Big Numbers in String Theory

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Abstract

This paper contains some personal reflections on several computational contributions to what is now known as the “String Theory Landscape”. It consists of two parts. The first part concerns the origin of big numbers, and especially the number $10^{1500}$ that appeared in our work on the covariant lattice construction [1]. This part contains some new results. I correct a huge but inconsequential error, discuss some more accurate estimates, and compare with the counting for free fermion constructions. In particular I prove that the latter only provide an exponentially small fraction of all even self-dual lattices for large lattice dimensions. The second part of the paper concerns dealing with big numbers, and contains some lessons learned from various vacuum scanning projects.

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1 Introduction

My scientific career began in 1977, just after the start of the GUT (Grand Unified Theory) era. Undoubtedly this influenced my expectations for particle physics. I became convinced that during my career I would witness major steps towards the fulfillment of “Einstein’s dream” of deriving the laws of physics (in particular the Standard Model and its parameters) from a fundamental theory. I started working on string theory in 1985, with that long-term goal in mind. When I entered string theory it appeared to be ideally suited to realize that dream. But in 1986 I wrote a paper with Wolfgang Lerche and Dieter Lüst [1] that radically changed my expectations. Our paper was not the only one to shatter the dream of uniqueness, but such a message has more impact if it emerges from your own work.

During the first few months of 1987 I was deeply worried about the relevance of string theory as a fundamental theory of all interactions. But then in the spring of 1987 I realized that the answer string theory gave was in fact precisely the right one to eliminate another worry that had been lingering in my mind for a few years already: the fact that the Standard Model seemed fine-tuned to allow interesting nuclear physics and chemistry essential for the existence of intelligent life, or at least our kind of intelligent life. When I brought that up in private discussions, this point of view was quickly labelled as “anthropic”, and for most people that was a synonym for “unscientific” or worse. Much later I learned that Andrei Linde had come to the same insight a little bit earlier, and had even had the courage to put it in print [2]. Anthropic ideas were already around for more than a decade, mainly in astrophysics and cosmology, and even occasionally in particle physics, but a possible rôle of string theory in this story was not widely discussed until much later, especially after Susskind’s paper “The Anthropic Landscape of String Theory” [3].

I have already written extensively about this elsewhere [4, 5]. Here I will not stray any further into the anthropic path (apart from some remarks in the last section) and focus on computational issues. String theory has driven us, whether we like it or not, in the direction of “Big Data”. This paper is not intended as a review article, but focuses on some of my own contributions to the subject: one of the first attempts at estimating the size of the problem, and work on scanning small subsets of the large number of possibilities. It includes some new insights gained with the benefit of hindsight.

This paper is organized as follows. In the next section I will discuss the origin of big numbers, and in particular the number $10^{1500}$ in [1]. In section [3] I briefly comment on the rôle of moduli, which were essentially overlooked in the papers on algebraic string constructions from 1986 and 1987, but which are now the main origin of the big numbers that characterize the landscape. In the second part, in section [4] I discuss some attempts to handle these big numbers by means of “vacuum scanning”. The last section contains a variety of thoughts concerning the rôle played by big numbers in the landscape.
2 The number $10^{1500}$

When we wrote our paper in 1986 we had no worries about large numbers of solutions, quite the contrary. We were not only pleased that we had found a really nice way to construct chiral string theories in four dimensions, but we also believed that, for better or worse, we had understood something important, namely that in four dimensions string theory was far from unique. To drive home that point we managed to sneak the really big number $10^{1500}$ into our paper. This was then taken out of context and quoted enthusiastically by some, and with disgust by others. Much later, after 2003, we were even credited by some to have anticipated the number of flux vacua. The estimate for the latter number, $10^{500}$ was rather close to ours, with a suitable definition of “close”. But the only thing these numbers really have in common is that they are usually quoted out of context.

Although the proper context was provided in [1], a few additional remarks should be made, and furthermore I want to correct a huge computational error (with minor consequences).

2.1 The Heterotic Compactification Landscape in 1986

But let me first sketch the scene we entered at the end of 1986. In 1984 heterotic strings were discovered [9]. They gave rise to ten-dimensional chiral gauge theories. The powerful consistency conditions of string theory seemed to determine them completely. Well, almost completely, because there were two possible gauge groups, $E_8 \times E_8$ and $SO(32)$. Shortly thereafter it was found [10] that the $E_8 \times E_8$ theory could be compactified on a six-dimensional Ricci-flat manifold called a Calabi-Yau manifold, which led in a rather natural way to $E_6$ GUT phenomenology. At that time the belief in uniqueness was still so strong that some people were convinced that it would soon discover why $SO(32)$ was inconsistent. The $E_8 \times E_8$ theory looked so much better that it had to be mathematically unique. But this belief was already being challenged. The preprint version of [10] states after defining Calabi-Yau manifolds: “Very few are known”, but this phrase was removed in the published version. In June 1985 the first orbifold paper [11] appeared. By now it was even more obvious that a unique outcome was not in sight, although the authors did not comment on that.

2.2 Narain Lattice Compactification

Then in December 1985 Narain wrote a paper entitled “New heterotic string theories in uncompactified dimensions $< 10$” [12]. Initially this was received as a bombshell, because Narain claimed to construct entirely new string theories directly in four dimensions. At CERN, where I worked at the time, a leading physicist remarked “this paper cannot be correct, because string theory is unique”. The bombshell was quickly defused in [13], where it was pointed out that Narain’s construction could be interpreted as a torus

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2The most recent estimate for the number of flux vacua is $10^{272000}$ [8].
compactification with additional $B_{\mu\nu}$ background fields. Torus compactifications were already known and were not seen as a threat to the hope of uniqueness, because they only produced non-chiral theories. These could then be dismissed as phenomenologically irrelevant, and one could hope that one day we would find a fundamental or dynamical reason why chiral theories were selected. Of course, a similar problem existed with the number of space-time dimensions.

2.3 More Chiral Compactifications

In February 1986 Strominger [14] discussed superstrings with torsion and found a large number of chiral solutions, and pointed out the urgent need for a dynamical vacuum selection principle. Later in 1986, Kawai, Tye and Lewellen [15] presented their work on free fermionic constructions. They also found a large number of chiral solutions. If there was ever any hope that the requirement of chirality would lead to uniqueness, this was certainly gone by now.

However, we sensed an attitude of denial in the string community, and this is part of the reason why we pushed the notion that four-dimensional string theory was not going to give rise to anything remotely like a unique answer so strongly. We sensed this correctly. Even two decades later the idea that string theory was going to predict the Standard Model uniquely was common, and even nowadays the failure to meet this expectation is sometimes used as an argument against string theory. There are no negative results in science, just bad expectations. We tried to adjust the expectations, although only with limited success.

2.4 The Covariant Lattice Construction

Our work can be described as a chiral version of Narain’s construction. Narain compactified $p$ left-moving and $q$ right-moving chiral bosons on a lattice $\Gamma_{p,q}$. Modular invariance requires this lattice to be even and self-dual with respect to a metric $(+,\ldots,+,\ldots,-)$, with $p$ $+$’s and $q$ $-$’s. This is called a Lorentzian even self-dual lattice. The consistency condition is invariant under $SO(p,q)$, but the spectrum is not. The spectrum depends on the norms of the left and right components of the lattice vectors, and hence is only invariant under $SO(p)$ and $SO(q)$. This leads to a moduli space

$$\frac{SO(p,q)}{SO(p) \times SO(q)}$$

parametrizing distinct string theories (apart from global relations). In the application to bosonic strings in $d$ dimensions, $p = q = 26 - d$, whereas in heterotic strings one has $p = 26 - d$, $q = 10 - d$. Generically, the moduli space has dimension $pq$, but in special points there may be additional moduli. There are always $pq$ scalars in the spectrum corresponding to the generic moduli. Their vertex operators are, in the bosonic string, $\partial_x X^I \partial_x X^J$, where $I$ and $J$ are the internal coordinates. In the heterotic string the
operators are $\partial_z X^I \Psi^J$, where $\Psi^J$ are the compactified world sheet fermions (one can get the operators in the same form as in the bosonic string by means of picture changing).

The novelty of our approach as compared to Narain’s was that we bosonized all world-sheet fermions. This allowed us to put all the momenta of the bosons on one common lattice. This even included the superghosts, and enabled us to avoid lightcone gauge and work covariantly. Our starting point was a paper my collaborators had written earlier in 1986 [16] about modular invariance in heterotic strings using a covariant quantization formalism developed in [17]. This led them to consider odd self-dual Lorentzian lattices[3] obtained by combining the roots of the Lorentz algebra with the superghost lattice. In a subsequent paper [18] we found a way of replacing the odd self-dual Lorentzian lattice by an even self-dual one, in such a way that modular invariance is maintained.

This allowed us to make use of powerful classification theorems for even self-dual lattices. In the Lorentzian case, such lattices are mathematically unique, up to Lorentz transformations. In the Euclidean case, $p \neq 0, q = 0$, the Narain moduli space (11) becomes trivial, but it turns out that there exist discrete sets of lattices if $p$ is a multiple of 8. These are referred to as ESDL’s (Even Self-Dual Lattices) henceforth. The solutions are completely known only for $p = 8, 16$ and 24, and they are respectively the $E_8$ root lattice; the $E_8 \times E_8$ root lattice and the $D_{16}$ root lattice with a spinor conjugacy class added; and the 24 lattices of dimension 24 classified by Niemeier [19]. Any attempt at guessing how this series continues will be catastrophically wrong, because beyond 24 dimensions the number of lattices starts growing dramatically, as we will see in a moment.

### 2.5 Classification of Ten-Dimensional Strings

Having written the modular invariance condition in terms of even self-dual lattices, we were able to derive all possible 10-dimensional string theories with a maximal rank (i.e. 16) in ten dimensions from Niemeier’s classification [19]. This includes the two supersymmetric heterotic strings, a non-supersymmetric, tachyon-free string theory with gauge group $O(16) \times O(16)$ [20] [21], and five additional tachyonic string theories, which were all already known [22]. Lattice methods are limited to maximal rank by construction. In ten dimensions there is one heterotic string theory that cannot be obtained that way, with gauge group $E_8$, realized as an affine algebra at level 2 [22] [23]. This additional theory can be obtained from a generalization of the Niemeier lattices to conformal field theory. The number of such theories has been conjectured to be 71 [24], and if this list is indeed complete, it implies the completeness of the list of heterotic strings in ten dimensions.

### 2.6 World-Sheet Supersymmetry

After completing [18] we tackled the analogous problem in four dimensions. There was just one hurdle to be taken, namely to find a way of realizing world-sheet supersymmetry.

