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**Research** Vici project

# Longe range stochastic dynamics

**In the Vici project 'Longe range stochastic dynamics' Ronald Meester and his team worked on sandpile models and the Bak–Sneppen model. In this article Meester describes these two systems. However, their research was certainly not restricted to these two subjects. They also investigated dependent percolation models with long range interactions, models for forest fires, invasion percolation, self-destructive percolation, divide and colour models, and more.**

If I remember correctly my own Vici story began with Jan van Mill stepping into my office with the Vici call in his hands, simply saying: "Ronald, I think this is something for you."

And well, yes, it was something for me. Applying for so much money of course required a big and ambitious program. Although I had been (and still am) interested in many other things as well, it was rather obvious that such a big and ambitious program should centre around some aspect of spatial random processes. Much of my research until that point had been devoted to percolation theory in a classical, continuum, and fractal setting. Percolation theory is also of obvious interest to many theoretical physicists and as such it was not unusual for me to participate in conferences where mathematicians and physicists actually talk to each other. As such, I had come across the notion of so called 'Self-Organised Criticality' (SOC). The phrase was coined by physicists like Per Bak, and referred in some vague sense to models that behaved as classical systems (like percolation) at the critical point, but without any tuning of parameters.

Let me elaborate on this point. Suppose you take classical percolation on the $d$-dimensional lattice $\mathbb{Z}^d$, which means that every nearest neighbour bond is retained with probability $p$, and deleted with probability $1-p$. For small values of $p$, all connected components of the resulting graph will be finite, while for large $p$, infinite components will arise (in fact, at most one infinite component will be formed but this is not important for my purposes here). In the former case, spatial correlations decay exponentially fast, while in the latter case, correlations do not decay to zero at all. It is precisely at the critical value for $p$, denoted by $p_c$, which separates the two regimes, where the correlations do decay, but according to a power law, rather than exponentially fast. Hence, power law behaviour is associated with criticality, and in this example, $p$ must be tuned to $p_c$ precisely in order to observe this power law behaviour. (How much of this can be demonstrated rigorously is another matter, I will not go into that direction here.)

In many physical, ecological and biological systems, power law behaviour is observed, but apparently without any tuning parameter. The systems 'organise' themselves into this apparent critical behaviour, hence the name Self-Organised Criticality. Examples are models for earthquakes, avalanches, evolutionary systems, forest fires and many more. For instance, when you plot the energy released in an earthquake versus the frequency of occurring, you will observe a power law, the basis for Richter's scale.

Much of this is rather attractive from a physics point of view, and there are many, many publications in the physics literature describing systems that are supposedly self-organised critical in the above, rather vague sense. For mathematicians, there are other things at stake. Even the precise mathematical formulation of some of these concepts is rather unclear, let alone proving them. This is not to say that the models that were introduced by the physicists would not be interesting from a mathematical point of view. In fact, they are *very* interesting, exactly because of

the fact that standard and classical methods seem to break down completely. In my Vici project, I took up the challenge to study some of these models. I already had some experience with some of them, and the Vici project seemed a very good opportunity to carry this further. The project was certainly not restricted to sandpiles and the Bak–Sneppen model: we also investigated dependent percolation models with long range interactions, models for forest fires, invasion percolation, self-destructive percolation, divide and colour models, and more. The general nature of the project was reflected in the title: 'Long range stochastic dynamics'. This encompasses a lot, and I thought it wise not to restrict myself too early.

In this article, I will of course not be able to discuss all the work done in the project. I will therefore restrict myself to two specific systems, namely sandpile models and the Bak–Sneppen model, which are easy to explain and hopefully interesting for a broader audience.

*The Abelian sandpile model*

The Abelian sandpile model (ASM) is a finite state discrete time Markov process, supposed to model avalanches. It is defined on a finite connected subset $\Lambda$ of $\mathbb{Z}^d$. It starts with a so called *configuration*, that is, every site in $\Lambda$ contains a non-negative integer-valued number of particles, or *sand grains*. This number is typically called the *height* of the site. A site is *stable* if its height is at most $2d-1$. When all sites in $\Lambda$ are stable we call the configuration stable. The state space of the Markov chain is the collection of stable configurations. The dynamics is as follows. We start with a stable configuration. Every discrete time step, an addition of one sand grain is made to a randomly chosen site. If this site becomes *unstable*, i.e., it has at least $2d$ grains, it *topples*, that is, it simply gives one grain to each neighbour. Hence the height of the toppled site decreases by $2d$, and the height of each of its neighbours increases by $1$. When a site at the boundary of $\Lambda$ topples, then the number of neighbours may be less than $2d$. This simply means that the corresponding grains leave the system. This is not the end of a time step, because the resulting configuration may not be stable. Indeed, a toppling may cause a neighbouring site to become unstable. Hence, we continue with toppling unstable sites until every site is stable again. The total of all necessary topplings is called an *avalanche*. After the avalanche we have reached the new configuration, and this finishes one time step of the Markov process.

It is not hard to see that a new stable configuration is reached after finitely many topplings. It is also not very difficult to show that the order in which topplings are executed has no effect on the final stable configuration. Although not too difficult, this simple fact already caused some discussion between the mathematical and the physics community. The physicists insisted that it was enough to observe that if you perform two topplings, one at site $x$ and one at site $y$, then the order in which you do this does not matter, hence the name *Abelian* sandpile model. However, this obviously does not settle the issue, because you have to prove that when you topple $x$ first, say, then the collection of sites that topples until stabilization, is the same as when you topple $y$ first. I am not sure I have been able to convince my physics friends on this issue.

