NEXT 26 Aug 2019

See Experimentation Notes 21 August 2019 Ryan Meeting

Benhabib, Schmitt-Grohe & Uribe (1999)

- challenges the conventional notion that active pol. (mon. pol that responds more than 1:1 to π) is stabilizing → it is only stable (unique) in a "very" local neighborhood of the st. st.

→ π fluctuates around its "stable" value for a while before converging to the passive st. st.

↳ makes emerging liq. traps hard to detect!

Needs ZLB.

↳ Is this a feature of learning models?

I said in the meeting that "ZLB episode should never have happened if beliefs were anchored"

→ I meant that $\pi = E(\pi)$, (kind of) and if $E(\pi) = 3\%$, π could never have gone down so low as to warrant $i = 0\%$.

→ Ryan said something like what if we were in the low-π period just τ periods too short, so that if it had persisted τ more periods, expectations had become unanchored and we'd never have gotten out?

2 options for Benhabib et al (1999):

- either learning models rationalize multiplicity for active policy
- or learning models offer a different explanation for the slide into liq. traps.

Davig & Leeper (2007)

Markov-process for Taylor-rule parameters

expectations-effects: even if you're in an active regime, if ppl. expect that you may switch, macro volatility ↑

You can get indeterminacy if passive regime is
a) sufficiently permanent b) or sufficiently passive

↳ this is in spirit like unanchored $E(\cdot)$.

⇒ Regime-switching <u>increases</u> the local determinacy region b/c you can "store up on" hawkishness so you have more allowed "dovishness credit"

↳ regime-change on the policy-side
vs. CEMP: regime-change on the learning-side

Interpretations of US mon. history: 27 Aug 2019

- Benhabib et al (1999):

  It's not actually active, but it's spiraling into a liq. trap (:S)

- Davig & Leeper (2007)

  The 70's wasn't indeterminate b/c the LRTP (LR Taylor principle) holds, but there were large shocks that were amplified by policy.

- CEMP

  The 70's was due to large gains.

  In "Limits to Mon. Pol" C&P & Giannoni also argue that the gain was large mainly due to loose policy in the 70's (less due to shocks)

Inflation targeting countries:

New Zealand (1990)

Canada (1991)

UK (1992) (RPI of 2.5% → CPI of 2% since 2003)

SWE (1993 announced, applied 1995)

ECB 2%

US (2012) (2% PCE)

→ in the data, what I see is that CPI inflation came ≈5 yrs after the intro of π targeting.

Let's add that Svensson is essentially saying that the "LR-PC becomes non-vertical when infl. $E(\cdot)$ are anchored" i.e. he's saying that the u-π tradeoff survives into the LR then

→ money non-neutrality in the LR.

CEMP-view of π-development:

$$\pi_t = E^{LR}(\bar{\pi}) + \text{shocks}$$

MC mon. pol

Svensson is saying

$$\pi_t = \overline{E^{LR}(\bar{\pi})} + \text{shocks}$$ < what it should be

MP didn't do enough!

$\Rightarrow$ so when $E(\cdot)$ anchored, MP very strong in terms of output gap / unemployment control.

$$\pi_t = E^{LR}(\pi_t - \pi^{Target}) + \text{shocks}$$

when unanchored, E^{LR} responds a lot to missing the target $\rightarrow$ overshooting gets amplified (& vice versa)

$\Rightarrow$ self-referentiality.

$\Rightarrow$ self-referentiality seems to make money neutral faster (the LR arrives quicker / earlier)

$\Rightarrow$ unanchored makes MP weaker: bigger interventions are needed.

→ the "Svensson scenario" doesn't describe current US monpol well, though:

• $\pi < \bar{\pi}$ while $u < u^n$

Or does it? Can we think of a story in which

$$\pi_t = E^{LR}(\pi_t - \bar{\pi}) + \text{shocks} \quad \text{(unanchored)}$$

② ↑ ↑ ④ ③ ↑ ① ↑

vs

$$\pi_t = E^{LR}(\bar{\pi}) + \text{shocks} \quad \text{(anchored)}$$

② ↑ ① ↑

→ π doesn't increase as much while labor market effects are large!