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[3] The Lorentzian signature of that lattice should not be confused with that of the Lorentz group nor with the signature of the Narain lattice: the lattice vectors refer only to right-moving bosons, and the negative signature belongs to the ghost components.
This was non-trivial since all word sheet fermions had been bosonized. But we could adopt a solution to that problem already proposed by Kawai et. al. [15] (after our paper was written we learned that Antoniadis, Bachas, Kounnas and Windey [25] had already presented this solution in October 1985, in a paper not only missed by us but apparently also by Kawai et. al.). This solution amounted to requiring the presence of a definite set of norm-4 vectors on the right-moving part of the lattice. With that ingredient added, the rest of the work was so straightforward that one of us even wondered if it was worth writing a paper about it. But there was little time to worry about that. A month earlier Kawai et. al. had published a second paper in which they introduced “charge lattices” derived from their free earlier fermion construction. These are odd self-dual lattices, clearly related to the covariant lattice construction we had already introduced and applied in our work on ten-dimensional strings [18]. We were convinced that our even self-dual lattice formulation was superior in elegance, and that therefore a four-dimensional sequel to [18] was worth writing. And there was more competition putting us under pressure to act quickly: while we were writing our paper we heard that Antoniadis, Bachas and Kounnas were working on free fermionic constructions of chiral four-dimensional strings. We submitted our paper on 24 November 1986, and it was followed just before the end of the year by the ABK paper [26]. Shortly after that Narain, Sarmadi and Vafa published a paper on yet another construction they called “asymmetric orbifolds” [27]. During the year 1987 it became clear that all these different constructions are closely related. However, the relations are all based on studies of classes of examples; even today a satisfactory overall picture is still missing.

The final result of our work was a class of string theories described by even self-dual lattices $\Gamma_{22,14}$, with the additional requirement that the right-moving part of the lattice was build out of the weight lattices of $D_5 \times (D_1)^9$ with a definite set of conjugacy classes required to be present in order to have world-sheet supersymmetry. These classes are (here $v$ denotes the vector conjugacy class of the orthogonal lattices)

\[
\begin{align*}
(v, v, v, v, 0, 0, 0, 0, 0) \\
(v, 0, 0, 0, v, v, v, 0, 0) \\
(v, 0, 0, 0, 0, 0, v, v, v)
\end{align*}
\]

Modular invariance at arbitrary genus\footnote{Assuming that the usual problems associated with the superghost partition function at arbitrary genus can be overcome.} is guaranteed by the even self-duality of the lattice.

### 2.7 Lattice Classification Theorems

We realized that the rigid structure of the right-moving part of the lattice freezes the Lorentz rotations of Narain compactifications. Hence we expected a discrete set of solutions, rather than the continuous Narain moduli space. But how could this class be enumerated? At this point we used a trick. Under modular transformations, the characters of a right-moving $D_n$ factor transform in the same way as a left-moving $D_{8-n}$
factor. This allowed us to replace the right-moving $D_5 \times (D_1)^9$ lattice factor by a left-moving $D_3 \times (D_7)^9$, which gives rise to a Euclidean lattice of total dimension 88. Hence all solutions can be read off from a list of such lattices. But we quickly realized that this was totally useless as an approach to enumeration, because the list of such lattice is unfathomably large.

There is an amazing formula known as “the Siegel mass formula” (see [28]), which states

$$\sum_{\Lambda} \frac{1}{g(\Lambda)} = \frac{B_{4k}}{8k} \prod_{j=1}^{4k-1} \frac{B_{2j}}{4j} \equiv L_{8k},$$

where $B_{2j}$ are the Bernoulli numbers, the number of dimensions is $8k$, and $g(\Lambda)$ is the order of the discrete automorphism group of $\Lambda$. Since this group has at least two elements (the identity and reflection through the origin), one derives a lower limit on the number $N_{8k}$ of lattices in $8k$ dimensions:

$$N_{8k} > 2L_{8k}$$

In [29] we made a quick estimate of $L_{88}$, and found a number of order $10^{1500}$. The actual number of lattices would have to be larger still, but the restrictions to be imposed on them (namely the presence of a $D_3 \times (D_7)^9$ and the vectors [2]) will reduce the total by another huge factor.

### 2.8 Finiteness, free fermions and enumerability

Although the map to Euclidean lattices is useless as a path towards enumeration, it did serve another purpose, namely showing that the number of solutions is finite. Finiteness is obvious in free fermion constructions with (anti)-periodic boundary conditions. If the fermions are complex $5$, the resulting four-dimensional string theory can be written in terms of a lattice. But the converse is not true: Most lattices cannot be written in terms of free fermions. If the lattice is made out of $D_n$ roots and weights, one obviously can, but not if it is made out of $A_n$ roots and weights. For example, the $A_2$ weight lattice defines a free bosonic CFT with conformal weights $0$ and $\frac{3}{2}$, while in free fermionic CFT’s one can only get multiples of $\frac{1}{8}$.

However, we are considering even self-dual lattices here, where all conformal weights are integer. Often in string theory two construction are related even if the relation is not manifest. So it is certainly imaginable that all self-dual lattices can be written in terms of free fermions, even if they are built out of $A_n$ lattices or other factors. In that case, the fermionic construction would provide a proof of finiteness. However, this does not work. In the appendix we show that there exist Euclidean even self-dual lattices that cannot be written in terms of free fermions with (anti)-periodic fermions. Hence the Siegel mass formula provides the only presently known way to demonstrate that the number of lattice solutions is finite. Indeed, I do not know an algorithm that would generate all even self-dual lattice of a given dimension in a finite amount of time.\footnote{Real fermions allow for additional possibilities that cannot be written in terms of lattices.}
One path towards such an algorithm might be the following. One starts by decomposing
the 8k-dimensional lattice into 8k U_{2R} factors. Here U_{2R} is a compactified chiral
boson with 2R primaries. Hence U_2 is equivalent to A_1 level 1 and U_4 is D_1, equivalent
to a complex pair of free fermions. Such a decomposition should always be possible due to
the lattice structure. The lattice CFT has 8k free bosons, and the only allowed operators
in such a CFT are derivatives of the bosons multiplied with $e^{i\vec{v}\cdot X}$, where $v$ is a lattice
vector. The fact that the unit cell of a self-dual lattice is non-zero implies that a basis
must exist such that all components $v_i$ are quantized.

For given $R$ this yields a finite algorithm for the construction of all even self-dual
lattices of a given dimension. However it is not clear what value of $R$ should be used for
given $k$. For $k = 1$ and $k = 2$ the value $R = 2$ suffices (indeed, $R = 1$ is already enough)
but in the appendix we show that for $k > 8$ $R = 2$ is not large enough (presumably $R = 2$
is insufficient already for smaller values of $k$). What is missing to turn this into a finite
algorithm is an upper limit of $R$ for given $k$. It is clear that this upper limit increases
with $k$. The same argument used in the appendix to estimate the number of fermionic
theories can be used for products of $U_{2R}$. For given $R$, the upper limit for the number of
such lattices is given by $f(R)^{k^2}$, where $f(R)$ is a large integer that depends on $R$, but not
on $k$ (for $R = 2$, the most conservative upper limit derived in the appendix is $f(2) = 4^{64}$).
Hence the upper limit only grows with a power $k^2$, which for any value of $R$ is always
surpassed by $L_{8k}$, for sufficiently large $k$.

### 2.9 An Upper Limit?

In stating that the total number of lattices is finite we made an assumption, namely that
the group of discrete symmetries of a finite-dimensional lattice is always finite. There is
in fact a plausible candidate for the 8k-dimensional lattice with the maximal number of
discrete symmetries, namely the D_{8k} root lattice. This can be made self-dual by adding
a spinor conjugacy class. In eight dimensions this enhances the discrete symmetries: one
obtains the root lattice of $E_8$, and hence the discrete symmetries are the Weyl group of $E_8$,
which is larger than the Weyl group of $D_8$. This happens because the additional spinor
roots provide additional Weyl reflections. In 16 dimensions and more this enhancement
does not occur, and the order of the symmetry group of the root lattice of $D_{16}$ with a
spinor conjugacy class added is that of the Weyl group of $D_{16}$. However, one may check
that the discrete symmetry group of the $E_8 \times E_8$ lattice (this group is the product of the
two Weyl groups and the interchange symmetry of the factors) is larger than the Weyl
group of $D_{16}$.

In 24 dimensions it is $D_{24}$ that provides the largest set of discrete symmetries, and
it is easy to check that in 8n dimensions for $n > 3$ the Weyl group $W_{8k}$ of $D_{8k}$ is larger
than the symmetry group of the lattice $(E_8)^k$. In general, symmetry groups of lattices
always contain the Weyl group of the root system, but usually this is only a small sub-
group of the full automorphism group. Indeed, the Leech lattice has no roots at all,
but its automorphism group is a double cover of the Conway group, and has order
$8,315,553,613,086,720,000 \approx 8.3 \times 10^{18}$. However, the Weyl group of $D_{24}$ has order
\[ 2^{23}(24!) \approx 5.2 \times 10^{30}, \] which is far larger. It is known that beyond 24 dimensions the bulk of the lattices has a root lattice of rank less than \( 8k \), and there are huge numbers of ESDL’s with no roots at all. With only one specimen of rootless lattices known explicitly it is a bit hazardous to speculate, but one might conjecture that the Weyl group of \( D_{8k} \) is the largest possible lattice symmetry group for all \( k > 3 \). Assuming this is true, we get the inequality

\[ N_{8k} < |W_{8k}|L_{8k} \]  

(5)

### 2.10 Upper and Lower Limits for \( 8k \leq 88 \)

It is straightforward to compute the number \( L_{8k} \) exactly. We have computed these numbers for all dimensions up to 88, and they are shown (multiplied by 2) in the second column of table 1. The conjectured upper limits are shown in column 3 of the table, where of course for \( k = 1 \) and \( k = 2 \) we used \( E_8 \) and \( E_8 \times E_8 \).

The estimate “10\(^{1500}\)” of [1] was supposed to be an approximation of the last number in the second column of the table. Hence the exact computation yields about 10\(^{930}\) instead of 10\(^{1500}\). Sadly, this was one of the worst estimates in the history of science. This is presumably due to an algebraic error rather than a poor approximation. Indeed, there exist extremely accurate estimates of the Bernoulli numbers (see eqn. (12) in the appendix) which we presumably used.