This sandpile model is said to exhibit self-organised critical behaviour, for the following reason. As the model evolves in time, it reaches a stationary distribution that is characterized, in the large-volume limit, by long-range height correlations and power law statistics for avalanche sizes. What I mean by this is that when $\Lambda$ is large, it seems that the probability to observe an avalanche which involves at least $k$ sites, decays with a power law in $k$, and similarly for the number of topplings of a randomly chosen avalanche (this must be made precise of course). As indicated above, this reminds one of critical behaviour in statistical mechanical models, like percolation. In percolation, one sees power law behaviour of cluster sizes only at the critical point, so one must choose parameters very, very carefully in order to observe this. For the sandpile however, it seems that this is achieved in a natural way, without apparent tuning of any parameters. Hence the model organises itself into behaviour which is associated with criticality, hence the name SOC.

One may wonder whether or not this mathematical model is suitable for describing anything which looks like a real avalanche. Indeed, isn't it more natural to let height *differences* decide whether or not an avalanche takes place? I would say yes, but the problem is that a sandpile model based on height differences is a lot more difficult to study since it is not Abelian. As a result, very little has been done in this direction.

Preceding my Vici project, I had already worked on the ASM with Dmitri Znamenski, a PhD student, and Frank Redig. Among other things, we had written a paper in which we gave rigorous proofs of various claims by the physicist.

*The Bak–Sneppen model*

The Bak–Sneppen model was originally introduced as a very simple model for evolution by Per Bak and Kim Sneppen [2]. I had already worked on it with Dmitri Znamenski in the years preceding the Vici grant. The model is as simple as one can imagine, but at the same time very difficult to study in a rigorous mathematical way. It is defined as follows. Let $N$ vertices ('species') be arranged regularly on a circle, and denote this structure by $\Lambda_N$. Assign a *fitness* to each vertex, that is, independent random variables, uniformly distributed on $(0,1)$. At each discrete time step the system is updated by locating the vertex with the lowest fitness and replacing this fitness *and those of its two neighbours* by independent and uniform $(0,1)$ random variables. The Bak–Sneppen model is again a discrete time Markov process. In some vague sense, the dynamics should remind us of evolutionary processes in which species with the lowest fitness disappear. Other species which somehow depend on the one with lowest fitness, run into trouble then, hence the rule that also neighbours obtain new fitnesses.

This neighbour interaction makes this model very interesting but also very difficult. One may wonder why this model is interpreted as a model for SOC. The reason for this is that one can define *avalanches* in a very natural way, as follows. An *avalanche at threshold* $0<b<1$ (also called a *$b$-avalanche*) is said to occur between times $s$ and $s+t$ if at time $s$ all the fitnesses are equal to or greater than $b$, and time $s+t$ is the next time after $s$ at which this occurs. Note that if we have a minimum fitness value of $b$, then we can choose any value up to (and including) $b$ to be our avalanche threshold. Furthermore, it is the threshold and not the exact initial values of the model that determines the behaviour of an avalanche. Once we have used the initial fitnesses to find out the minimal fitness and its location, all other information can be discarded for the purposes of analysing individual avalanches.

The notion of an avalanche helps to explain the self-organised critical nature of the model. Indeed, when $b$ is small, avalanches appear to be (exponentially) short, both in the time of duration as in the number of sites involved. When $b$ is large, avalanche durations are not uniformly bounded in the system size $N$. The critical threshold $b_c$ is the threshold values separating these two

