MOVING THE NEXT NOTES to the iPad. 10 August 2020

So: trying to generate truth w/ more action in expectations (k, b).

Observed:

1) If you increase the fe-support, fe's in the simulation become more tight around zero, and extrema are smaller. (E.g. $fe \in (-2,2)$ vs. $fe \in (-0.5, 0.5)$).

2) If you scale up α's, fe-distribution becomes more spread out: fatter tails

E.g. $4 \cdot \alpha^{true}$ gives the following fe-distrib

instead of sthg like

and $\bar{\bar{\pi}}$ fluctuates much more.

Estimating 4.α

- loss was initially much smaller 4.95 instead of on the order of 4000.
- estimation gets a lot of "fe was nan" messages and also a new one: "obj function returned NaN, trying a new point". then...
  - ↳ I already got this message 9 times
- It took 585 sec.
- Implied k^{-1} is < 0 4 times!
- → maybe that's for negative k?
- Again, $loss(\hat{\alpha}) = 3.2 < loss(\alpha^{true}) = 416$.
- Not possible to evaluate loss function?!
  - ↳ It seems to be nans everywhere?
    Is that why the loss fct returned nan b/c all simulations were nan in the cross-section?
- Look at the loss: it sucks:
  - for α_1, α_5, it's apparently always nan
  - for α_2, α_4 it blows up
  - for α_3, it's oscillatory.

Any $\alpha > 0.5$ gives all nans. Maybe that's b/c since the truth is higher, it more quickly explodes?

- I'm not thrilled that many histories explode.
- Also very many VAR instability problems.

Let's do more or less peak at the truth even now.
(well, α_3 doesn't and α_1 & α_5 have 2 minima around the true value, but still)

→ Does this indicate that the data-generation is wrong or the estimation?

- True moments changed very much:
  - moments pertaining to x scaled up, ones pertaining to π or i scaled down / were unchanged
  - own moments or cross-moments w/ π or i have the same shape
  - cross-moments w/ x have changed shape: are now more U- or reverse U-shaped.

↳ I think these make sense b/c since k is low, and $i = 1.5\pi$, x is the one that absorbs movements in expectations, while the pass-thru to π and to i is small.

⇒ The data-generation I think is correct. But this kinda suggests that the mean moments are also (more or loss) computed correctly, of which I'm not convinced.

↳ Let's try an estim w/ $\Psi_x = 0.5$.

- But suppose the data is generated correctly. Why isn't the "x absorbs all" mechanism matchable by the estimation? I mean, moments didn't move far from initial ones.

    ↳ Explosion % reveals that in the estimation, either 0% or 100% of the N simulations explode. No, sometimes it's 14, 79 or 99%. And it seems that the obj fit returns 'nan' when 100% explode. Yes.

    I learn also, by the way, that each iter of the solver evaluates the loss function exactly 6 times.

Ask Ryan

↳ Why? It's not changing α, or if it is, only one and only marginally

I'm also noticing that it's only considering α's really close to α_0 ⇒ multistart!

I'm also noticing that the simulation is not giving the 'fe was nan' error most of the time, even though the solver gets a positive expl-percent. Hm!

When the "fe was nan" error comes, then the explosion-counter does catch it. But apparently there are a bunch of explosions that come not from these.

Ok, it's clear: when fe is nan, it's always caught. The other thing that causes "explosive" simulations is $k^{-1} < 0$. These get caught and register as explosions, but aren't really.

→ My check for "global nonnegativity" one wasn't catching everything!

Iter 40 : that's where the troubles start.

⇒ Initially, fe is nan (maybe the gain is too high) but then the bulk of the supposed explosions come from $k < 0$.

Ok... even when fe is nan it seems it's b/c or at the same time as $k < 0$ ...

→ It's at the same time. The initial iteration involves 5% "fe was nan" (5/100 simulations) but 100% have $k < 0$ at one point!

Let's see for the July 6 dataset ... and yes, the initial iters involve 1% fe nan
69% k < 0

and later iters have 2% fe nan
86% k < 0

Converges after 23 iter.
Thus no more problems after iter 4.

Why is fe often nan early in the search and not late? Is it b/c of the initial values α_0?
And could a <u>finer, and more broad fe-grid</u> for the global nonnegativity test work?

↳ I introduced the variable <u>broaden=2</u>, which extends the fegrid-fine by ± broaden at both ends. It seems like this catches & subsumes all the previous errors!

It still finds the same min though :S
(But at least it's 2/3 of the time)

If I make the loss fct = nan when k<0 globally, I can engineer the "Obj fct returned nan" error message.