But this is still gives only a lower limit on the number of lattices. The true upper limit, assuming a maximal automorphism group, is about 10\(^{1000}\), as shown in the third column of the table. None of this makes any difference for the conclusions, however. The only scientific point we were making is that the number is finite and large, and that is true regardless of the estimate. In six and eight space-time dimensions the same arguments can be used, and the Euclidian lattice dimension is respectively 64 and 40. We read off from the tabel that there are at most \( 10^{455} \) resp. \( 10^{111} \) such lattices.

<table>
<thead>
<tr>
<th>Dimension</th>
<th>Lower limit ( \times 10^a )</th>
<th>Upper limit ( \times 10^b )</th>
<th>Actual Number</th>
</tr>
</thead>
<tbody>
<tr>
<td>8</td>
<td>2.870554085831864 ( \times 10^{-9} )</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>16</td>
<td>4.977181647677474 ( \times 10^{-18} )</td>
<td>2.4160839160839161 ( \times 10^{16} )</td>
<td>24</td>
</tr>
<tr>
<td>24</td>
<td>1.587356093933540 ( \times 10^{-14} )</td>
<td>4.130854882089717 ( \times 10^{52} )</td>
<td>?</td>
</tr>
<tr>
<td>32</td>
<td>8.061846587120415 ( \times 10^{7} )</td>
<td>2.277750478211998 ( \times 10^{52} )</td>
<td>?</td>
</tr>
<tr>
<td>40</td>
<td>8.786162893954708 ( \times 10^{51} )</td>
<td>1.970535004851803 ( \times 10^{111} )</td>
<td>?</td>
</tr>
<tr>
<td>48</td>
<td>3.051507011767375 ( \times 10^{121} )</td>
<td>2.66564898686395 ( \times 10^{196} )</td>
<td>?</td>
</tr>
<tr>
<td>56</td>
<td>1.276238439666753 ( \times 10^{219} )</td>
<td>1.634633237068218 ( \times 10^{310} )</td>
<td>?</td>
</tr>
<tr>
<td>64</td>
<td>9.544539505709636 ( \times 10^{346} )</td>
<td>5.585108422305436 ( \times 10^{454} )</td>
<td>?</td>
</tr>
<tr>
<td>72</td>
<td>9.6133130349683812 ( \times 10^{506} )</td>
<td>6.949609601107582 ( \times 10^{631} )</td>
<td>?</td>
</tr>
<tr>
<td>80</td>
<td>5.862018298127880 ( \times 10^{700} )</td>
<td>1.267986279010439 ( \times 10^{843} )</td>
<td>?</td>
</tr>
<tr>
<td>88</td>
<td>6.485314719426174 ( \times 10^{929} )</td>
<td>9.307090939221263 ( \times 10^{1089} )</td>
<td>?</td>
</tr>
</tbody>
</table>

Table 1: Minima and maxima of the number of even self-dual lattices in \( 8k \) dimensions.
2.11 Dimension 32

A bit more is known for dimension 32. The Siegel mass formula can be worked out for any given root lattice separately, and this was done in [30]. The root lattice of an ESDL is by definition the sub-lattice spanned by the vectors of norm 2. This may be trivial, and it may be of lower dimension than the lattice under consideration. If the rank of the root lattice is equal to the rank of the ESDL, the root lattice is called complete. Summing over the contributions of all 13218 root lattices that can actually occur (including the trivial class with no roots at all), one obtains a lower bound of $1.1 \times 10^9$. This involves a substantial amount of computation. To find out which root lattices can occur one works out all combinations of simple Lie algebra root lattices with rank 32 or less, and works out the Siegel mass formula of that class. If it is zero, there are no lattices with that root lattice, and if it is non-zero, there must be at least one.

This analysis shows also that there are at least $10^7$ ESDL’s of dimension 32 without roots. Furthermore it is known that there are exactly 132 even selfdual lattices that are indecomposable and complete [31]. This means that their root lattice, generated by the vectors of norm 2, has rank 32 but is not a direct sum of lattices of dimension 8 and 24 or twice 16. These 132 ESDL’s have 119 different root lattices. If we include $(D_{16})^2$ and $E_8$ times the 23 Niemeier lattices with a complete root system, we get a total of 143 distinct complete root systems, a small fraction of the full set of 13218.

One could use also this approach to work out an upper bound along the lines sketched above. In the tex source of [30] there is a table giving for all 13218 root lattice the Siegel mass (the sum of reciprocals of orders of automorphism groups) times the order of the Weyl group. Let us call this number (i.e. Siegel mass times Weyl group order) $m_R$, where $R$ labels the root lattices. Instead of formula (5) we now get

$$N_{8k} < \sum_R m_R \frac{|W_{8k}|}{|W_R|},$$

where $W_R$ is the Weyl group of $R$. This sum can be worked out explicitly using the list in [30], but it is not going to improve the upper bound in the table by much, if anything, because there are several individual contributions of order a few times $10^{51}$.

2.12 A More Accurate Estimate

A more accurate estimate for the number of covariant lattices in four dimensions can be made along the lines of the arguments in the appendix, at least for the subset that can be realized in terms of free fermions. This means that although the lattices we need are generically of the form $D_3 \times (D_7)^9 \times \Gamma_{22}$, we will only consider the subset where $\Gamma_{22}$ is a product of 22 $D_1$ factors. There may be additional lattices, but since we have only 22 dimensions that can deviate from free fermions, we can try to estimate what is missing by asking which fraction of the 24 Niemeier lattices cannot be realized in terms of free fermions. This fraction may well be zero, but at least eight of the Niemeier lattices (including the Leech lattice) have an explicit free fermion realization, so in that case
fermionic constructions are underestimating the total by at most a factor 3. The fraction of non-fermionic lattices increases rapidly with dimension, so since 22 is close to 24, but smaller, this suggests that we will not be missing too much – at least on a log scale – by limiting ourselves to free fermions.

Now we have a lattice with 32 orthogonal group factors, and this problem is combinatorially analogous to \((D_1)^{32}\), as was already pointed out in a footnote in [1]. This implies that one can bring the lattice basis to the form shown just above Eqn. (15) in the appendix. This basis divides the 32 lattice components into three blocks, one of size \(N\), a second one of size \(32 - 2N\) and a third one that is again of size \(N\).

Since the 32 orthogonal factors are not all identical one has to sum over all possible ways of distributing \(D_3, D_7\) and \(D_1\) over the three blocks, which enlarges the total number of distinct possibilities. For each such choice the total number of possibilities is given by the formulas given in the appendix, such as Eqn. (21). This estimate gives a good approximation to the number of lattices a computer would find in a systematic search. The only source of inaccuracy is due to the estimate of the fraction of vectors that have a certain required length (modulo 2) and a certain inner product (modulo 1). Since we are now dealing with mixed \(D_1, D_3\) and \(D_7\) lattices, the inaccuracy is harder to estimate than in the appendix. The easiest way to deal with this is take into account the maximal observed variation in both directions as an error estimate. For lattices of 32 components that gives an error of about ±3 in the exponent.

Now we have to impose the condition that the constraint vectors (2) are on the lattice. Since the lattice is self-dual, this is true if and only if the constraint vectors have integer inner product with all lattice vectors. To check this, we have to consider the inner product of each of these three vectors with the lattice spinor generators, i.e. the first \(N\) glue vectors. The inner product can be integer or half-integer, with each possibility occurring roughly in half of the cases. Hence we get a reduction by a factor \(2^{-3N}\). Although the \(D_7\) factors are associated with definite lattice factors, there may be several distinct choices for combining them into triplets. These choices must also be summed over. The total number of distinct covariant lattice choices \(N_{CL}\) is then

\[
N_{CL} = \sum_{N=0}^{16} d(N) 2^{32(N-1) - N^2 - 3N},
\]

where \(d(N)\) is the number of distributions of \(D_3\) and \((D_7)^9\) over the three lattice blocks generated by the basis choice and the number of distinct assignments of \(D_7\) to triplets. Note that for some assignments, for example when all of \(D_3(D_7)^9\) is in the first \(N\) components, no choice of constraint vectors is on the lattice. Then this configuration does not contribute to \(d(N)\). The general rule is that each of the three constraint vectors must have at least one component in the second or third block, so that their inner products with the \(N\) spinor classes can be tuned to the the correct value. This is because the first \(N\) entries of the spinor conjugacy classes are already fixed.

The factors \(d(N)\) can be computed exactly, and are of order a few tens to at most few hundreds. The exponential factor takes its maximal value for \(N = 14\) and \(N = 15\),
and this value is about $10^{53}$. Taking into account the error in estimating inner products, and allowing two orders of magnitude for the estimate of $d(N)$, this gives an upper limit of about $10^{55\pm5}$. This is the maximal number of orthogonal group lattices that can be generated by a computer going systematically through all possibilities. Of course this number is still reduced by lattice degeneracies, which potentially gives a huge suppression. This suppression is less than it is for $(D_1)^{32}$, because not all factors are identical. Based on what we know about lattices of dimension 32 and less, it may be expected to reduce the number by 20 to 50 orders of magnitude. The total number of distinct four-dimensional covariant lattice theories with the triplet supercurrent then comes out somewhere between a mere $10^5$ to $10^{35}$, with large uncertainties, but in any case nowhere near $10^{1500}$. But this number was never claimed as an estimate anyway.

Using the method of [30] mentioned above one should be able to find lower bounds on the number of lattices of dimension 88 with a root lattice $D_3 \times (D_7)^9 \times X_{22}$ where $X_{22}$ is any root lattice of dimension 22 or less. This would undoubtedly be a considerable amount of work but it might be doable, since the number of options for $X_{22}$ is a subset of all possible 32 dimensional root lattices, which have already been enumerated. If these results could be extended to lattices generated by vectors of norm 4 one might be able to get lower bounds on the number of lattices containing the supersymmetry constraint vectors (2), i.e. the ones of actual physical interest. But this sounds too good to be true.

### 2.13 Other supercurrents

There are more four-dimensional strings with a covariant lattice description than the ones described above. The choice of supercurrents (2) is not the only possible one. A second possibility is to use nine vectors of the form $(v, 0, \ldots, \sqrt{3}, \ldots, 0)$. This uses a realization of world-sheet supersymmetry first found in [32]. Other realizations were found in [33, 34]. In each of these cases one could presumably build a mapping to even self-dual lattices, proving again that the number of possibilities is finite. But it is not known what the number of ways of realizing world-sheet supersymmetry in terms of free bosons is. If one could prove that this number is finite, we would know that the entire class of covariant lattice theories with chiral spectra is finite. My guess is that it is.

