Sudoku Players' Forums

THE REAL DISTRIBUTION OF MINIMAL PUZZLES

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Author	Message

Red Ed
 	
Joined: 06 Jun 2005
Posts: 604
	Posted: Fri Jul 17, 2009 5:33 pm Post subject: quote
	
	Denis, can you revisit this please:
	
	denis_berthier wrote:
	
	Code:
	`26 1354 550.57`
	`27 679 15,185.39`
	
	The ratios look wrong (I'd expect about the same "corrected" number of 26s and 27s).
	
	Also, how many trials were involved? -- it would be good to know your estimates of the number of minimal puzzles, not just their proportions ... *and* we can get confidence intervals for those numbers. See my request to eleven and his response. It seems a shame to throw away information that is available to you for free.
	
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denis_berthier
 	
Joined: 19 Jun 2007
Posts: 741
Location: Paris, France
	Posted: Fri Jul 17, 2009 6:06 pm Post subject: quote edit
	
	Red Ed wrote:
	
	Denis, can you revisit this please:
	
	denis_berthier wrote:
	
	Code:
	`26 1354 550.57`
	`27 679 15,185.39`
	
	The ratios look wrong (I'd expect about the same "corrected" number of 26s and 27s).
	
	Remember that my sample has only 4000 puzzles when 1,000,000 would probably be necessary. All the above figures should be taken with extreme caution. However, in the case of 26s and 27s, it seems unlikely that a larger sample would make them ~ equal.
	Unfortunately, as I'll have my summer holidays soon, it is unlikely that I can have much larger samples before. And after, I'll have no time for Sudoku.
	
	Red Ed wrote:

> Also, how many trials were involved? -- it would be good to know your estimates of the number of minimal puzzles, not just their proportions ... *and* we can get confidence intervals for those numbers. It seems a shame to throw away information that is available to you for free.

This information is NOT available to me. I'm using the version of suexg modified by eleven. The only output is the puzzle.
My goals and my approach are different of yours.
I have no estimate of the number of minimal puzzles - nor do I need it.
Indeed, I defined the layered structure and the controlled bias algorithm to circumvent its estimation.

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Red Ed

Posted: Fri Jul 17, 2009 6:16 pm Post subject: quote

Joined: 06 Jun 2005
Posts: 604

denis_berthier wrote:

> **Red Ed wrote:**
>
> Denis, can you revisit this please:
>
> **denis_berthier wrote:**
>
> **Code:**
>
> ```
> 26          1354          550.57
> 27          679           15,185.39
> ```
>
> The ratios look wrong (I'd expect about the same "corrected" number of 26s and 27s).
>
> Remember that my sample has only 4000 puzzles when 1,000,000 would probably be necessary. All the above figures should be taken with extreme caution. However, in the case of 26s and 27s, it seems unlikely that a larger sample would make them ~ equal.

I disagree. I corrected the figures by multiplying up by choose(81,c) for each number of clues, c, and got very similar values for 26s and 27s. You must just have made a typo somewhere; please take another look.

> **Quote:**
>
> My goals and my approach are different of yours.

How do your goals differ from mine? I want the number of minimal puzzles; you want the same up to any fixed multiplicative factor. But the multiplicative factor is there for free - you are *choosing* to throw it away!

As for different approaches - I've already argued that, at least in the s=c case of my method, they are extremely similar.

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Red Ed

Posted: Fri Jul 17, 2009 6:24 pm Post subject: quote

btw, I estimate that you did ~1e9 trials. Wow, that's quite a lot of CPU.

Joined: 06 Jun 2005
Posts: 604

The standard error for each of your count values is ~sqrt(count value); so for example your 1354 has std err ~37, or about 2.7%. I can achieve the same performance in fewer trials: yours seems an expense approach.

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denis_berthier

Posted: Fri Jul 17, 2009 7:30 pm Post subject: quote edit

Joined: 19 Jun 2007
Posts: 741
Location: Paris, France

Red Ed, there was indeed an error in the formula as I typed it in the first version of my post. Thanks for noticing.
The correct formula is obviously the one for which the algorithm was designed from the beginning: cf(n+1)/cf(n) = (81-n)/(n+1).