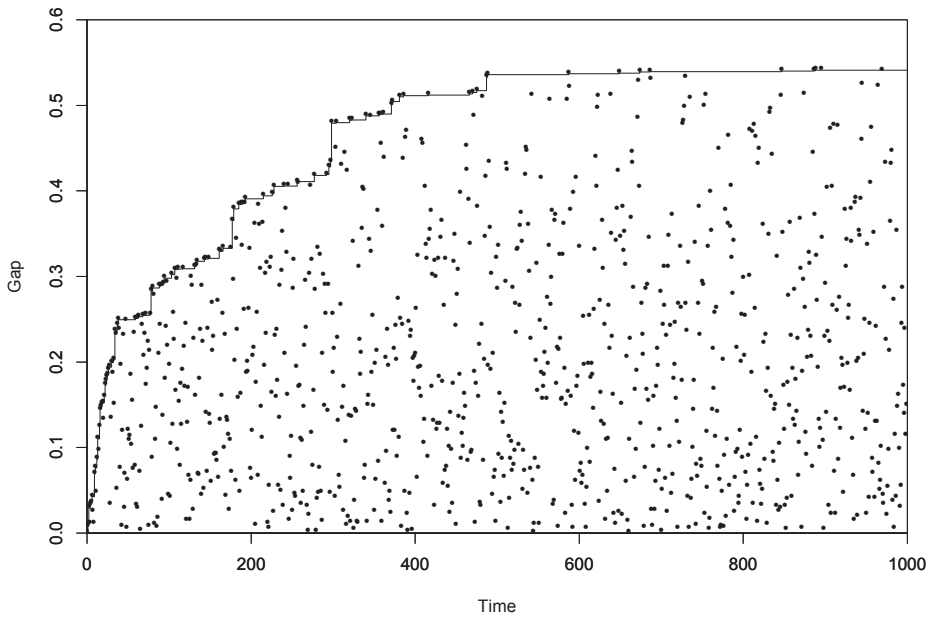


**Figure 1** A snapshot of the Bak–Sneppen model in stationarity.

regimes. Running the model on a computer (this is really simple; I do not think that there are many models which are so simple to describe, and at the same time so deep from a mathematical point of view) there seems to be a threshold $b_c$, close to $\frac{2}{3}$, such that after a while, the dynamics appear to consist of consecutive avalanches at $b_c$, and in addition, these avalanches seem to exhibit power law behaviour in the sense that both duration and range can be described by power laws [2]. The threshold $b_c$ is not set beforehand; the model seems to organise *itself* into this state. See Figure 1 for a typical snapshot of the Bak–Sneppen model in stationarity, with $N = 300$. On the horizontal axis we have the $300$ vertices, with the dots representing the fitnesses of the vertices.

**Some philosophical issues**

I will discuss some progress in understanding these models later. Before that, it is perhaps interesting to elaborate on the claim that these processes indeed organise themselves into a critical state. How self-organised are these systems, really? And how realistic is the implicit or explicit claim that power law behaviour in spatial or temporal quantities can or should be interpreted as being the result of self-organised criticality? Power law behaviour is abundant, and probably for a variety of reasons. In this connection it is useful to read Per Bak's book *How nature works* on the subject. The title of the book of course already gives away what seems to be at stake: self-organised criticality as the leading principle in many physical, ecological and biological processes. Ignoring the obvious self-satisfaction displayed by Bak, reading his book it is hard to avoid the conclusion that they had great difficulties to formulate a model which in fact showed power law behaviour as desired. Especially the description of the Bak–Sneppen model did not come for free. Isn't this careful formulation in itself slightly at odds with the claim that everything goes 'by itself'? One has to define a model very cautiously in order to see critical behaviour. The tuning of parameters has been replaced by careful selection of the model, perhaps. In addition, claiming that SOC is the way nature works is claiming that a power law behaviour can be explained by SOC which, in my opinion, is not well founded. When you have a hammer, everything looks like a nail.

There have been philosophical attempts to explain the fact that sandpile models behave like a critical classical system. These attempts involve the definition of parametrized sandpiles in infinite volume, and since we will need this later anyway, this is the right moment to introduce them.

Consider the Abelian sandpile described above, but this time in infinite volume, let us say on $\mathbb{Z}$. In this situation, we cannot choose a site uniformly any more. Hence, this system has no additions, only topplings. Starting from an initial configuration of heights, one step of the ensuing Markov process is to simply topple all unstable sites once. In this context, the question is whether or not the configuration converges (in the usual product topology) to a limiting configuration. Clearly this depends on the initial configuration, and in order to formulate this as a classical parametric model, we let the heights in the initial configuration be distributed as independent Poisson-$\rho$ random variables. In this set-up, we expect a phase transition in $\rho$: when $\rho$ is small, not many sand grains are present, and probably all motion will stop locally after a transient period, that is, all sites topple only finitely many times. If $\rho$ is large, then there is no limiting configuration since there will not be enough space to accommodate all particles in a stable way, that is, all sites will topple infinitely many times. The critical point of this system separating the two regimes is denoted $\rho_c$; the similarity between this critical density and the critical probability $p_c$ in percolation is clear.

With this infinite volume model in place, we can look at the connection between the original Abelian sandpile and this infinite-volume system. In a widely cited series of papers [6–7, 23, 25–26], Dickman, Muñoz, Vespignani and Zapperi developed the following heuristic argument. If the density of particles in the finite volume system is larger than $\rho_c$, one 'typically' should have topplings, and as a result particles might leave the system. If this density of particles is smaller than $\rho_c$, then 'typically' one should only have addition of particles. Hence the density of particles should always change in the direction of $\rho_c$ and therefore the finite volume sandpile resides, when the volume is large, around the density $\rho_c$, and as such, its behaviour should be close to the behaviour of the infinite-volume system at criticality. According to this reasoning, for $n$ large, the finite volume ASM should behave very similarly to the infinite volume model *at its critical point*, so it would be reasonable to expect critical behaviour in the finite volume sandpile.

In the above situation, that is, the Abelian sandpile on the line, one can actually prove that the above intuition is correct [19]. However, when we change the graph, this need not be true any more. As an example (taken from [8]), consider the *bracelet graph*. This graph is similar to the line, except for the fact that there are two edges between neighbouring vertices (this can be done both in finite and infinite volume). The ASM on the bracelet graph closely resembles the one-dimensional ASM described above. In fact, we can repeat the entire description, except for two differences: first, sites are called stable if their height is $0,1,2$ or $3$, and second, in a toppling occurring at site $x$, the height of $x$ decreases by $4$ and the height of both its neighbours increases by $2$.