### 3 Moduli

From the current perspective the status of moduli in these early works is rather strange. Calabi-Yau compactifications, first proposed in the end of 1984 [10], were known to have moduli. The problems associated with moduli had also already been discussed [35]. Yet none of the free boson or fermion four-dimensional string papers discussed above makes any mention of moduli at all. The reason for this omission is clarified by the following quote from a paper by D. Gepner [36]. This paper appeared in April 1987 and contained the description of a class of four-dimensional string models based on interacting CFT’s,
namely minimal $N = 2$ superconformal field theories. We will return to these “Gepner models” in the next section. The conclusion of the preprint version of Gepner’s paper contains the phrase: “The compactifications described here are inherently free of the moduli problem”. The reason given for this is that any change in the radius of the free bosons “would break the $N = 1$ conformal gauge condition”. In the published version of the paper was inversed, and a statement was added that these compactifications, contrary to what was believed earlier, are in fact Calabi-Yau compactifications in a special point in moduli space. Moving away from that point by giving vacuum expectations to the moduli leads to perfectly valid string compactifications, but which do not admit an exact CFT description.

This is quite analogous to what happens with covariant lattice models. Narain lattice model have moduli; the usual ones of the torus. The requirement of world-sheet supersymmetry imposes a rigid structure on the right-moving lattice. We took it for granted that this fixed all the moduli; indeed, we did not even discuss moduli. It is certainly true that all Narain moduli are removed from the massless spectrum. A Narain lattice is a special case of a covariant lattice where $\Gamma_{22,14}$ takes the form $\Gamma_{22,6} \times E_8$, where $E_8$ contains the space-time lattice $D_5$. Massless scalars in any covariant lattice theory must be vectors in $D_5$. To be massless, these vectors must have a conformal weight $\frac{1}{2}$ component in the rest of the right-moving lattice, and a conformal weight 1 component from the left sector. In a Narain compactification there always exist states with those properties. Some of the roots extending $D_5$ to $E_8$ are vectors of $D_5$ and have total conformal weight 1. They can be combined with vertex operators $\partial X^I$ from the left sector, or any root of the left lattice. The latter may not exist, but the former are always present, and give rise to precisely the expected $6 \times 22$ generic moduli.

It follows that if $D_5$ is not extended by roots that are $D_5$ vectors, then these canonical moduli are not present in the theory, in agreement with the fact that the rigidity of the right-moving lattice obstructs all Lorentz rotations. But there can be other moduli. If there are vectors on the lattice $\Gamma_{22,14}$ that have the form $(x_L, x_R)$, with $x^2_L = x^2_R = 2$, such that $x_R$ is a vector of $D_5$, then these vectors give rise to massless scalars, which may well be moduli. These vectors should not be combinations of roots of the left- and right lattice. In that case they imply an extension of the $D_5$ lattice to $D_n$, $n > 5$, and this implies that the theory is at least partly a torus compactification from a higher dimension. Then at least some of the moduli are canonical Narain moduli.

What does $(x_L, x_R)$ correspond to in the even self-dual lattice $\Gamma_{88}$? We can decompose vectors on that lattice as $(u, s, t)$, where $u$ is a $D_3$ vector, $s$ a vector in $(D_7)^9$ and $t$ lives in the remaining 22-dimensional space. Clearly $t = x_L$ and $u$ is a $D_3$ vector. What $s$ is depends on how the conformal weight $\frac{1}{2}$ is decomposed in terms of the $(D_1)^7$ right-moving lattice. There are two options: a vector of one of the $D_1$ factors, or four spinors of four distinct $D_7$’s. In the former case $s$ is just a vector of one of the $D_7$ factors, and $(u, s, t)$ is a vector of norm 4 on $\Gamma_{88}$. In the latter case $s$ is a combination of four spinors of four distinct $D_7$ factors. These have norm $\frac{7}{4}$ each, so that the total norm of $(u, s, t)$ is $1 + 7 + 2 = 10$. There is no simple principle that governs the presence or absence of such special norm 4 or norm 10 vectors on ESDL’s. Hence there may exist chiral covariant
lattice theories without any moduli (other than the dilaton), but they would be nearly impossible to find by any known methods.

The main reason moduli are discussed here is that they led, in the beginning of this century, to a famous big number in string theory. It was conjectured (based on flux compactifications and many other ingredients) that the long-standing problem of stabilizing them has a huge number of solutions: the famous number $10^{500}$ [6, 7]. As already stated, this number has nothing to do with the large numbers discussed earlier. It is also not correct to say that the large number of string compactifications found in the eighties is merely a subset of the $10^{500}$. The different string compactifications generically have different chiral properties and hence are in different moduli spaces, although undoubtedly there will be cases that lie in the same moduli space. Roughly speaking the total number of string compactifications is more like the product of both large numbers, or more precisely (but still not precisely enough) a sum over the number of stabilized vacua for each moduli space. Based on what I know today I would expect the first large number to be considerably smaller than the second. If we write these numbers as $10^k$, my best guess at the moment is that $k$ is of order 10 to 50 for the number of moduli spaces, whereas it may be as large as $272,000$ [8] for the maximal number of vacua per moduli space. However, in the latter case it should be noted that the existence none of these moduli-stabilized deSitter vacua is generally accepted. See [37] for a recent discussion and references.

4 Vacuum scanning

The rest of this paper is about dealing with big numbers. The four-dimensional string constructions that emerged in 1986 almost invited writing computer programs scanning these huge spaces. The two constructions most amenable for computerized scanning are free fermion constructions [15, 26] and RCFT tensor products. In fact, free fermion constructions can be viewed as a special case of RCFT tensor products, with the Ising model as a building block. There are numerous papers on parts of the free-fermionic landscape, see for example [38] and references therein, and [39] for pioneering work on free fermion vacuum scanning. But what I really mean by RCFT constructions are tensor products of $N = 2$ minimal models, also known as “Gepner models” [40]. These are the main topic of this chapter. Other building blocks can be considered, but outside this area the terrain is largely undeveloped. Essential pieces of formalism are missing, and the few attempts at venturing here have not anything spectacularly different or vastly larger. The only cases that have been considered are Kazama-Suzuki models [41], explored in [42] and permutation orbifolds [43, 44]. There are also many articles on vacuum scanning using orbifold methods, both in heterotic strings and in orientifolds. The same is true for the geometric approach, where Calabi-Yau manifolds defined a large scannable class of manifolds. Here especially the work of Kreuzer and Skarke [45] stands out. This section is not intended as a review of vacuum scanning, but only to give some personal reflections based on my own experiences. But many other people contributed to this subject, see e.g. [46] [47, 48, 49] for various perspectives and further references. Many more references
can be found in my review article [5].

4.1 The goal of vacuum scanning

I use the term “vacuum scanning” because I assume that the results of these scans give us some relevant information about actual metastable ground states of string theory, usually called “vacua”. Some people question the existence of such ground states, especially people who do not like the overwhelmingly large landscape of vacua that string theory suggests. But if string theory is relevant for our universe, it must contain at least one “ground state”, no matter how one chooses to define it, that describes the Standard Model. Vacuum scanning searches for anything that has the same general features, but differences in gauge interactions and matter. If there is a metastable Standard Model ground state, there is no reason why the alternatives should not exist. One may prefer to wait for a mathematically rigorous proof of the existence of one or more metastable deSitter vacua with gauge symmetries and chiral matter, but that is not how we usually make progress in physics. Then one could also argue that we should not even have started thinking about string phenomenology without first developing a full non-perturbative description of string theory.

But why are we doing this? I want to make it clear that for me at least, and based on our present knowledge, finding the Standard Model is not the goal of vacuum scanning. This is not an achievable goal at the moment because too many features of the desirable vacuum cannot be taken into account. This includes moduli stabilization, supersymmetry breaking and the gauge hierarchy. An even more serious problem is vacuum energy, which in any string theory with broken supersymmetry comes out in Planck units. One may hope that these issues can be factored out of the problem and dealt with separately, but that is just wishful thinking at the moment.

The features most likely to survive are gauge symmetries and chiral matter. But even if we get those right, the second problem is that we do not really have any idea how much of the landscape we are missing. Any known construction comes with built-in limitations. Since we know that in the end we are dealing with large numbers, the part we are missing in any scan can be huge, and can usually not be estimated. A nice toy example is provided by the discussion in the appendix. Here a popular construction method, free complex fermions with (anti)-periodic boundary conditions is shown to cover only a fraction of less than $10^{-370}$ of a larger landscape with a size of order $10^{1000}$. This does not prove anything about the actual string landscape, but it serves as a warning.

The other reason for doing vacuum scans is in order to gain statistical information about the landscape. This might enable us one day to postdict certain features of the Standard Model, and perhaps even make predictions. The fact that these predictions are statistical in nature is not a valid argument against them. All predictions in physics are based on statistics. There is a chance of about 1 in $10^{10}$ that the Higgs boson does not exist, despite all observations. Given the conjectured number of vacua in the landscape, it is not unthinkable that one feature stands out by a statistical factor of much more than $10^{10}$. Statistical methods were advocated especially by M. Douglas [50]. However, our
present knowledge is still too crude to expect practical applications, other than getting a rough idea which features might need new physics, and which ones might just come out right. For some pioneering work on vacuum scanning and statistics see e.g. [51, 52, 53, 54].

At this stage, the real reason for doing vacuum scanning is usually just to get a picture of a previously unexplored region in the landscape. Just as pictures of astrophysical objects, it is important to get as high a resolution as possible. This allows rare features to become visible, and helps in deciding if all of the features of the Standard Model can be realized, and if they can all be realized simultaneously. But just as one should not expect to understand the entire universe from a single high-resolution image, one should not expect to understand the entire landscape from vacuum scanning of some region.

4.2 The computer program Kac

While working out examples for [1] we used a computer program to generate covariant lattices. The example we produced had chiral spectra, $N = 1$ space-time supersymmetry or no supersymmetry, and orthogonal gauge groups. This computer program has not been preserved. It is therefore hard to say if using modern computers to search more deeply we would be able to get anything similar to the standard model. However, three family covariant lattice models do exist, see below.

The foundation for future work on “vacuum scanning” was laid in the years 1987-1989. I worked with Shimon Yankielowicz [55] on properties of fusion rules. This led to an idea which we called “simple currents”, discovered independently in [56], and which I will say more about below. I made a computer program that could work out the Kac-Peterson [57] formula for the modular transformations of affine Lie algebras, and that could compute their fusion rules using the Verlinde formula [58]. I made the good decision to preserve these algorithms by making this program future-proof. It has a command line interface and an automatically generated help system, so that it was possible to remember how to run it even long after publication of the paper it was used for. Previous programs I had written were ad-hoc, written for a specific computation only, and would greet the user with messages like “Enter N”, without explaining what “N” was.