I don't know if it is chance or anything else, but the 26s and 27s are now much alike, as you said they should.

> **Red Ed wrote:**
>
> > **denis_berthier wrote:**
> >
> > My goals and my approach are different of yours.
>
> How do your goals differ from mine? I want the number of minimal puzzles; you want the same up to any fixed multiplicative factor. But the multiplicative factor is there for free - you are *choosing* to throw it away!

It may be a simple change to make in suexg, but as I'm totally incompetent in C, I'm not really choosing.

> **Red Ed wrote:**
>
> As for different approaches - I've already argued that, at least in the s=c case of my method, they are extremely similar.

AFAIK, you want to build an unbiased generator. I only want to build a generator with controlled bias.
And I only need a very raw estimate ot the relative numbers of clues, because their impact on complexity of puzzles is limited.
But I think we finally have the same problem: finding sufficiently many 29s and 30s.

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Red Ed

Posted: Fri Jul 17, 2009 9:32 pm Post subject: quote

Joined: 06 Jun 2005
Posts: 604

> **denis_berthier wrote:**
>
> But I think we finally have the same problem: finding sufficiently many 29s and 30s.

Yep, and no very good ideas for tackling it 

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Allan Barker

Posted: Sat Jul 18, 2009 3:06 am Post subject: quote

Joined: 21 Feb 2008
Posts: 291
Location: Bangkok

> **Red Ed wrote:**
>
> > **denis_berthier wrote:**
> >
> > But I think we finally have the same problem: finding sufficiently many 29s and 30s.
>
> Yep, and no very good ideas for tackling it 

Generation and Rating of Big Minimals. Monte Carlo vs Monty Hall.

When rating minimal puzzles for SER, NRCZT, or even the number of intrinsic truths (BITS**), vs. clue size, it's not necessary to test the number of puzzles that corresponds to the natural distribution of puzzles vs. clue size. Rather, the distribution is an indicator that the puzzles are random.

The following is a way to quickly generate large clue size puzzles, hopefully, at random. Given a clue size C, the algorithm uses a top down approach to reach a random valid grid of size C, followed by a Monte-Carlo random convergence to a nearby minimal puzzle of the same size, while keeping the puzzle size unchanged.

Using this, I generated about 20,000 size 30 minimal puzzles in a few hours. I have generated numbers of 31s, 32s and 33s, and found a few 35s. Some data is shown below. Note, in this process there is no clue size distribution  however, the generator provides distances measured in terms of numbers of clues. Although the distances are very reproducible, their interpretation is not straight forward (but interesting).

Algorithm

0. Choose C = Desired clue size
1. Generate a random 81 candidate grid.
2. Randomly remove clues until clues=C, maintaining single sol. at each step. Call the G(C).
3. Measure Dinit., the number of independently removable clues. (for cross check only)
4. Maintaining C clues, Monte Carlo search for minimal puzzle.
5. Measure **Dwalk**, the number of clues that have changed. (D means distance)

Code:

```
          Distance Measure
        ------------------------
Clue    Dwalk   Dinit.    1-Per
---------------------------------------
34
33      17.72   19.20       ?    } 1-Per: average
occurance of
32      16.79   16.33       ?    } minimal G(C)s before
search
31      15.55   13.92       ?    } not measurable for
large C
30      14.66   11.89   20000??  }
29      12.95    9.13       ?    }
28      10.59    6.84     ?833
27       8.18    4.68     ~95
26       5.47    2.91      16.2
25       3.15    1.62      4.61
24       1.41    0.75      2.02
23       0.52    0.30      1.32
22       0.17    0.12      1.13
21       0.05    0.03      1.03
20       0.02    0.01      1.01
19       0.00    0.00      1.00
18       0.00    0.00      1.00
17
```

1-Per The measured average number of G(C) grids required to get 1 minimal of size C. This is not easy to measure for the large minimals but not important here.