In this model, the parity of a given site is invariant under topplings. Indeed, the height changes that occur in a toppling are all even. This simple observation allows one to calculate explicitly the various critical densities. For a configuration in the bracelet ASM, we write

$$\eta = 2\eta_e + \eta_o,$$

where $\eta_e(x) = \lfloor \eta(x)/2 \rfloor$ is the number of pairs of particles at $x$, and $\eta_o(x) = \eta(x) \mod 2$ is the indicator that $\eta(x)$ is odd. Let us look at the infinite volume model, where we only have topplings. Topplings have no influence on $\eta_o$, therefore the expected value of $\eta_o(x)$ is constant in time, and for every $x$ equal to the probability that a Poisson-$\rho$ random variable is odd. We denote this probability as $P_\rho(odd)$. Topplings influence $\eta_e$ as follows: only sites $x$ where $\eta_e(x) > 1$ are unstable, and in a toppling, $\eta_e(x)$ decreases by 2 and the number of pairs of the neighbours increases by 1. In other words, $\eta_e$ in the bracelet ASM evolves in precisely the same way as $\eta$ in the one-dimensional ASM. We can conclude immediately that there is a phase transition when the 'pair density' is 1. A simple computation now yields that $\rho_c$ is the solution of $\rho = P_\rho(odd) + 2$. However, a rather elementary analysis of the finite-volume version leads to the conclusion that the average density of sand grains converges to $\frac{5}{2}$, as the system size grows to infinity, so the finite volume will not reside around the critical value from the infinite volume model, and the picture of Dickman et al. breaks down.

The situation is, therefore, not so clear. One of the ways to get rid of local toppling invariants, is to let the topplings themselves being random in the sense that each particle randomly chooses a neighbour. In this model, the so called *Manna model*, a lot of structure is lost, and the picture sketched by Dickman is still a possibility; we do not know.

## Progress in the Bak–Sneppen model

*Maximal avalanches*

Prior to the Vici project, we had concentrated on the expected duration of an avalanche at a fixed and non-random threshold $b$ [22].

Results include a number of useful monotonicity results, as well as an explicit differential equation relating the expected duration of avalanches to their expected range. During the project we studied the avalanches at random thresholds which appear in, or are strongly related to, the thresholds in the so called *maximal avalanche decomposition*. Here the first avalanche threshold is defined to be the minimum fitness value from the initial fitness values. After this and every subsequent avalanche, another avalanche begins with the threshold chosen to be the new minimal value of the model; this is the maximal threshold choice. It is clear that this will lead to the Bak–Sneppen model being seen as a series of avalanches at strictly increasing thresholds. The *gap function* at time $s$, $G(s)$, is defined to be the avalanche threshold at time $s$. The gap function is a stepwise increasing random function which jumps to a new value each time an avalanche finishes. Note that for all finite systems the gap function tends to 1 almost surely. Figure 2 shows a realisation of the gap function represented by the line, with the dots being the minimum fitness values at each time step. The initial fitnesses were independent and uniform $(0, 1)$ distributed.

One reason for looking at the maximal avalanche decomposition is to gain insight into how the Bak–Sneppen model tends towards criticality.

On $\Lambda_N$ the threshold is the only variable needed in order to determine the distribution of an avalanche's duration. By this we mean that the durations of two avalanches on a transitive graph are identically distributed if their thresholds are the same. Consider the Bak–Sneppen model on $\Lambda_N$. Concentrating first on the *initial* avalanche in the maximal decomposition, we see that the initial threshold is the minimum of $N$ independent uniform $(0, 1)$ random variables. To be more explicit, we have an avalanche with random threshold $B$ whose density $h_N(b)$ is given by

$$h_N(b) = N(1-b)^{N-1}, \quad 0 \le b \le 1.$$

Letting $D_N$ denote the duration of the initial avalanche on $\Lambda_N$, we have the following theorem.

**Theorem 1** [12]. *The expected duration of the first avalanche on $\Lambda_N$ is infinite, i.e. $\mathbb{E}(D_N) = \infty$.*

One consequence of this result is that any subsequent avalanche also has infinite expected duration, as its threshold is stochastically larger. Hence the gap function consists of a sequence of steps, each of which has infinite expected length.

The usual way to analyse the Bak–Sneppen model has been to run computer simulations. Compared to these simulations, our result seems somewhat surprising, since divergent behaviour is not typically noticeable under numerical simulations of the model, especially when $N$ is large. This is because the long avalanches that are behind this result occur when the (random) threshold $B$ is high, which is exponentially unlikely in $N$. If one were to run computer simulations of the initial avalanche in order to estimate its expected duration, it would still be possible to detect this, but only from the dramatic variability of these estimations (even when a very large number of simulations are used). Theorem 1 is, therefore, an example of the value of an-