Does this influence the estimate? (Previously I had set the objective to 1e+10) No it doesn't.
→ I don't think so but it does affect the plots of the loss I would think!

Let's go back to the scaled-up truth and plot the loss. Should understand why k<0 : is it, as I think, that α isn't convex and so a fc outside the grid is extrapolated to a k<0?

The other thing is that this doesn't seem to really affect the behavior of the loss fct. It still converges to the same (wrong) thing. Actually, for the 4.α truth, it converges to sthg different, but most likely that's b/c it took an avg of a smaller set.

The loss: k<0 and fc=nan return. Does that mean that fcgrid-fine needs to be even broader?
Not necessarily b/c now it happens that e.g.

fc = nan in 14%

but k<0 in 0%.

In fact, k<0 doesn't happen a lot! fc is the problem here!

Why is it that for the estimation, $k \simeq 0$ is the root of all evil, but in plotting the loss, suddenly $fe = nan$ becomes the problem #1? Is it that the estim avoids high α's (or maybe also very low ones) that could cause turmoil?

<u>Materials 40, Section 4: Look into behavior</u> <u>11 August 2020</u>
<u>of simulation</u>. I think I learn a lot from this. Fig 7, which shows means & distribs of k^{-1}, $\bar{\pi}$ and fe are indicative.

1) While k^{-1} spends a lot of time being near 0 (see histogram), mean$(k^{-1}) \simeq 0.0185$ (see means)
↳ If the moments capture the latter aspect, then the estim should have a hard time pushing α_3 down. But the moments should more capture the first. Moreover, what does it matter if mean$(k^{-1}) = 0.019$? It still can enter the lower parts of its state space, and it does! So scrap that.

2) The cross-sectional mean of $\bar{\pi}$ & of fe ↓ as N↑. Maybe this is why loss ↑ in N?
I'm not sure this should happen? ⟶

As $N \to \infty$, $e \to 0$. So $fe \to 0$ and thus $\bar{\pi}$ too.

But that means that mean moments will reflect a sim in which $\bar{\pi}$ isn't moving much, and fe are very small.

Now turn to changing α's: do the following exercises:

- $4 \cdot \alpha$
- More edges: α_1 & α_5
- More middle: α_2 & α_4
- More 0-point: α_3

Obs. 1: If all α's ≤ 0.1, simulation doesn't blow up. At 0.11, they do. (2 occurrences of "fe was nan", one of which was also $k_{t-1} < 0$)

Obs. 2: $\alpha = 0$ doesn't cause explosions.

Obs 3: In the early scenarios, "fe was nan" is the only message. In the late ones, it comes together w/ "$k_{t-1} < 0$". Does this mean that $k < 0$ when α_3 is large?

↳ Making the code output α suggests that:

- "fe was nan" alone occurs when α_1 & $\alpha_5 \geq 0.11$
- "fe was nan" + "$k_{t-1} < 0$" occurs when α_2 & $\alpha_4 \geq 0.11$

- α_3 doesn't shit any water: it can also be 0.5, that doesn't cause explosions. (0-neighborhood indifference problem)
- these seem to occur across shock histories.

Now let's look at the plots of the means:

<u>Scenario 1: Varying α_1 and α_5:</u> 0, 0.05, 0.1

- Shifts k^{-1} up from (0.01, 0.012) to (0.02, 0.03)
- Shifts $\bar{\pi}$ from (−0.01, 0.01) to (−0.02, 0.04)
- Barely shifts fe: it remains in the (−0.2, 0.2) range

<u>Scenario 2: Varying α_2 and α_4</u>

- Shifts k^{-1} up from (0.005, 0.001) to (0.05, 0.06)
- Barely shifts $\bar{\pi}$: it remains in the (−0.01, 0.03) range
- Barely shifts fe: it remains in the (−0.2, 0.2) range

<u>Scenario 3: Varying α_3:</u>

- Shifts k^{-1} up from (0.015, 0.02) to (0.05, 0.06)
- Barely shifts $\bar{\pi}$: it remains in the (−0.01, 0.03) range
- Barely shifts fe: it remains in the (−0.2, 0.2) range

↳ So: why aren't $\alpha_{2,3,4}$ moving $\bar{\pi}$ & fe?
  ↳ is it b/c in that range $k^{-1} \cdot$ fe is a really small number? i.e.: is the 0-neighborhood indifference a problem

here too?

→ I think so! I think what is happening is that the entire $k \in (-1,1)$-region yields too small $k^{-1} \cdot k$ products such that they do matter a little bit (the loss does take a min there) but not a lot (the loss is very flat)

⇒ To be identified, you need to consider α's associated w/ $k > 1$ (possibly even greater). But: you also need a truth that's not too big, b/c $\alpha > 0.1$ seems to be ill tolerated by the model.