Dwalk. The average distance traveled to find a minimal. Although I call this Dwalk, this is not a random walk, it's a convergence distance. Example

Code:

```
.-----------.       .-----------.
| A . | . . |       | A . | d*. |
| . B | . F |       | . . | . F |
|-----------| --->  |-----------|  Dwalk = 2
| C D | . . |       | C . | . b*|
| . . | E . |       | . . | E . |
.-----------.       .-----------.
```

I don't yet have a good handle on how these distances relate to "grid space" but they are very large numbers, as they should be. I'm trying to work that out now.

Dinit. Used only as a sanity check, the number of random clues removed to find a smaller minimal, like a normal top down generator. This puzzle is discarded.

If anyone is interested, I can put the first batch of 30s on my website. More later, off to the dentist.

** BITS, Basic Intrinsic Truth Scale.

Last edited by Allan Barker on Sat Jul 18, 2009 6:26 am; edited 1 time in total

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denis_berthier

Posted: Sat Jul 18, 2009 4:38 am Post subject:

quote edit

Joined: 19 Jun 2007
Posts: 741
Location: Paris, France

Allan,

Very interesting data and original approach of generation. If you think again of the interpretation of distances, let's know. There's probably some interesting theory behind it.

Allan Barker wrote:

> If anyone is interested, I can put the first batch of 30s on my website.

I'm interested. Large samples of 30s, 31s, 32s ... are not so frequent.
I think your procedure is sufficiently random to allow unbiased computations of the SER and NRCZT for each clue size.

If one wants to compute global unbiased distributions of SER and/or NRCZT (the reason why I opened this thread), one needs both the distribution for each number of clues (which are currently missing for large clue sizes) and unbiased distribution of clues (an estimation of which I hope to get with the controlled-bias algorithm). Your work is thus very relevant to what we're doing in this thread.

Not the proper thread for this (the "rating" thread would be more appropriate), but can you say more about BITS?

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denis_berthier

Posted: Sat Jul 18, 2009 6:28 am Post subject: quote edit

Joined: 19 Jun 2007
Posts: 741
Location: Paris, France

> **denis_berthier wrote:**
>
> > **Red Ed wrote:**
> >
> > > **denis_berthier wrote:**
> > >
> > > > Could you make the same tests on Allan's collection of puzzles (http://www.sudokuone.com/xsudo1/rab1mran.zip), generated with his own top-down generator whose first phase (complete grids) is based on completely different principles?
> > > > Why, if suexg was so bad in the complete grid generation phase, would Allan's generator give results in every respect very close to those of sudogen0_1M?
> > >
> > > I'll need to find the time to generate the complete solution grids from his puzzles but, yes, good idea, I'll do that some time.
> >
> > Don't waste your time doing this (I mean the complete grids). This computation is currently running, because I needed it for other purposes. As soon as it is finished, I'll put it on my web pages.

Done.
You can find it in the references, here:
http://www.carva.org/denis.berthier/HLS/Classification/index.html

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denis_berthier

Posted: Sat Jul 18, 2009 6:50 am Post subject: quote edit

Joined: 19 Jun 2007
Posts: 741
Location: Paris, France

As my sample of puzzles generated by controlled-bias algorithm grew from 4,000 to 5,000 I updated the results presented at the bottom of the previous page. I updated them on my web page
http://www.carva.org/denis.berthier/HLS/Classification/index.html but not here, so that you can compare.

It appears that the results are much more stable than I thought.
Although two 30-clue puzzles have appeared among the new 1000 puzzles, nothing's changed dramatically - except of course the unbiased distribution of the 30s.

The estimated unbiased mean number of clues is almost unchanged: **26.55** (instead of the previous 26.54). (Notice that eleven already found 26.55 with only 200 puzzles).
The estimated unbiased SER or NRCZT means and standard deviations are unchanged.

I think my approach of using a controlled-bias algorithm and applying correction factors provides a very simple and effective way of obtaining unbiased results.

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Red Ed

Posted: Sat Jul 18, 2009 8:49 am Post subject: quote

Joined: 06 Jun 2005
Posts: 604

denis_berthier wrote:

> I think my approach of using a controlled-bias algorithm and applying correction factors provides a very simple and effective way of obtaining unbiased results.