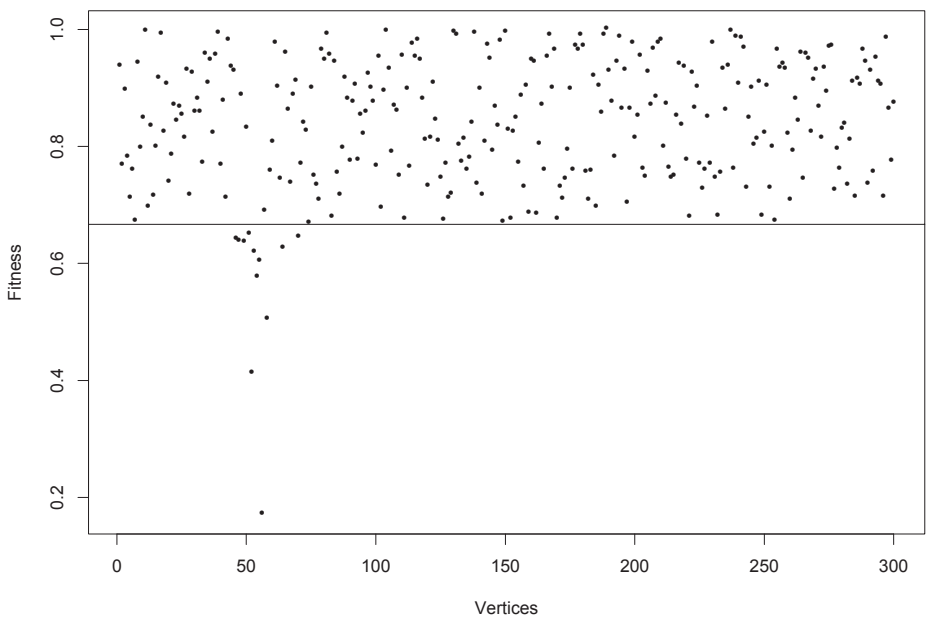


**Figure 2** A realisation of the gap function when $N$=100.

alytic methods, as only very careful interpretation of computer simulations would lead one to suspect this result.

We decided to perturb the avalanche threshold by making it stochastically smaller and see whether this would lead to convergence. It turns out that $\mathbb{E}(D_N)$ is 'barely infinite' in the sense that making the threshold a tiny bit stochastically smaller yields finite expected durations. To be precise, we denote by $D_N^n$ the duration of an avalanche at a threshold which is set by the minimum of $n$ uniform $(0,1)$ random variables on $\Lambda_N$. In this notation, $D_N^N = D_N$, with Theorem 1 now stating that $\mathbb{E}(D_N^N) = \infty$. We proved the following result.

**Theorem 2** [12]. *An avalanche from a threshold chosen as the minimum of $n > N$ independent uniform $(0,1)$ random variables has finite expectation, i.e. $\mathbb{E}(D_N^n) < \infty$ for all $n > N$.*

So just adding one uniform random variable when setting the threshold is enough to get a finite expected duration, no matter the size $N$ of the system.

However, it is possible to show that under certain conditions all *further* avalanches have infinite expected duration. Recall that on $\Lambda_N$, setting the threshold as the minimum of $N$ independent uniform $(0,1)$ random variables, gives infinite expected duration. If all the fitnesses (except the minimum) are independent and uniformly distributed above the threshold at the start of the avalanche, then at the end of the avalanche all the vertices will again be independent and uniformly distributed above the threshold. So even if you fix $b$ and choose your fitnesses to be uniform above it, it follows from Theorem 1 that the next avalanche will have infinite expected duration.

A more general, but weaker form of this result applies when we drop the condition that the fitnesses had to be nicely distributed at the start of the avalanche. All the vertices updated by the avalanche will be independent and uniformly distributed above the threshold at the end of the avalanche. So once we have had a spanning avalanche (one that updates every vertex in the system during its duration) all subsequent avalanches (from maximal thresholds) will have infinite expected duration, no matter what initial fitness values are taken.

*The critical value*

We already noticed that a sandpile model in infinite volume can be defined, without additions and only topplings. Obviously, there is also a problem if we want to define the Bak–Sneppen model on an infinite graph, since every time step requires the choice of the vertex with minimal fitness. Nevertheless, for certain initial configurations we can define the model on an infinite graph, at least for a certain number of time steps. For instance, choose a threshold $b \in (0,1)$, and consider a configuration of fitnesses with exactly one vertex, $x$ say, having fitness below $b$, and all other fitnesses above $b$. In this situation we can start the system and run it at least until the first time that all fitnesses are above $b$ again (if this happens). In other words, the notion of a $b$-avalanche makes perfect sense in infinite volume. As such it is very natural to define the critical threshold $b_c$ as the infimum over all thresholds $b$ for which the probability of an infinite $b$-avalanche is positive. In fact, we can do this on any infinite graph $G$.

The question then is what we can say about the critical value $b_c$. Computing it seems beyond reach, but interesting lower and upper bounds can be computed. A non-trivial lower bound is obtained by comparing the Bak–Sneppen model to a branching process. This is not very demanding and a very common technique. In this case this quickly leads to the following result.

**Proposition 3** [13]. *On any locally finite transitive graph $G$ with common vertex degree $\Delta$, we have*

$$b_c(G) \geq \frac{1}{\Delta + 1}.$$

A non-trivial upper bound, however, is another matter. One of the most satisfactory things in mathematics is to relate two models to each other which did not have an obvious connection form the outset. In this direction, we were able to relate the Bak–Sneppen model on a graph $G$ top independent site percolation on the same graph. In independent site percolation, we independently colour the vertices black or white with probability $p$ and $1-p$ respectively, and $p_c^{site}(G)$ is the infimum over al $p$ for which the probability that an infinite black component arises is positive. It turns out that the following is true.

**Theorem 4** [13]. *On any locally finite transitive graph $G$, we have*

$$b_c(G) \leq p_c^{site}(G).$$

This result implies that on many locally finite transitive graphs, $b_c$ is non-trivial. For the Bak–Sneppen avalanche on $\mathbb{Z}$, Theorem 4 gives a trivial upper bound, but in this case we know from [21] that $b_c(\mathbb{Z}) \leq 1 - \exp(-68)$.