The program slowly evolved to an extensive C-program I named “Kac” (an acronym for “Komputations with algebras and currents”). It can compute spectra of tensor products and coset models, work out the consequences of field identification [59] and the resolution of their fixed points [60] [61], compute simple current modular invariants [62] [63] [64], and the modular transformations of simple current extended tensor products [65]. At a later stage, it was extended to boundary conformal field theory, building on work by the Tor Vergata group [66], which after several steps culminated in a general formula [67]. The program incorporates a long list of algorithms developed in the papers mentioned here and others.
4.3 Heterotic Gepner Models

However the first vacuum scanning project did not use the program Kac yet. It was a scan of all simple current modular invariants of Gepner models [68]. The spectra this produced had chiral $SO(10)$ or $E_6$ gauge group with $N$ families. To our disappointment we did not find any cases with $N = 3$, other than the “exceptional” example that Gepner had already found [69]. At the time of this work the full set of simple current invariants was not known yet. Their full classification only became available in 1993 [64]. Instead, we multiplied single current modular invariants. This was done by multiplying a set of matrices, each generated by a single current $J$. In the tensor product such a current $J$ is a vector $(J_1, \ldots, J_M)$, where $J_i$ is a simple current of a factor in the $N = 2$ tensor product, and $M$ is the number of factors, determined by the requirement that the total central charge be equal to 9. For an $N = 2$ minimal model factor with level $k$, there are $4k + 2$ choices for $J_i$. This leads to a huge number of choices for $(J_1, \ldots, J_M)$. On top of this one has to make such a choice for each factor in the modular matrix product. We considered products of up to 9 matrices. Since this space was too large to explore systematically, we used a stochastic procedure. We made random choices for the current components $J_i$ in each matrix. We observed that the list of spectra saturated after a while, and assumed that this meant that we had essentially explored the entire set.

The main outcome of this work was that the number of families in each minimal model combination was quantized, usually in units of 6 and 4. This made the conclusion that there were no three-family spectra far more convincing.

The full results of the scan were archived at CERN for the longest time the system allowed, which turned out be 1999. But already many years before that year they had already disappeared. They are still available in paper form, but unfortunately not in scanned or electronic form. This list still turned out to useful later, when we (with Beatriz Gato-Rivera) returned to the problem [70] to implement some extra feature: we allowed breaking of world-sheet symmetry in the sector of the theory that maps to a bosonic string. We also allowed the $E_6$ gauge group to be broken to an $SU(3) \times SU(2) \times U(1)$ (times other factors) subgroup. All of these possibilities were already mentioned in the 1989 paper, but at that time running this algorithm cost about 80 seconds per spectrum, and only a few spectra were collected. We could only run on the CERN IBM, using quota shared by the entire theory division. Exceeding these quota implied that the entire theory division would not be able to login anymore, and this was enough reason to abandon the project. However, already enough spectra had been collected to make the disturbing observation that they always contained particles with fractional electric charge. This observation could be turned into a theorem [71].

When we returned to the heterotic Gepner models more than 20 years later, the computer program we used in 1989 was not available anymore, but fortunately I had meanwhile developed the program Kac, which was capable of building $N = 2$ minimal models as coset CFT’s, work out their field identification, tensor them and compute the full set of simple current invariants. It also takes into account permutation symmetries of identical factors, and the charge conjugation symmetries of each factor. For all of the
168 tensor products of minimal models with $c = 9$, this program computes all distinct MIPFs in just a few seconds, and hence the entire 1989 computation can be reproduced very quickly. Since we still had the old results in paper form, we could check our new results, which were obtained in a completely independent way. They agreed, and this also showed that for the $N = 1 E_6$ models (the ones with a Calabi-Yau like spectrum) the 1989 scan was essentially complete.

The new features, broken $E_6$ and worldsheet supersymmetry, could not be scanned completely. To appreciate how much more difficult this is, consider a formula derived in [62] for the number of distinct modular invariant partition functions for a simple current group $(\mathbb{Z}_p)^k$:

$$N_{\text{MIPF}} = \prod_{\ell=0}^{k-1} (1 + p^\ell)$$

where $p$ is prime. The analogous formula for discrete factors $\mathbb{Z}_{p^n}$ is unknown; if there are different prime factors with a single power one gets a factor for each prime. A well-known combination of $N = 2$ factors is $(3,3,3,3,3)$, five factors of $N = 2$ models with $k = 3$. This lies in the moduli space of the quintic Calabi-Yau manifold. If we leave $SO(10)$ unbroken, this has a simple current group $(\mathbb{Z}_{20})^5 \times \mathbb{Z}_4$. But if we write this in terms of characters of the $SU(3) \times SU(2) \times U(1)^2$ subgroup of $SO(10)$ the discrete group enlarges to $(\mathbb{Z}_{20})^6 \times \mathbb{Z}_{30} \times \mathbb{Z}_3 \times \mathbb{Z}_2$. Instead of five $\mathbb{Z}_5$ factors, this has seven. This alone increases the number of possibilities by a factor $(1 + 5^5)(1 + 5^6) = 48846876$. This required us to resort to stochastic methods once again.

The results for broken $E_6$ were again somewhat disappointing. There was an even clearer family number quantization, which cases that previously only had quanta of 12 or 24 also showing new spectra quantized in units of 6. But there were no three family spectra. To my knowledge the origin of family quantization in these models has never been clarified.

But then in two subsequent papers we added another ingredient. This was to replace an $N = 2$ factor in the bosonic sector of the heterotic string by an entirely different CFT with the same modular transformation properties, a trick we called “heterotic weight lifting” [72]. It turns out that this completely removed family quantization, and allowed us to get three family spectra in abundance. Family quantization in units of merely 4 and 6 turned out to be an artifact of the left-right constructions considered until then, and using heterotic weight lifting and a closely related method applied to the superfluous $B - L$ symmetry we explored [73] [74] an entirely novel part of the heterotic landscape.

As a by-product three family models were found for supercurrents of the form

$$(v, 0, \ldots, 0, \sqrt{3}, 0, \ldots, 0).$$

These were obtained as tensor products of $k = 1, N = 2$ minimal models. These are examples of Gepner models, but with the special property that they can be written in terms of free bosons, and hence as covariant lattices.
4.4 Gepner Orientifolds

The roots of my involvement with open string or orientifold model building can also be traced back to the late eighties: to the paper by Eric Verlinde [58] leading to the “Verlinde formula” for fusion rules, the paper by Cardy [75] that related the Verlinde formula to boundary conformal field theory, and my own work with S. Yankielowicz on simple currents [55] as well as our work on fixed point resolution [60]. The link with fixed point resolution was made by the Rome group, as early as 1991 [76]. During the last decade of last century this link was strengthened, and it became clear that the fixed point resolution matrices played an essential role in the computation of boundary reflection coefficients in open string conformal field theory. Meanwhile the formalism for computing such matrices had been developed further in [77, 65], and the formalism for computing boundary coefficients for non-diagonal modular invariants was worked out in various examples, see e.g. [78, 79, 80, 81]. A condition for the completeness of boundaries was formulated in [66]. The fixed point resolution formalism was applied to open string boundaries in [82].

Another ingredient that was needed were crosscap coefficients for orientifold planes, as well as a CFT characterization of all such planes. The foundation for this work was given in another paper by the Rome group [78]. We started the generalization of this work to general simple current MIPFs, as well as simple current related Klein bottles in [83, 84]. Meanwhile the boundary state formalism was also generalized to other simple current MIPFs [85, 86, 87]. It was time for a grand synthesis, which was finally achieved in [67].

After about a decade of work by many people we had a general formalism that allowed us to compute string spectra for all simple current MIPFs and for all simple current related orientifold choices. Furthermore the entire formalism had been built into my computer program Kac, in order to check it for consistency, for example to check the integrality of all partition function coefficients. At the end of the year 2000 this formalism could have been applied straightforwardly to vacuum scanning in Gepner orientifolds. Examples of such orientifolds had already been worked out in 1996 [88], but without any aim at Standard Model phenomenology. On the other hand, orbifold-orientifold methods starting being used with some success. In [89] the Standard Model was obtained from intersecting branes, but without requiring stability. A remaining challenge was to obtain a stable, supersymmetric realization with full cancellation of all tadpoles. When this still had not been achieved in 2004 we decided to see if our formalism could make a contribution.

It would be an exaggeration to say that this was merely a matter of pushing a button, but it is true that all ingredients were already in place. The main work that was needed were several optimizations in order to make the computations doable. We were facing several large numbers. First of all there are 168 combinations of $N = 2$ minimal models. Each has hundreds or thousands of primary fields. We wanted to consider all simple current MIPFs. There are 5403 in total, for the 168 Gepner models combined. For each MIPF there is a handful to a few tens of orientifold choices. This gives a grand total of 49304. For each of these one has to compute all possible boundary coefficients. Typically there are hundreds or thousands of boundaries. A standard model configuration is obtained by combining four sets of boundaries, in such a way that open strings ending on
these boundaries produce a massless spectrum that corresponds to the Standard Model.

We aimed for a set of realizations closely related to the one first written down by the Madrid group [89]. These “Madrid models” are build out of four intersecting branes stacks: a $U(3)$ stack that produces QCD and baryon number, a $U(1)$ stack that yields lepton number, a third stack to produce the weak interaction gauge group $SU(2)$ and a fourth one providing endpoint for open string that yield the quark and lepton weak singlets. The third stack can be $U(2)$ or $Sp(2)$, and the fourth one $U(1), O(2)$ or $Sp(2)$ (in the latter case there is an additional $SU(2)_R$ factor in the gauge group). In Gepner orientifolds these stacks are realized as RFCT boundary states, but one can describe the resulting configuration using the more intuitive language of intersecting branes. The boundary states are either unitary, orthogonal and symplectic, and one can count the total number of combinations that have the required structure, prior to demanding a particular stack multiplicity. In table 2 we show some counts for the six possible combinations of stacks. The column “Total number” gives their total, summed over all orientifolds, MIPFs and orientifold choices. The grand total is $4.5 \times 10^{19}$.