→ TO DO!

For Peter meeting:

① Loss does have a min, almost at right spot. (Fig 1)

a) Need convexity assumption – don't get removal (Fig. 2)

b) Rescaling W affects loss, although no indication of inversion issues. (Fig 3.b)

c) Adding E(·) screws things up in a way I don't get. (Fig 4)

(② Truth w/ more action in E(·)) (Fig 6)

I thought that if E(·) aren't moving, then both rescaling

and informativeness of moments might be screwed up.
Explosions hinted at something else.
$\alpha \,\dot{\le}\, 0.1$ to avoid explosions.

(3) Behavior of simulation

a) Loss ↑ in N ↑ b/c $\bar{\pi}$ & fe ↓ in N? (Fig 7 b&d)

b) Fig 8: Only α_1 & α_5 can affect $\bar{\pi}$, and even that can't affect fe. (Fig 8)

Peter meeting

11 Aug 2020

Fig 1. Loss higher at $\hat{\alpha}$ vs α^{true}

Again: b/c data from model is nonlinear, this may not suggest that α^{true} is a local min.

Thinks that a lot of problems come from that est a nonstat model is tricky.

The next issue: holding α's fixed, but estimating more than one α may not work. This would happen if you est $Y = \beta_1 X_1 + \beta_2 X_2$ and X_1 & X_2 multicollinear.

↳ might be some weird interaction between α's so that the concavity restriction helps provide curvature.

Fig 8 conclusions sound exactly right.

Rescaling: still quite strange

Fix α, check loss w/ and w/o scaling $\rightarrow$ should recover the scaling factor.

Also explains why imposing convexity helps.

Wanna guard against explaining what is really a coding error. $\rightarrow$ Should go back to that.

The level of loss fct has no meaning; only the curvature.

Min in Fig 1. don't line up w/ truth: not a problem b/c if you sim using a diff c, you'd get a diff min.

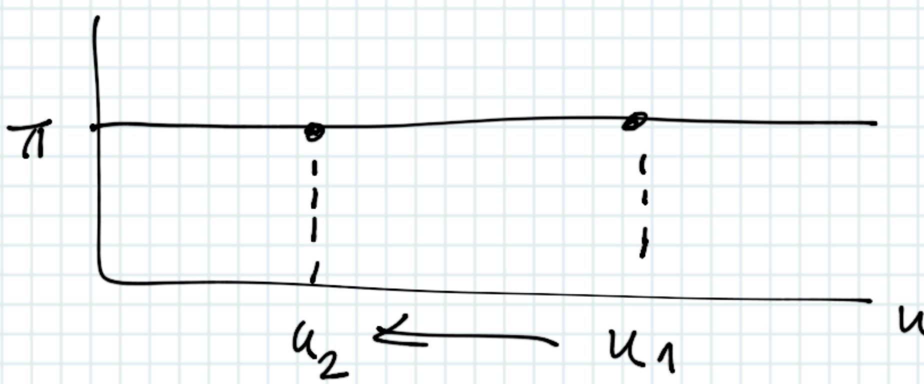
If std. errs large, can be b/c of
- $\rightarrow$ ID $\rightarrow$ doesn't go away as $N \rightarrow \infty$
- $\rightarrow$ sampling error
  - $\hookrightarrow$ goes away as $N \rightarrow \infty$

Recall the logistic functional specification for anchoring fct: somehow more structure has to be imposed on the anchoring function to guide the info in the data

Analogy: Selena?
Silvana Tenreyro: $\hat{PC}$: slope is ≈ 0 → b/c CB is credible! So est a PC w/ these data doesn't tell you what would happen if CB abandoned target. (b/c that's just not in the data!)

→ In obs that have modest fe, we don't learn a lot about how agents learn from forecast errors.

"flat PC": "π isn't moving in tandem w/ u anymore"



↳ he says that the "flat PC" term refers to the idea that π doesn't follow u, not the other way around!

low u doesn't lead to any diff π!

- A fall-back approach: a simpler functional form for anch. fct.
- Or: pick unconditional moments w/o VAR or weighting matrix. He called it an in-between btw est & calibration. "Moment-matching": take sample moments & match those.

Successful esti will involve picking restrictions on 1st OR on the anchoring fct to allow to glean info that <u>is</u> in the data.

↳ And I'm not sure why he calls the "moment-matching" by that name: it's not much different than my SMM.

Let's try the thing w/ α's out in the edges. <u>12 August 2020</u>

Attempt ①: July 6 data, fegrid = [-2, -1.5, 0, 1.5, 2]
(truth same) converged, not much change.