The following heuristics make Theorem 4 plausible. If a vertex's fitness is not minimal, then its conditional distribution based on this information is stochastically larger than its original uniform $(0,1)$ distribution. So if a vertex is updated by a neighbour having minimal fitness, this makes its fitness stochastically smaller, making the vertex more likely to be active and therefore, intuitively at least, the avalanche is more likely to continue. This means that on average the interference from the non-extremal vertices of the Bak–Sneppen model on the extremal vertices should be beneficial to the spread of the avalanche.

A proof is another matter. We are interested in comparing the open cluster at the origin of site percolation to a Bak–Sneppen avalanche. Typically, site percolation is considered to be static, but it is also possible to build up the open cluster at the origin dynamically. This can be done as follows. To begin with, consider the origin to be open and look at its neighbours. Decide which of these vertices are open. Then look at the new neighbours of the open cluster and iterate.

The growth of both a Bak–Sneppen avalanche and the open cluster at the origin is driven by the *extremal* vertices. These are those vertices that are contained within the avalanche and have neighbours outside the avalanche. It is only through one of the extremal vertices having the minimal fitness that the range of the avalanche can increase. For site percolation, the extremal vertices are those having a neighbour in the open cluster at the origin, but that are themselves unknown as to be open or closed. These are exactly the vertices at the edge of the cluster that will increase the size of the cluster by being open. Since it is the extremal vertices that drive the spread of both processes, the task is to relate the two sets of extremal vertices to each other.

The major difficulty to overcome is that in the Bak–Sneppen model an extremal vertex may be updated by neighbouring activity before having minimal fitness itself, whereas in site percolation a vertex is just either open or closed. This means that it is not useful to couple the two models in the naive way by realising the fitness and determining if the vertex is open and closed immediately with the same random variable, and suggests that the coupling needed is rather subtle and requires great care.

Since the critical value of site percolation on $T_\Delta$, the regular tree with common degree $\Delta$, equals $1/(\Delta-1)$, the following corollary holds.

**Corollary 5.** *The critical value of the Bak–Sneppen model on a regular tree with common degree $\Delta$, $T_\Delta$, satisfies*

$$\frac{1}{\Delta+1} \le b_c(T_\Delta) \le \frac{1}{\Delta-1}.$$

*A power law*

One may wonder whether any progress has been made on power law behaviour in the Bak–Sneppen model. During the course of the Vici project we have not addressed this issue, but some years later, we realised that power law behaviour of avalanches can be rigorously demonstrated in a modified version of the Bak–Sneppen model [24]. This modification is defined as follows. At each discrete time step, the particle with the lowest fitness is chosen, together with one other particle, chosen uniformly at random among all remaining particles. (The restriction to update only one extra particle is rather arbitrary. The proof — suitably reformulated — goes through for multiple choices as well.) The fitnesses of both chosen particles are replaced by new fitnesses, according to independent uniform random variables on $(0,1)$.

We define the *duration* of an *avalanche at level* $t \in (0,1)$, also referred to as a *$t$-avalanche*, as follows. Consider an initial configuration of fitnesses in which one particle has fitness $0$ and all other particles have fitnesses strictly larger than $t$. Let $A_t^N$ denote the first time at which all fitnesses are strictly larger than $t$. The random variable $A_t^N$ is the duration of a typical avalanche at level $t$. The number of particles updated in such an avalanche is called the *size* of an avalanche, and the corresponding random variable is denoted $S_t^N$.

**Theorem 6** [24]. *In the modified Bak–Sneppen model, the following hold.*

(a) *For $t < \frac{1}{2}$, we have for all $N$,*

$$P(A_t^N \ge k) \le e^{-c_1(t)k},$$

*for some positive constant $c_1(t)$ independent of $N$.*

(b) *For $t > \frac{1}{2}$ and all $k$, we have*

$$\lim_{N\to\infty} P(A_t^N \ge k) \ge c_2(t),$$

*for some positive constant $c_2(t)$.*

(c) *For $t = \frac{1}{2}$, we have*

$$\lim_{k\to\infty}\lim_{N\to\infty} \sqrt{k} P(A_t^N \ge k) = 2/\sqrt{\pi}.$$

*The statements in* (a)–(c) *remain valid when we replace $A_t^N$ with $S_t^N$.*

A rigorous power law at last; the proof of this result proceeds through coupling with a branching process, in combination with the known power law behaviour of a branching process at criticality. At least this result shows a proof of principle. The critical threshold is $\frac{1}{2}$ in this case, and one can understand why power law behaviour is observed now. Indeed, if the minimal threshold is below $\frac{1}{2}$, then the avalanche at that threshold is exponentially short. Only when the minimal threshold is close to $\frac{1}{2}$, one notices the power law statistics, and the system seems to organise itself, roughly, around consecutive avalanches at level $\frac{1}{2}$.

## Progress on sandpile models

*Stabilization in infinite volume*

I already mentioned the stabilization problem in infinite volume. Above, we simply toppled all unstable sites once, but there are many other ways to organise the topplings in infinite volume. We mention some of them.

