_{\beta} \equiv \sum_j p_j N_{\beta j} .
\] (A.3)

Substituting Eq. (A.2) into Eq. (A.1) then yields the condition

\[
\sum_i p_i \left\{ \left[ \frac{N_{\alpha i}}{\Omega_{\alpha}} - \frac{N_{\beta i}}{\Omega_{\beta}} \right] - \left[ \frac{M_{\alpha i}(D')}{\Omega_{\alpha}} - \frac{M_{\beta i}(D'')}{\Omega_{\beta}} \right] \right\} = 0 .
\] (A.4)

Let us now assume that \( \alpha \) and \( \beta \) are in the same universality class, as defined through Eq. (3.13); the more general definition in Eq. (3.14) can be handled through a reshuffling of indices in what follows. Given Eq. (3.13), let us define the ratio

\[
\gamma \equiv \frac{N_{\alpha i}}{N_{\beta i}} = \frac{N_{\alpha j}}{N_{\beta j}} \quad \text{for all } (i, j) .
\] (A.5)

We then trivially see that

\[
\frac{N_{\alpha}}{N_{\beta}} = \frac{\sum_i N_{\alpha i}}{\sum_i N_{\beta i}} = \gamma
\] (A.6)

and

\[
\frac{\Omega_{\alpha}}{\Omega_{\beta}} = \frac{\sum_i p_i N_{\alpha i}}{\sum_i p_i N_{\beta i}} = \gamma \frac{\sum_i p_i N_{\beta i}}{\sum_i p_i N_{\beta i}} = \gamma ,
\] (A.7)
whereupon it follows that the term in the first square brackets in Eq. (A.4) vanishes. Eq. (A.4) thus reduces to the condition
\[ \sum_i p_i \left[ \frac{M_{\alpha i}(D')}{\Omega_\alpha} - \frac{M_{\beta i}(D'')}{\Omega_\beta} \right] = 0. \] (A.8)

However, this condition must hold for all appropriately balanced pairs \((D', D'')\), since we want our results to be stable as a function of sample size. Indeed, there are literally an infinite number of such pairs \((D', D'')\) for which we require that Eq. (A.8) hold, leading to a number of distinct constraints (A.8) which is guaranteed to exceed the number of probability populations. (One can prove this last statement through induction.) Given this, there is only one possible solution: we must have
\[ \frac{M_{\alpha i}(D')}{M_{\beta i}(D'')} = \frac{\Omega_\alpha}{\Omega_\beta} = \gamma \] (A.9)
for all \(i\). It then follows that
\[ \frac{M_\alpha(D')}{M_\beta(D'')} = \frac{\sum_i M_{\alpha i}(D')}{\sum_i M_{\beta i}(D'')} = \gamma, \] (A.10)
and in conjunction with Eq. (A.6) this yields Eq. (3.17), as originally claimed. Thus, once again, we see that we can overcome all of the biases inherent in the model-generation process by focusing on the probabilities for generating new distinct models, as expressed in Eqs. (3.15) or (A.2), and by comparing the numbers of models which have emerged at different points in the model-generation process at which these respective production probabilities are equal.
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