<table>
<thead>
<tr>
<th>Type</th>
<th>Total number</th>
<th>Number searched</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
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<td>187171389940312068</td>
<td>99.75%</td>
</tr>
<tr>
<td>UUUU (1+7)</td>
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</tr>
<tr>
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</tr>
<tr>
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<td>2792296847030752</td>
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</tr>
<tr>
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<td>9019267374778532</td>
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</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>45761187347637742772</strong></td>
<td><strong>43752168618082181524</strong></td>
<td><strong>95.61%</strong></td>
</tr>
</tbody>
</table>

Table 2: Gepner Orientifold models by type: search statistics

All these brane stack combinations must then be subject to two constraints. If we give them the Standard Model stack multiplicities, their total contribution to the dilaton tadpole must be less than or equal to the contribution of the orientifold plane; furthermore their chiral intersections must reproduce the Standard Model spectrum. Especially the latter check is computationally expensive. It involves computing annulus coefficients of the form

$$A_{ab}^i = \sum_m \frac{S_m^i R_{am} R_{bm}}{S_{0m}}$$  \hspace{1cm} (9)$$

Here $a$ and $b$ are the boundary labels, and $m$ is a label of an “Ishibahi state”. These are the closed string states that can propagate between boundary and orientifold states given a certain choice of MIPF. These integers $A_{ab}^i$ give the number of times a state with CFT characters $\chi_i$ appears for the given boundaries $a$ and $b$. Now one expands the characters and looks for massless states. Summing over these gives the total number of massless states in the open strings stretching between boundary states $a$ and $b$. The
chirality of these states is also known, and the net chiral states are compared to the Standard Model. The matrix $S$ is the modular transformation matrix of the Gepner model under consideration, and the matrices $R$ depend on $S$ and the MIPF. The matrix $S$ can be obtained straightforwardly from the modular transformation matrices of the $N = 2$ minimal models making up the Gepner model, but the resulting matrix can be huge. In the worst case, occurring for the tensor product $(1, 5, 82, 82)$, it is a $108612 \times 108612$ matrix. Another computational bottleneck is the $P$-matrix \cite{13} that appears in the computation of the Moebius and Klein bottle amplitudes. This matrix is defined as $P = \sqrt{T}ST^2S\sqrt{T}$, where $T$ is the generator of the modular transformation $\tau \rightarrow \tau + 1$. Since it involves multiplication of large matrices it is preferable to store it, but for a CFT with 106812 primaries this requires huge amounts of storage space.

Initially, the computation was done on the cluster of desktops of the Nikhef theory group. Our goal was to find the first examples of exact, well-defined (and hence supersymmetric) open string theories reproducing the Standard Model chirally. Although work on other methods (especially orbifold orientifolds) was approaching that goal, it was surprisingly hard to satisfy all conditions, and all papers before 2004 made some compromises. We found the first example fairly soon after starting the project. It occurred for the tensor product $(3, 8, 8, 8)$. We had already understood that there was an additional constraint worth imposing, namely the absence of a massless $U(1)$ gauge boson corresponding to $B - L$. The presence of such an additional $B - L$-photon is in contradiction with experiment, but one could try to appeal to some low-energy Higgs-like mechanism to make it massive. However, there is a known way to give a mass to this gauge boson already at string level, namely axion mixing, which produces a Stueckelberg-like mechanism. We built a check for axion mixing into the program, and just a few days after the first success we found an example with a massive $B - L$ photon. We worked out a few more examples, and produced a short paper \cite{14} about this as soon as possible, because we were feeling the pressure of the competition.

It was our intention to really search all cases, or at least push it as far as we could. We succeeded in pushing it very far indeed thanks to the extensive use of simple current methods. This organizes all of the aforementioned labels into “orbits”. The boundary and Ishibashi states are labelled by an orbit label $\ell$ and a simple current $J$. The computationally difficult part is the calculation of all relevant matrices $S, R$ and $P$ as well as the annulus coefficients between orbits; the dependence on the current $J$ can be worked out analytically. In the example mentioned above the 108612 labels are organized into just 2646 orbits. This leads to a reduction by a factor $50^3$ for the computation of the matrix $P$, and a reduction by a factor 2500 in the required storage space. We were in the fortunate situation that Nikhef was building a cluster that was part of the GRID, to be used for LHC data analysis. We were able to use this facility, because LHC had not started yet. We even made a minor contribution to LHC physics by putting this system to the test. Indeed, we found some bugs. At one point my student Tim Dijkstra, using the WiFi of the hotel he was staying in during a conference in Madrid, had to remove 18000 mail messages from his mailbox. These were crash reports sent erroneously by the GRID software. But despite the huge computer power, in some cases the simple cur-
rent reduction simply was not sufficient, and we had to give up. Two of the 168 Gepner combinations were not considered at all (namely (1,5,42,922) and (1,5,43,628)) and for four others we only considered models of types USUU and UUUU. The total number and percentage of configurations that was checked is shown in table 2.

But this was still not the end of the work. Open string models must satisfy tadpole cancellation constraints, for reasons of consistency and stability. Before we had even started the search we had decided that we would allow additional branes (a “hidden sector”) to achieve the tadpole cancellation. These were required not to have a chiral intersection with the Standard Model. So all solutions to the Standard Model spectrum constraints were checked for the existence of tadpole cancelling hidden sectors. It was clear very quickly that pursuing that until the end was impossible. In a given Gepner model there can be thousands of candidate hidden sector branes. Even after imposing the absence of chiral intersections with the Standard Model, there can be hundreds left. Each such brane can occur with a maximal multiplicity of order 10. So one is confronted with $10^{\text{hundreds}}$ of options, that cannot be searched systematically. We were hit directly by the Big Number problem because the computer program experienced some mysterious segmentation faults each time it was starting to explore a very large hidden sector. We finally discovered that this was due to a message stating something like “remaining time .... years; aborting”. The number of years, expressed as an unlimited size integer, was so large that it overflowed the text buffer allocated for it.

We developed a variety of strategies aimed at finding at least one solution per Standard Model configuration. We ended up with about 200,000 distinct solutions. This number was obtained after removing many spectra that were identical to others. Searches of these kind have large numbers of degeneracies. Some of them can be taken into account in advance to reduce the search effort, some have a known origin, but can be taken into account most easily \textit{a posteriori}, and there are also some unidentified degeneracies that can only be removed by comparing spectra. Table 3 gives the result for each type (types 6 and 7 are the same as 0 and 1, but with massive $B - L$). Column two gives the number of Standard Model configurations, expressed in column three as a fraction of the total. Column four and five give the same information including tadpole cancellation. These numbers are prior to removing degeneracies.

After the paper was published we understood that there was another constraint that should be imposed, namely the “K-theory” constraint, related to global anomalies. In [91] Angel Uranga presented a convenient formalism using “probe branes” to take these constraints into account. We did this in [92]. It required a rescan of the entire database, and there was a possibility that the number of tadpole solutions would be decimated by the additional constraint. But this did not happen. Quite the contrary, because in a few cases we were able to push the tadpole cancelation search algorithm a bit further we ended with even more solutions, 211634 in total (after removing degeneracies).

So far these searches were limited to a particular kind of Standard Model realizations, related to the Madrid model. In [93] we extended the search to essentially any kind of brane configuration with at most four brane stacks. In particular we allowed arbitrary
choices for the embedding of the Standard Model hypercharge in the brane stack charges. We found that the bulk of the possible models belonged to three classes, distinguished by the contribution that the $U(1)$ in the $SU(3)$ stack makes to the hypercharge. This contribution can be either $\frac{1}{6}$ (this includes Pati-Salam models and flipped $SU(5)$), or $-\frac{1}{3}$ (including $SU(5)$ GUTs). There is also a class where this fraction is not fixed by the Standard Model particle charges because the massless spectrum is entirely built out of unoriented strings. This implies that one can add charge to one open string endpoint and subtract it at the other end, keeping all particle charges unchanged.

This scan was considerably harder than the Madrid model scan, because in the latter case we could make use of the fact that the baryon/QCD brane and the lepton brane have the same intersections with all others. They only differ in their Chan-Paton multiplicities. This made it possible to search for candidate baryon and lepton branes in one go, essentially reducing the number of nested loops in the Standard Model search from four to three. For this reason the generalized search could not be pushed as far as the Madrid model we search. We decided to impose a cut-off on the number of boundary states at 1750. The scan yielded 19345 chirally distinct realizations of the Standard Model. This includes the eight classes already discussed above. Each of the 19345 classes may contain thousands of explicit models, each with a different non-chiral spectrum. Indeed, the largest class has about 10 million members (this class has a rather unappealing $U(3) \times Sp(2) \times Sp(6) \times U(1)$ gauge group, with $Sp(6)$ containing a flavor symmetry group). Chirally distinct models all have a different phenomenology; one could write 16345 separate papers about them.

The results of this scan were stored in order to make them maximally reproducible. It is now possible to display one of the 19345 models by simply giving a unique spectrum identification number. It is also possible to reload all the models and check for further criteria. This database proved useful in later work on instanton-induced neutrino masses [94], $SU(5)$-Yukawa couplings and proton decay [95, 96, 97] and discrete symmetries [98].

We also investigated free fermion orientifolds using the same criteria [99]. This search produced no three-family models. We also searched for tachyon-free non-supersymmetric

<table>
<thead>
<tr>
<th>Type</th>
<th>SMs found</th>
<th>Fraction</th>
<th>Tadpoles solved</th>
<th>Fraction</th>
</tr>
</thead>
<tbody>
<tr>
<td>USUU (0)</td>
<td>1096682</td>
<td>$5.9 \times 10^{-12}$</td>
<td>215846</td>
<td>$2.7 \times 10^{-13}$</td>
</tr>
<tr>
<td>USUU (6)</td>
<td>49794</td>
<td>$2.7 \times 10^{-13}$</td>
<td>4468</td>
<td>$2.4 \times 10^{-14}$</td>
</tr>
<tr>
<td>UUUU (1)</td>
<td>131704</td>
<td>$3.1 \times 10^{-15}$</td>
<td>1280</td>
<td>$3.0 \times 10^{-17}$</td>
</tr>
<tr>
<td>UUUU (7)</td>
<td>1306</td>
<td>$3.1 \times 10^{-17}$</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>USOU (2)</td>
<td>947494</td>
<td>$1.1 \times 10^{-10}$</td>
<td>431633</td>
<td>$4.8 \times 10^{-12}$</td>
</tr>
<tr>
<td>UUOU (3)</td>
<td>16891580</td>
<td>$2.3 \times 10^{-11}$</td>
<td>12533</td>
<td>$1.7 \times 10^{-14}$</td>
</tr>
<tr>
<td>USSU (4)</td>
<td>16227372</td>
<td>$5.8 \times 10^{-9}$</td>
<td>978200</td>
<td>$3.5 \times 10^{-10}$</td>
</tr>
<tr>
<td>UUSU (5)</td>
<td>1178970</td>
<td>$1.3 \times 10^{-11}$</td>
<td>5682</td>
<td>$6.3 \times 10^{-14}$</td>
</tr>
<tr>
<td>Total</td>
<td>45051902</td>
<td>$1.0 \times 10^{-12}$</td>
<td>1649642</td>
<td>$3.8 \times 10^{-14}$</td>
</tr>
</tbody>
</table>

Table 3: Gepner Orientifold models by type: success rates
orientifolds \[100, 101\]. This was only partly successful. The combined constraints of getting the Standard Model spectrum, removing all tachyons and cancelling all tadpoles could not be satisfied within the sample we considered, but there are solutions when any of the three constraints is dropped. We suspect that with a sufficiently large sample all constraints can be met.