- *Markov toppling processes.* These are examples of random stationary toppling procedures and are defined as follows. Each site $x \in \mathbb{Z}^d$ has a Poisson clock (different clocks are independent) with rate one. When the clock at site $x$ rings at time $t$ and in the configuration $\eta_{t-}$, $x$ is unstable, then $x$ is toppled.
- *Toppling in nested volumes.* Choose a sequence $V_1 \subset V_2 \subset \cdots$ of subsets of $\mathbb{Z}^d$ such that $\cup_n V_n = \mathbb{Z}^d$, but all $V_n$ contain finitely many sites. We start toppling all the unstable sites in $V_0$ until the configuration in $V_0$ has no unstable sites left, then we do the same with $V_1$, et cetera.
- *Topplings in waves.* This procedure is only used for initial configurations having a single unstable site, say at $x \in \mathbb{Z}^d$. In the first step, we topple $x$ once and subsequently all *other* sites that become unstable. All these topplings form the first wave. If after these topplings, $x$ is still unstable, then we perform the second wave, et cetera. In each wave, no site topples more than once.

Now the question about an infinite-volume analogue of the Abelian property arises: how do we know that the order in which we apply the topplings has no effect on the final stable configuration, or, for that matter, on the very question whether or not stabilization occurs? We formulated the following answer to this question. We call a toppling procedure $T$ *finite* for initial configuration $\eta$ if starting from $\eta$, every site topples at most finitely many times. We call it *stabilizing* for initial configuration $\eta$ if it is finite for $\eta$ and eventually leads to a stable configuration. We call a configuration $\eta$ *stabilizable* if there is at least one stabilizing toppling procedure for $\eta$.

**Theorem 7** [10].

1. *If $T$ and $T'$ are two stabilizing toppling procedures for $\eta$, then for all $x \in \mathbb{Z}^d$, $x$ topples the same number of times under $T$ and $T'$. In particular, this means that for stabilizable $\eta$, the limit configuration $\eta_\infty$ is well-defined.*
2. *For* stabilizable *$\eta$, there does not exist a non-finite toppling procedure.*
3. *If $T$ is stabilizing for $\eta$, then there is at least one site $x$ that does not topple.*

Strictly speaking, the above is only true under a very weak technical condition, which is empty in case the topplings evolve in discrete time, and satisfied for Markov topplings. The upshot of this result is that we can talk about stabilizable initial configurations since it essentially does not matter which toppling procedure we use.

Another interesting question is the behaviour of the infinite volume sandpile at its critical density $\rho_c$. Does the system stabilize at $\rho_c$ or not? The behaviour at criticality is in general one of the most difficult things to understand and to prove, but for the infinite-volume ASM in dimension $1$, it turns out to be within reach. We say that a probability measure $\mu$ on initial configurations is stabilizable if $\mu$-almost all configurations are stabilizable. The following result is not the most general result we obtained, but it has a short and elegant proof which I sketch below.

**Theorem 8** [10]. *Let $\mu$ be a translation invariant probability measure such that $E_\mu(\eta(0)) = 1$ and such that $\frac{1}{\sqrt{n}}\sum_{x=-n}^{n}(\eta(x)-1)$ converges in distribution, as $n\to\infty$, to a non-degenerate normal random variable. Then $\mu$ is not stabilizable.*

*Proof.* The *toppling matrix* $\Delta$ is defined as the matrix indexed by sites $x, y \in \mathbb{Z}$, with entries

$$\Delta_{x,y} = 2\mathbf{1}_{x=y} - \mathbf{1}_{|x-y|=1}.$$

Suppose now that $\mu$ is stabilizable. Then

the only final stable configuration can be the configuration which is constant and equal to $1$. Denote by $N(x)$ the number of topplings needed at $x$ to stabilize $\eta$. From the way topplings are defined, we then must have

$$\eta - \Delta N(x) = \overline{1}. \tag{1}$$

where $\overline{1}$ denotes the configuration with height $1$ everywhere. By stationarity of the toppling mechanism, the joint distribution of $N(x)$ is stationary under translations. From (1) we obtain

$$\begin{aligned}\frac{1}{\sqrt{n}} &\sum_{x=-n}^{n} (\eta(x) - 1) \\ &= \frac{1}{\sqrt{n}} \left( \sum_{x=-n}^{n} \Delta N(x) \right) \\ &= \frac{1}{\sqrt{n}} \big( N(-n-1) - N(n) \\ &\qquad + N(n+1) - N(-n) \big).\end{aligned}$$

The right hand side converges to $0$ in probability as $n \to \infty$. This leads to a contradiction since, by assumption, the left hand side converges in distribution to a non-degenerate normal random variable. □

*Zhang's sandpile model*

A less well known sandpile model was introduced by Zhang [27]. Instead of discrete sand grains, he uses continuous height variables, as follows.

Consider a finite connected subset $\Lambda \subset \mathbb{Z}^d$. Initially, every lattice site $x \in \Lambda$ is given an *energy* $0 \le E_x < E_c$, where $E_c$ is the so called *critical threshold*, and often chosen to be equal to $1$. Then, at each discrete time step, one adds a random amount of energy, uniformly distributed on some interval $[a, b] \subset [0, E_c]$, at a randomly chosen lattice site. If the resulting energy at this site is still below the critical value then we have arrived at the new configuration. If not, an *avalanche* is started, in which all unstable sites (that is, sites with energy at least $E_c$) 'topple' in parallel, i.e., give a fraction $1/(2d)$ of their energy to each neighbour in $\Lambda$. Hence, after toppling of site $x$, $E_x = 0$. For boundary sites, energy leaves the system in complete analogy to the Abelian sandpile model. As in the ASM , the stabilization of an unstable configuration is performed instantaneously, i.e., one only looks at the final stable result of the random addition, and the transition form one stable configuration to the other comprises one step the Markov process.