But this is where the "where are they anchored?" comes in:

$$1.5\% = \underbrace{E^{LR}(1.5\% - 2\%)}_{<0} + \text{shocks}$$

<0 but maybe not large or persistent enough for expectations to adjust. → this story is harder

to tell if expectations are anchored at 3%,
a much higher level.

⇒ the question though also is "whose
expectation"?

→ the 3% may be lower if the agg. econ's
expectations overshoot less than those of HHs.

But ok, at least I can rationalize Svensson's story
– and maybe past errors need to be very big
or very persistent for expectations to become
unanchored.

⇒ and I've also rationalized why the ECB
was scared of unanchoring of expectations
during the crisis:

- spiral down
- loss of control

• ZLB

I agreed w/ myself that much larger MP shocks are necessary to move π if $E(\cdot)$ are <u>not</u> anchored.

→ it's poss. to get into ZLB w/ anchored beliefs by being unlucky:

$\pi_t = E^{LR}(\bar{\pi}) + \text{shocks}\downarrow$

→ and getting out should be a lot harder (require bigger MP shocks) if beliefs become unanchored b/c as long as $\pi_t < \bar{\pi}$

$\Rightarrow E^{LR} < 0$ which pushes $\pi \downarrow$

What if they get unanchored again at a lower $\pi = 1\%$? Well then it's easy to get to $\pi = 2\%$ one-time, but it will take a persistent series of MP shocks to maintain that level unless you "unanchor" beliefs in order to shift the anchor to the correct place → this seems to be an "overshoot-risky" thing.

Divine coincidence

no tradeoff btwn output gap - & π stabilization

In COMP there's no demand side

↳ well now after the Peter meeting it feels like the DC doesn't hold: Fed trades off π-exp(·) ⇒ π vs. output in the SR!

Peter meeting 27 Aug. 2019

- diss-fells. 2 Prezi oct 1.
- Preston
- new dir: a learning model take on std issues:

i) infl targeting ii) credibility iii) effectiveness of MP

iv) "anomalies of Taylor-rules

Benhabib, Schmitt-Grohe & Uribe (1999)

global stability of TR: 2 eqb < active / passive

→ fall into Liq. traps

Davig & Leeper (2007)

LR-TP: expanded determinacy region when

monpol can be thought of regime-switching.

Preston

- Try a simul w/ only 1 source of randomness or no randomness at all ($\bar{i}$ or r^n)
- − instead of a +.

To Benhabib et al:

Fed changes TR only around ZLB

→ we follow a TR but when $i = 0\%$

we keep it at 0%.

→ nonlinearity in TR (Benhabib et al)

↳ vs. we switch to sthg else at ZLB

(regime-switching)

Benhabib:

In RE, the bad eqb is the attractor: how much

of that depends on RE ass? Would it be

worse / better w/ learning?

Beware: global analysis w/ learning might

be tough

Another: COMP-exp.

Compared to the case of RE → the CB may be

forced in some sense to deviate from a TR or adopt a diff TR w/ diff coefficients just to keep expectations anchored / maintain credibility

↳ tradeoff btwn π-management & credibility

→ sweep in Leeper

std TR

diff regime when unanchored

⇒ could be done using linear methods and simulation

"here's a MP rule that preserves credibility and here's a switching regime that gets the best of both worlds"

↳ and data (estimate using the gain-result from CEMP so you don't have to estimate the beast)

Erceg & Levin JME (2003)

Volcker-disinflation not explainable using RE

dynamics of (π, Y) cannot be explained

w/o a signal extraction on CB's target

Marvin Goodfriend (1993) "Interest rate policy

and the Infl. Scare Problem"

Credibility of Fed was called into question

→ so when FOMC took actions that

weren't justified by TR it's b/c they

wanna preserve credibility.