5 Final thoughts

Thirty years after the first signs of a huge landscape started emerging, we still have no idea how big it really is. Current estimates range from 0 to $10^{272000}$ \[8\]. The lower bound is based on the actual number of deSitter vacua that has been demonstrated to exist in a way that convinces everyone.

For a recent discussion on the existence of dS vacua, especially in the KKLT construction \[102\], see \[37\]. This paper comes out in favor of the conclusions of KKLT, but references to skeptics can also be found here. The fact that I only cite this single paper is because it is a convenient entry point to the most recent discussion, and not because I agree with its conclusion; I am simply unable to decide. As far as I can tell, few people expect the actual number of dS vacua in string theory to be zero, unless there is an overlooked no-go theorem. Most skeptics attack specific parts of certain constructions, such as brane-antibrane uplifting in KKLT. But even if they are only partly right, an explicit dS vacuum may be subject to a very complicated set of constraints that is hard to satisfy explicitly. For example, suppose the number $10^{272000}$ is reduced by a factor $10^{271000}$ due to constraints that are very unlikely to be satisfied. There would still be $10^{1000}$ vacua left, but they would be impossible to find; it might well be impossible to demonstrate that any exist.

A large landscape is not as problematic as a poorly distributed one. If it is simply large it might be possible to show by means of statistical distributions that the Standard Model has to be in there somewhere. Such a proof would inevitably imply that it is realized a huge number of times. Presumably each realization would give different predictions for new physics or additional digits in observed masses and couplings. This would not be most people’s dream of the ultimate theory, but if such an argument is combined with a proof that gravity requires string theory (or some generalization), and that string theory implies a well-distributed landscape of gauge theories, that would be the end of the story.

But a distribution with huge holes would be the worst possible outcome, even if the total number of vacua is far less than $10^{500}$. We would not be able to demonstrate the presence of the Standard Model statistically, nor find it explicitly.

There are also positive sides to big numbers. An obvious one is that a large landscape is required to explain fine-tuning. A number larger than about $10^{120}$ is needed for the multiverse explanation of the smallness of the cosmological constant \[103, 104\], and a substantial number of vacua is needed to demystify anthropic tunings in the Standard Model parameter space. A less obvious, and rather speculative advantage is that big numbers may help us make predictions and falsifications. Big numbers may lead to vastly
different landscape probabilities for certain features or regions of the landscape, making all but a single one of them statistically relevant. A big hierarchy of probabilities may result either from landscape statistics (vacuum counting) but also from multiverse probabilities, see e.g. [105][106].

One may even envisage a scenario where anthropic arguments are in peaceful coexistence with essentially unique predictions. Naively one would expect that a landscape explanation of anthropic fine-tunings always requires a large ensemble of vacua, and hence necessarily a loss of predictive power. There would not be just one anthropic vacuum, because that by itself would require a mysterious fine-tuning. However, if there is a large hierarchy of probabilities the statistically dominant anthropic vacuum may occur extremely rarely in the multiverse, but it may still be vastly more likely than the next one. This would lead to a very strong prediction, but it would still let the landscape play its rôle as an explanation for anthropic fine-tunings. See sections III.F.4 and III.F.5 of [5] for more discussion of this kind of scenario, as well as a serious potential problem, also discussed below.

This may sound like fantasy, but consider the results of [107] on a class of flux compactifications. These authors find a huge exponential suppression of the number of vacua when the rank of the gauge group is increased. Let us, inspired by this result, suppose that going from rank 4 to rank 5 costs a factor $10^{-1000}$. If one can find a convincing anthropic argument that at least rank 4 is required, one would get a very strong prediction of the rank of the gauge group by a combination of landscape statistics and anthropics.

Anthropic arguments of this kind have been made in the limited setting of intersecting brane models with at most two stacks [108]. Here indeed the Standard Model gauge group clearly stands out among its competitors, on the basis of molecular complexity. If one adds more stacks, other options become available, but extra stacks come at a statistical price. However, this cost may not be the extreme exponential fall-off of the previous paragraph.

That is just as well, because here is also a downside to this sort of argument. It may push us deep into the tail of anthropic distributions. Consider for example the Standard Model gauge group. One may imagine anthropic arguments for the necessity of QCD and QED, but the weakest link is the necessity of the weak interactions [109]. One could argue that it is needed to provide chirality to protect quark and lepton masses. This might be true is chirality plus a single light scalar boson is statistically cheaper than having three or more light fermions. This may be against standard lore about technical naturalness, but not against what we know about landscape distributions for fermion masses and Yukawa couplings. It also fits well with the “just the Higgs” results of LHC (so far). However, this assumed statistical advantage of the “just the Higgs” scenario over alternatives (Dirac masses, composite models, supersymmetry, etc.) will have to overcome any statistical gain obtained by dropping the weak interactions altogether. If there is a strong exponential rank dependence as suggested above, the statistical gain due to rank reduction might be something like $10^{1000}$. This may turn apparently unlikely options into the statistically preferred ones.

Another interesting potential consequence of big numbers was recently suggested in
These authors argue that F-theory with one specific Calabi-Yau fourfold dominates everything else by factors as large as $10^{3000}$, simply by having the largest number of stabilized vacua (namely about $10^{272000}$). It predicts that the fundamental gauge group is $E_8^0 \times F_4^8 \times (G_2 \times SU(2))^{16}$, making a mockery of Grand Unification. It also appears to predict large numbers of dark matter sectors with non-abelian interactions, originating from the same group. If this kind of dark matter has generic observable signatures this has the potential of falsifying the entire landscape. If this particular F-theory does not contain the Standard Model, but does contain another anthropic gauge theory, this would also falsify the entire landscape. For example, suppose one can get $SU(3) \times SU(2) \times U(1)$, but only with an even number of families. Then the question whether the exact Standard Model with three families is realized in another part of the landscape becomes irrelevant. Perhaps for unknown reasons life with two families is somewhat challenged, but probably not by a factor $10^{-3000}$. So then the landscape would strongly predict two or four families, in disagreement with the data.

The authors of [8] caution “we do not claim that we have proved anything here”, but if they are right it would make all attempts at vacuum scanning performed so far utterly irrelevant. Not only would these scans have had no chance of finding “the” Standard Model (I took that for granted already), but it would be highly questionable if anything significant can be learned about the distribution of physical quantities in the landscape by considering statistically insignificant subsets.

Big numbers could make everything in this paper irrelevant. They could help falsify the string landscape and thereby string theory as well. But they might also provide the key to its ultimate vindication.

Acknowledgements

I would like to thank Dieter Lüst for reading the manuscript. This work has been partially supported by funding of the Spanish Ministerio de Economía y Competitividad, Research Project FIS2012-38816, and by the Project CONSOLIDER-INGENIO 2010, Programme CPAN (CSD2007-00042).

A Euclidean ESDL’s and free fermions

In this appendix we show that not all Euclidean even self-dual lattices (ESDL’s) can be written in terms of complex free fermions with periodic and anti-periodic boundary conditions. The basic idea of the proof is to compute an upper limit to the total number $F_{8k}$ of fermionic theories in $8k$ dimensions, and show that for sufficiently large $k$ this upper limit is smaller than the lower limit on ESDL’s.
If one constructs a modular invariant CFT in $8k$ dimensions out of complex free fermions, one obtains a CFT with $8k$ independent free bosons. The set of physical states of that CFT is described by momentum states of each of these bosons. These form vectors in $8k$ dimensions. Closure of the operator product expansion requires these vectors to close under addition. Hence they lie on a lattice. Modular invariance requires each of the lattice momentum states to appear with the same multiplicity as the vacuum state, i.e. 1. The lattice vectors are equal to $0, \pm \frac{1}{2}$ or 1 modulo 2, because of the free fermion boundary conditions.

If follows that the free fermionic ESDL can be built as direct sum of one-dimensional components that are $D_1$ lattices (see [110] for a review of lattice constructions). Those lattices have a root lattice consisting of all the even integers, and four conjugacy classes of weights represented by the one-dimensional vectors $0, \pm \frac{1}{2}$ and 1. These classes are usually (by analogy with $D_n$) labelled as (0), (s), (c) and (v) respectively. The $D_1$ root lattice has a basic cell consisting of the interval $[0, 2)$, which has volume 2. Its dual lattice, the weight lattice, has volume $\frac{1}{2}$. The full ESDL is described by a list of conjugacy classes (called “glue vectors” by mathematicians) of the form

\[(x_1, \ldots, x_{8k})\]

where $x_i$ are the conjugacy classes (0), (v), (s) or (c) of $D_1$. These vectors must have integral inner products, even norm, and they must reduce the size of the unit cell from $2^{8k}$ to 1. If these conditions are met we have obtained an ESDL. If we add a glue vector of order $N$ to a lattice, this reduces the volume of the unit cell by a factor $N$. For example, to a single $D_1$ root lattice we can add a vector conjugacy class. This has order 2 and reduces the volume of the unit cell to 1. We can also add a spinor conjugacy class. This has order 4 and reduces to volume of the unit cell to $\frac{1}{2}$.

If while building up a set of glue vectors we add one of order 1, then this means that it is dependent on the previous ones and should not be considered. Therefore every factor must have at least order 2, and hence reduce the unit cell volume by a factor 2. Therefore there can be at most $8k$ glue vectors. Then each fermionic ESDL’s in $8k$ dimensions can be specified by an $8k \times 8k$ matrix with entries (0), (v), (s) or (c). Hence an upper limit on the number of fermionic ESDL’s in $8k$ dimensions is

\[F_{8k} < 4^{64k^2} \equiv G_{8k}\] (11)

Although this is an extremely loose upper limit, it is sufficient to show that for sufficiently large $k$ the number $L_{8k}$ defined in (4) overwhelms $F_{8k}$. To see why, consider the increment of these numbers from $k$ to $k+1$. We will make use of the following exact formula for the Bernoulli numbers

\[\|B_{2n}\| = \frac{2(2n)!}{(2\pi)^{2n}} \left[ \sum_{\ell=1}^{\infty} (\ell)^{-2n} \right]\] (12)

For large $n$ this is extremely well approximated by just the first term in the sum, and furthermore the exact Bernoulli numbers are slightly larger than the approximation. Using
this approximation we find

\[
\frac{L_{8(k+1)}}{L_{8k}} \approx \frac{(8k)!(8k + 2)!(8k + 4)!(8k + 6)!}{4^4(2\pi)^{32k+16}}
\]

(13)

On the other hand, the increment of \(G_{8k}\) is

\[
\frac{G_{8(k+1)}}{G_{8k}} = 4^64(2k+1)
\]

(14)

A factorial \(p!\) is always larger than \(x^p\), for any given \(x\) and sufficiently large \(p\); the four factorials in (12) are just overkill. Hence the increment of \(L_{8k}\) eventually becomes larger than the increment in the \(G_{8k}\), and therefore for sufficiently large \(k\) we have \(L_{8k} > G_{8k}\). Numerically, \(G_{8k}\) is much larger than \(L_{8k}\) for small \(k\), but \(L_{8k}\) surpasses \(G_{8k}\) for \(k > 902\).