For this process, it is not even obvious that a (unique) stationary distribution exists, and one of our contributions is to prove precisely this in the special case of the Zhang model on the line, with $N$ sites.

**Theorem 9** [9, 11]. *For every $0 \le a < b \le 1$, and $N \ge 2$, Zhang's sandpile model $(N, [a, b])$ has a unique stationary distribution which we denote by $\mu^{a,b,N}$. Moreover, for every initial distribution on $[0, 1)^N$, the distribution of the process at time $t$ converges exponentially fast in total variation to $\mu^{a,b,N}$, as $t \to \infty$.*

This result was proved in a number of papers, with special proofs for various special cases first. The proof technique is to find a coupling between the evolution of the process starting from different initial configurations in such a way that the processes eventually coalesce. Conceptually this proof method is classical, but to carry it out is not always so easy. It quickly becomes rather technical.

It is not so easy to obtain specific information about the stationary distribution. Here I state one result in this direction. Obviously the one-dimensional marginals have an atom at $0$. Apart from this, the marginals turn out to be absolutely continuous with respect to Lebesgue measure on $(0, 1)$. This is of some independent interest, since in [4], a version of Zhang's sandpile model is discussed in which additions are deterministic and this model behaves radically different.

**Theorem 10** [11]. *Let $\nu$ be the stationary distribution for Zhang's model on $N$ sites. Every one-site marginal of $\nu$ is on $(0, 1)$ absolutely continuous with respect to Lebesgue measure.*

There are interesting connections between the ASM and Zhang's sandpile model, and I discuss one of them. In his original paper, Zhang observes from numerical simulation, that for large lattices, the energy variables in the stationary state tend to concentrate around discrete values of energy. He calls this the emergence of energy 'quasi-units'. Furthermore, he argues that in the thermodynamic limit, that is, when the volume tends to infinity, the stationary dynamics should behave as in the discrete ASM, with the grains of sand replaced by the discrete values of the energy.

One of the things we proved is that these quasi-units do indeed appear when $a \ge \frac{1}{2}$, in the following sense: as $N \to \infty$, all one-site marginals of the stationary distribution concentrate on a single, non-random value. (We believe that the same is true for $a < \frac{1}{2}$ but we cannot prove this.) This is consistent with the behaviour of the ASM in one dimension, since the stationary distribution of the ASM is uniform over all configurations with at most one site with no sand grains. Hence, also in the ASM, the one-site marginals concentrate, in the limit as the system size tends to infinity, on a single value.

To state our result, we introduce the notation $\mu_N$ for the stationary distribution for the model on $N$ sites, with expectation and variance $\mathbb{E}_N$ and $\mathrm{Var}_N$, respectively.

**Theorem 11** [11]. *Consider the $(N, [a, b])$ model with $a \ge \frac{1}{2}$, and write $U$ for the random addition quantity. For the unique stationary measure $\mu_N$ we have*

$$\lim_{N \to \infty} \mu_N = \delta_{\mathbb{U}} \tag{2}$$

*where $\delta_{\mathbb{E}(U)}$ is the Dirac measure concentrating on the (infinite-volume) constant configuration $\eta_i = \mathbb{E}(U)$ for all $i \in \mathbb{N}$, and where the limit is in the sense of weak convergence of probability measures.*

We proved this theorem by showing that for $\eta$ distributed according to $\mu_N$, in the limit $N \to \infty$, for every sequence $1 \le j_N \le N$,

1. $\lim_{N \to \infty} \mathbb{E}_N(\eta_{j_N}) = \mathbb{E}U$,
2. $\lim_{N \to \infty} \mathrm{Var}_N(\eta_{j_N}) = 0$.

The proof of the first item is not difficult. However, the proof of the second part is rather complicated.

**Finally ...**

Many people have asked me at some point what the effect of the Vici grant has been on my career. On this point I can be brief: of course a grant like the Vici helps a lot and it is a wonderful feeling to be financially independent for a number of years. But there are other effects as well.

Supervising so many students in such a short time is rather demanding, certainly when you take the postdocs also into account. The grant was supposed to run over a rather limited period, and was not supposed to be extended too easily. Hence one should find good students on a rather short notice which has not been so easy.

Furthermore, we all know that in mathematics, one cannot advice too many PhD students at the same time. The way I dealt with it, was to ask other people to help me supervising some students. However, the point of

discussion remains: is the Vici grant system, with at least for mathematics a very substantial amount of money, the best way to finance mathematical research? Or would it be better to give less money to more researchers? I have the impression that the mathematics community would in majority vote for the latter and I tend to agree. This point becomes even more important these days, with even more concentration, for instance with the *Zwaartekracht* call. It is great, really, that so much money flows into mathematics, but I feel that mathematics would even be better off if that amount of money would be spread out more evenly over various disciplines within mathematics.

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