Sorry, but I've got to raise an objection. As I keep pointing out, your method is (mathematically) the s=c case of mine that pre-dated it. I don't know if it's intentional, but the tone of your announcements always seems to be that you are uniquely responsible for our knowledge of the number-of-clues distribution and that there is no prior art (for example, Knuth probing done by myself and Ocean back in 2006) nor current competing (I would say better ...) methods.

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Red Ed Posted: Sat Jul 18, 2009 8:53 am Post subject: quote

Joined: 06 Jun 2005
Posts: 604

Re solutions of Allan's puzzles, denis_berthier wrote:

> Done.
> You can find it in the references, here:
> http://www.carva.org/denis.berthier/HLS/Classification/index.html

Thanks; I'll test that now.

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denis_berthier Posted: Sat Jul 18, 2009 9:09 am Post subject: quote edit

Joined: 19 Jun 2007
Posts: 741
Location: Paris, France

Red Ed wrote:

> **denis_berthier wrote:**
>
> > I think my approach of using a controlled-bias algorithm and applying correction factors provides a very simple and effective way of obtaining unbiased results.
>
> Sorry, but I've got to raise an objection. As I keep pointing out, your method is (mathematically) the s=c case of mine that pre-dated it.

Then it will certainly be very easy for you to give references to:
- where you introduced the notion of a controlled-bias generator,
- where this generator is available,
- where you introduced the notion of a correction factor and where you gave the list of its values.

The only thing from you I could find about generators is filtering the output of a biased generator according to an unbiased distribution of clues that'd be computed elsewhere. This idea is very different from that of a controlled bias generator.

For the rest, everybody knows much work has already been done on the distribution of minimal puzzles and you've played an important part in it.

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Red Ed Posted: Sat Jul 18, 2009 9:17 am Post subject: quote

@Allan: that's a nice way of finding big minimals - you should cross-post onto the max minimals thread (I forget the exact title)

Joined: 06 Jun 2005
Posts: 604

the max minimals thread (I forget the exact title).

Your step (2) introduces bias for the same reason that the original top-down generator did; but it's easy to overcome, e.g. loop back to step 0 if you get multiple solutions (or, equivalently, generate (grid,mask) pairs until the c-clue subgrid under the mask has a single solution).

Step (4) just outputs a single minimal puzzle, the first one found, right? In that case it can't be used as an unbiased source of puzzles because you'd end up producing on average the same number of c-clue puzzles for every solution grid (and we know that different solution grids have different numbers of puzzles). I can't see it working as an unbiased estimator of the number of puzzles, either, but that may just be a failure of my imagination at this point.

In any case, a nice new(?) idea for big minimals!

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Red Ed

Posted: Sat Jul 18, 2009 9:32 am Post subject: quote

Joined: 06 Jun 2005
Posts: 604

denis_berthier wrote:

> The only thing from you I could find about generators is filtering the output of a biased generator according to an unbiased distribution of clues that'd be computed elsewhere. This idea is very different from that of a controlled bias generator.

Indeed; I should have been more precise. There are two aspects to this as I see it (1) the problem of estimating the number-of-minimals distribution; (2) the problem of unbiased generation of minimal puzzles for a fixed number of clues.

For (1), the *mathematics* in your case says: take the raw count and multiply up by choose(81,c) * k, where k can be any constant but you happen to have chosen k = 1/choose(81,24). In my case, the *mathematics* says: take the raw count and multiply up by choose(81,c) / (choose(s,c)*trials). It's essentially the same thing and quite clearly based on the same principles.

Still on (1), we mainly differ in the implementation. You find c-clue minimals by walking top-down. I just jump straight to a c-clue subgrid (in the case s=c at least) and hope that it's a proper minimal puzzle. Is one a more efficient way of coding it up than the other? I would guess that my way is, but I've honestly not checked. All this stuff about controlled-bias generators and correction factors is implementation detail: it doesn't make sense to challenge me to say where I introduced the same concepts ... because my implementation was different.

On (2), however, you are the only person generating unbiased samples of c-clue puzzles. I can't do that unless I set s=c in my algorithm, since when s>c my puzzles are not independent. But my goal is only to address problem (1).

I'm thinking that maybe someone needs to write a good clean summary of all the methods for unbiased estimation and generation. But the weather's too nice to bother with that now.

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