It possible to arrive at an estimate that is a lot tighter than (11). For any given lattice we can start by collecting independent basis vectors as follows. First choose a vector with a spinor entry \(s\). We can interchange \(D_1\) factors such that \(s\) is the first entry. Now consider all remaining vectors. By making linear combinations we can nullify their first entry. In this set vectors, take one that contains an entry \(s\), if there is one. We commute that entry to the second position, and subtract it from the first vector to nullify it on the second position.

We can continue doing that until we have a set of \(N\) vectors of the form

\[
\begin{align*}
(s, 0, 0, 0, \ldots, 0, \ldots, x_{8k}^1) \\
(0, s, 0, 0, \ldots, 0, \ldots, x_{8k}^2) \\
(0, 0, s, 0, \ldots, 0, \ldots, x_{8k}^3) \\
\ldots \\
(0, 0, 0, 0, \ldots, s, \ldots, x_{8k}^N)
\end{align*}
\]

Each factor reduces the volume of the lattice by a factor 4. Hence there can be at most \(4k\) of them. After a finite number of steps there are either no vectors to choose anymore, so that we are finished, or there are none that contain a spinor entry.

In the latter case we continue with classes that consist entirely of vectors. We follow the same procedure. After another \(M\) steps we will run out of new glue vectors. By permutations and nullifications we bring the \(M\) vectors of order 2 into a form where the entries \(N + 1, \ldots, N + M\) of these \(M\) vectors form an \(M \times M\) diagonal matrix \(\text{diag}(v, v, v, \ldots, v)\). The remaining entries of the vectors, beyond entry \(N + M\) can only be 0 and \(v\). Furthermore we can make linear combinations of these order-2 vectors with the \(N\) spinor classes, so that their entries \(N + 1, \ldots, N + M\) are either 0 or \(s\). After this procedure we end up with a matrix of the form

\[
\begin{align*}
(s, 0, 0, 0, \ldots, 0, 0 & | y_{N+1}^1, \ldots, y_{N+M}^1 | x_{N+M+1}^1, \ldots, x_{8k}^1) \\
(0, s, 0, 0, \ldots, 0, 0 & | y_{N+1}^2, \ldots, y_{N+M}^2 | x_{N+M+1}^2, \ldots, x_{8k}^2) \\
(0, 0, s, 0, \ldots, 0, 0 & | y_{N+1}^3, \ldots, y_{N+M}^3 | x_{N+M+1}^3, \ldots, x_{8k}^3)
\end{align*}
\]

28
\[
\begin{array}{cccc}
0, 0, 0, 0, \ldots, 0, s & y_{N+1}^N, \ldots, y_{N+M}^N & x_{N+M+1}^N, \ldots, x_{8k}^N \\
0, 0, 0, 0, 0, 0 & v, 0, 0, 0, 0, 0 & z_{N+M+1}^{N+1}, \ldots, z_{8k}^{N+1} \\
0, 0, 0, 0, 0, 0 & 0, v, 0, 0, 0, 0, 0 & z_{N+M+1}^{N+2}, \ldots, z_{8k}^{N+2} \\
0, 0, 0, 0, 0, 0, 0 & 0, 0, 0, 0, 0, 0, 0 & z_{N+M}^{N+M}, \ldots, z_{8k}^{N+M} \\
\end{array}
\]

Here \( y \) can take the values 0 and \( s \), \( z \) can take the values 0 and \( v \), and \( x \) can take all four values. The volume of the unit cell equals 1 if \( 4^N \times 2^M = 2^{8k} \), hence

\[ 2N + M = 8k \] (15)

This implies that the upper left block is a square \( N \times N \) matrix, and the two other unfixed blocks have \( NM \) entries. Note that for \( N = 0 \) there is no solution, because then \( M = 8k \) and the \( 8k \) vectors \( v^i \) are all of the form \( v^i_j = v^i \delta_{ij} \). This is an integer but not even self-dual lattice. It is also easy to see that for \( N = 1 \) there is only one solution, namely the \( D_{8k} \) root lattice with a spinor conjugacy class added (for \( k = 1 \) this is the \( E_8 \) root lattice).

Without further restrictions, the total number of possibilities for given \( N \) and \( M \) equals

\[ 4^{N^2}2^{NM2M}N = 4^{N(8k-N)} \] (16)

The exponent is maximal for \( N = 4k \), and then it is equal to \( 4^{16k^2} \). Since there are \( 4k \) possible values for \( N \), it follows that the total number of free fermionic theories \( F_{8k} \) satisfies

\[ F_{8k} < 4k \times 4^{16k^2} \] (17)

Numerically, this becomes smaller than \( L_{8k} \) for \( k \geq 15 \).

This count does not include the requirement that the vectors have even norm and integer inner product. We can estimate that effect as follows. For the first \( N \) vectors, about \( 1/8 \) of randomly chosen vectors will have even norm (i.e. their last \( N + M \) components have norm \( \frac{1}{4} \)), and \( 1/4 \) of the dotproducts will be integer. Hence there is an additional suppression factor

\[ 8^{-N} \times 4^{-\frac{1}{2}N(N-1)} \] (18)

This is unfortunately not exact, because it depends on a statistical estimate of distributions of norm and inner products. We can make it a little bit more precise. Numerically, the chance of randomly chosen vectors of \( D_1 \) conjugacy classes to have norms \( \frac{9}{8} \) mod 2 has a slight dependence on \( N \). We find that the values \( n = 0, 4 \) occur with a frequency of almost exactly 12.5%, but for \( n = 1, 5 \) we find 13.28% \( \approx \frac{17}{128} \), for \( n = 3, 7 \) the fraction is 11.72% \( \approx \frac{15}{128} \), for \( n = 2 \) it is 12.55% \( \approx \frac{57}{2048} \) and for \( n = 6 \) we find 12.45% \( \approx \frac{255}{2048} \). It should be possible to derive this analytically. These values hold if the number of vector entries is sufficiently large, which in practice means \( 10 \) or larger. A similar statistical estimate for the inner products of these norm \( \frac{1}{4} \) vectors shows that it is integer in about 24.8% of all cases. Using .1172 instead of \( \frac{1}{8} \) and .248 instead of \( \frac{1}{4} \) gives and additional
suppression factor. For \( N = 4k \) this suppression ranges from \( 10^{-16} \) for \( k = 11 \) to \( .002 \) for \( k = 4 \). We will not include this in the numerical results. This just implies that the actual upper bound is still a few orders of magnitude smaller than what we compute here.

The \( M \) vectors of order 2 have mutually integer inner products, but they have only 50\% chance of having even norm. In addition, their inner product with the first \( N \) vectors is integer or half-integer. Hence we get a reduction factor

\[
2^{-M_2 - NM}
\]  

This has a much smaller impact than (18) and hence the error in the suppression is less relevant here. Putting this all together we get

\[
2^{2N(N+M)}2^{-3N}2^{-N(N-1)}2^{-M_2 - NM} = 2^{8k(N-1) - N^2}
\]  

This can be written as

\[
2^{8k(2k-1)-(N-4k)^2}
\]

This reaches its maximum for \( N = 4k \), and at the maximum it equals \( 2^{8k(2k-1)} \). Therefore a highly plausible (though not mathematically rigorous) upper bound is

\[
F_{8k} < 4k \cdot 2^{8k(2k-1)}
\]

This is smaller than \( 2L_{8k} \), the lower bound on the number of ESDL’s, for \( k \geq 8 \). For \( k = 11 \), the value of interest of [1], it is about \( 10^{558} \). Hence the free fermionic theories yield a fraction of less that \( 10^{-370} \) of all ESDL’s of dimension 88.

Although all free fermionic partition functions that solve the conditions considered so far are ESDL’s, there will be many degeneracies among them. In particular, we have at our disposal \((8k - N - M)!\) permutations of the last \( 8k - N - M \) components, \( 2^N \) inner automorphisms of the last \( N \) \( D_1 \) factors (interchanging \( s \) and \( c \)), \( N! \) permutations of the \( N \) spinorial glue vectors and \( M! \) permutations of the \( M \) vectorial ones. Furthermore there is an unestimable number of distinct ways of choosing different basis vectors for a given lattice other then just permutations of basis vectors. On top of that there are other transformations, such as triality rotations within \( D_4 \) sublattices (if any exist). But these reductions are all correlated, so it is difficult to make the estimate more precise. Treating the \( D_1 \) factor permutations, inner automorphisms and spinor class permutations as if they were uncorrelated gives and additional suppression \(( (4k)! )^2 2^{4k} \) for \( N = 4k \). This is about \( 10^{122} \) for \( k = 11 \).

We also have some empirical evidence for the amount of overcounting. For \( k = 1 \) formula (22) overcounts the actual number of distinct lattices by a factor 1024; for \( k = 2 \) by a factor \( 10^{15} \) and for \( k = 3 \) by a factor \( 10^{36} \). For \( k = 4 \) the overcount factor is at least \( 10^{16} \) (because \( F_k \) is that much larger than the maximum number of ESDL’s). One is inclined to assume that, because of its factorial nature, the exponent of the overcount factor increases at least linearly with \( k \). This would suggest an overcount reduction by about a factor \( 10^{200} \) for \( k = 11 \).

We conclude that there are ESDL’s which cannot we written in terms of free fermions. In fact, the fraction of ESDL’s that \textit{can} be written in terms of free fermions is infinitesimal,
and hence the special cases $k = 1$ and $k = 2$ are misleading. For dimension 88, this fraction is smaller than $10^{-370}$, and probably far less than that. For dimension 24, it is not known to me if all Niemeier lattices have a free fermionic description.

References