Attempt ②: July 6 data, fegrid = [-4, -3, 0, 3, 4]
Took 500 sec & converged to sthg quite diff!
Not correct but maybe getting near?

Attempt ③: July 6 data, fegrid = [-4, -3 ; 3, 4] nfe=4
Took 115 sec & converged to sthg very diff.
Isn't correct either but the thing is that the "truth" doesn't have a lot of fe in the 3-4% range.

Attempt ④: July 6 data, fegrid = [-4, -3, 0, 3, 4]
but w/ Wmid=1000.
Took 233 sec. Looks good. fe ±3 & 4 are still off, but I'm hopeful.

Attempt ⑤: generate truth with large forecast errors which you can est. w/ fegrid = [-4, -3, 0, 3, 4]

First day Aug 10 data ($4 \cdot \alpha^{true}$)
(w/ Wmid = 1000)

I'm also noting that the convexity thingy ain't working well.

Took 643 sec.
Got a bunch of "3% of sim histories exploded" messages.
There we go, lookin' good!

~~Attempt~~ ⑥: Make α's associated w/ small fe large, so the truth looks something like:

Oh shit. This is exactly the 4 x times thing!

And $5 \times \alpha^{true}$ explodes already.

I don't think I can generate a dataset w/ bigger fe b/c large fe need large α's, but sufficiently large α's also explode, so...

Real data with Attempt ⑤ settings leads to a 1-2% sim histories exploded sim. 390 sec.

I think I'm going to leave it at that and consider the rescaling issue. Can I recover the scaling factors? Aand... no. Actually yes, if you recall that the loss involves squaring W.

↳ Ask Ryan: could it make a diff that in Lsqnonlin you specify the equations resids · W?

<u>Applied Young Economists Webinar</u> (AYEW) <u>22 Aug 2022</u>

Nan Hu & Zexi Sun "Vague Talking at Central Bank's Press Conference: News or Noise?" (19 participants)
Hong Kong Institute for Monetary and Financial Research (← note)

interpret as "news" vs. noise: Vague talking that includes words like "risk", "uncertainty", "volatility" and "turbulence" raises stock returns
→ b/c investors expect expansionary mon pol in the future: the ⊕ news dominate the ⊖ news that "current sitn is bad".
Expansionary disinflation (Ball 1994)

dictionary from Loughran & ..., freq. of uncertain words

Regression: $R_t = b_0 + b_1 \Delta \text{vaguetalking}_t + \gamma'_b X_t + \varepsilon_T$

First diff b/c EMH: stocks should only react to <u>surprise</u>

Prepare Ryan meeting:

(1) Loss has a min at truth (Fig 1.)

(2) Loss is indifferent to α's such that $|fe| \leq 1$ (Fig. 8)

(3) Having a more action in fe truth and est-ing α's corresponding to large fe works (Fig 15)

Details & problems:

(4) Solver evaluates loss 6 times for the same α?

(5) Rescaling: changes shape of loss while no sign of inversion issue?

<u>Ryan meeting</u> <u>12 August 2020</u>

- Need to est σ of shocks in order to get model-implied moments closer to those of the data, at least for π. → or play around w/ 'em.
  ↳ If shocks too small, no fe ⇒ ID problems

- Isn't the whole story, b/c edgepoints also off

- Fig 8: wouldn't learn more if you plotted the auto-covariograms for these exercises!

→ So what's still amiss? We still don't know.

Weighting matrix: Eq (2): multiply by $\sqrt{diag(w)}$

→ Take the $\sqrt{\cdot}$ b/c otherwise you may put all weight on one moment.

· My rescaling strategy likely not to solve inversion issues b/c it's the ratio $\frac{\max(\text{element})}{\min(\text{element})}$ of elements of Σ which matters. If it's more than 10e6, then problem.

Ryan's strategy

→ Fix largest el. of Σ and fix all elements to be not smaller than 1/10 000 the largest.

· Not aligning the right els of W w/ the right moments could be a potential problem.

· Search alg:

6 evals b/c it computes gradient & Jacobian.

(by 100 say) You can multiply the step for eval of ↑, then the est. of derivatives is less precise but you get a better idea of the slope.

"first/fwd difference"

Search algorithm evaluates loss 6 times for each guess b/c it 1.) evaluates at the guess

2) Changes each α_i a LITTLE bit to estimate

first and second derivatives. It changes one α_i at a time, so this gives 5 additional evaluations.

Main point: the story that we can't use small fc to learn about how agents learn from fc is good, but it isn't the whole story. And one may be that shocks are too small, partly responsible for why there are no large fc in the data.