\+ "whatever it takes" (Draghi)

"the Fed listens" Chicago conference

idea here (Powell & John Williams & Clarida)

"we don't wanna get into low E( ) "

"better act now than later"

Leeper, Preston, Margaret Jacobson
"Recovery of 1933"
what ended the Deflation of Great Recession
FDR took over and I'm in charge of the Fed"
→ under those circumstances it's ok

Work after

Eruzy & Levin: a DSGE model in which agents try to disentangle permanent vs transitory shocks to the inflation target

↳ Evans & Wachtel (1993): show using survey data that persistent fcst errors aren't irrational, instead they reflect uncertainty on the regime

Goodfriend: inflation scare: when market-based π-expectations jump up (here: the LR int rate), indicating low credibility of the Fed → Fed has to raise the FFR to indicate its commitment to low π & maintain credibility.

The main takeaway seems to be:

a diff take on US mon history (echoing Goodfriend's idea of an "inflation scare") is that under learning, there's a tradeoff btwn mon. objective and credibility → this can explain US mon history as well as the recent int. rate cut (July 2019) as signalling commitment to the 2% target.

→ Could demonstrate in an NK model that

- when anchored, a TR does fine
- when unanchored, a new rule does better

⇒ a hybrid rule that is regime-switching gets the "best of both worlds"

→ and this kind of behavior is what policy-makers are talking about.

⇒ would also shed light on the "flat NKPC" issue:

- when unanchored, not flat, but you fight to get anchoring
- when anchored, flat b/c $E(\pi)$ don't respond.

Let's return to the question 28 Aug. 2019

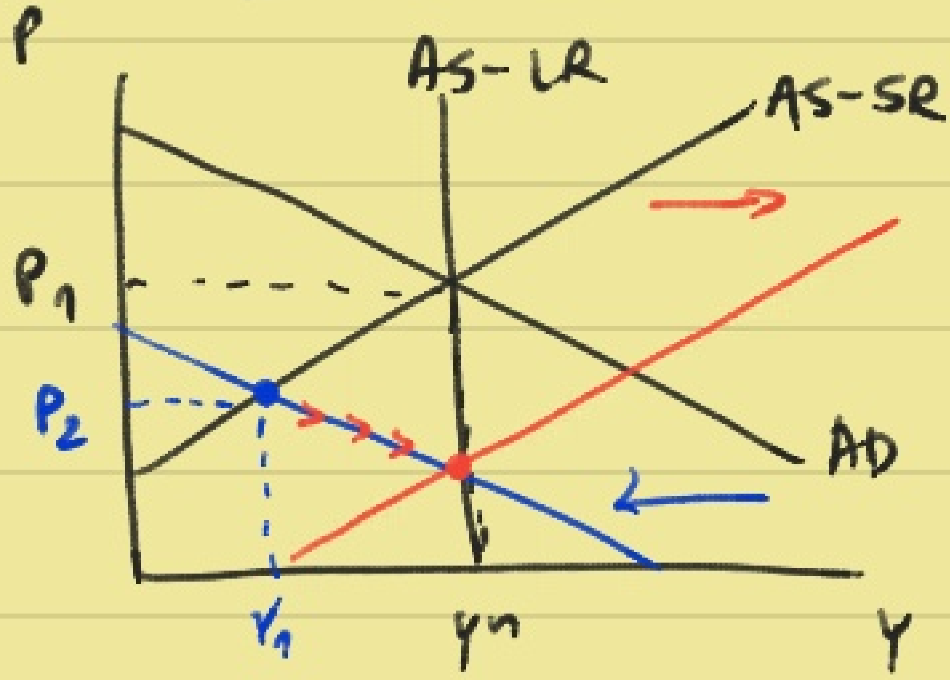
of <u>when monpol is powerful under learning</u>:

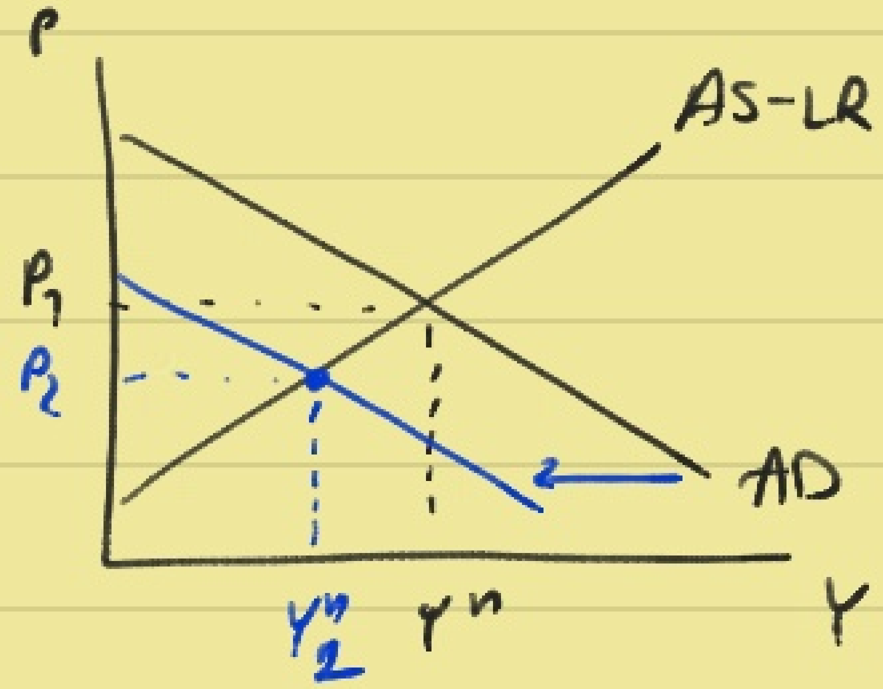
$$\pi_t = E^{LR}(\pi_t - \bar{\pi}) + \text{shocks}$$

anchored: can't move expectations

need bigger shocks to move π

(can be a blessing & a curse)





$P_2 < E(P) \Rightarrow E(P)\downarrow$

$\Rightarrow$ AS-SR shifts R

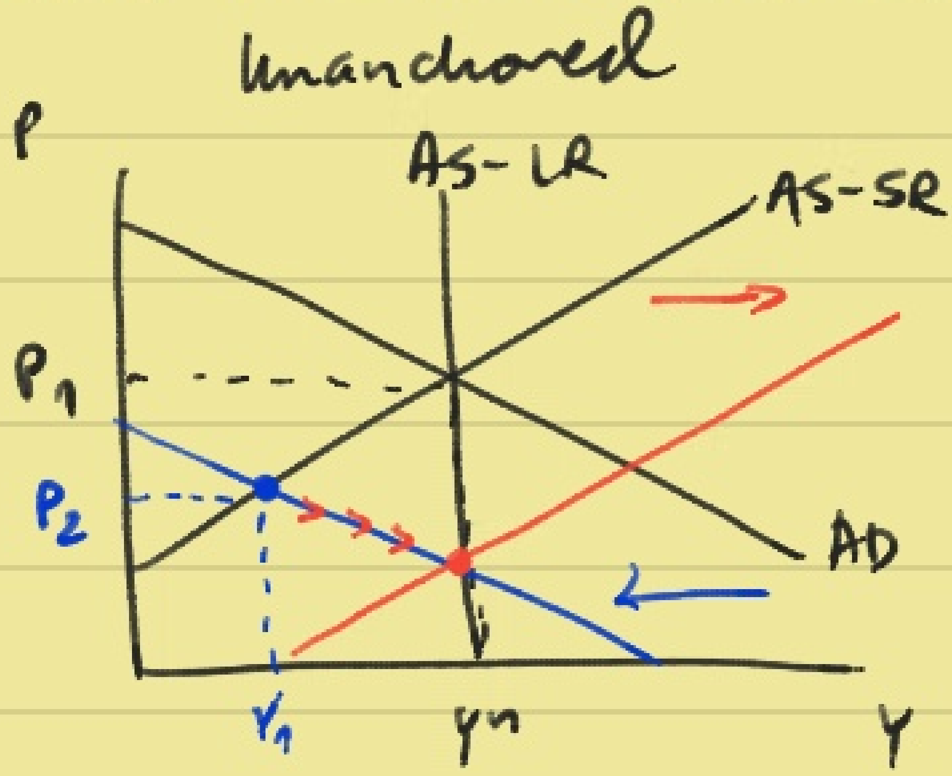
Technically, if one had ∞ shocks one could keep the output gap open forever

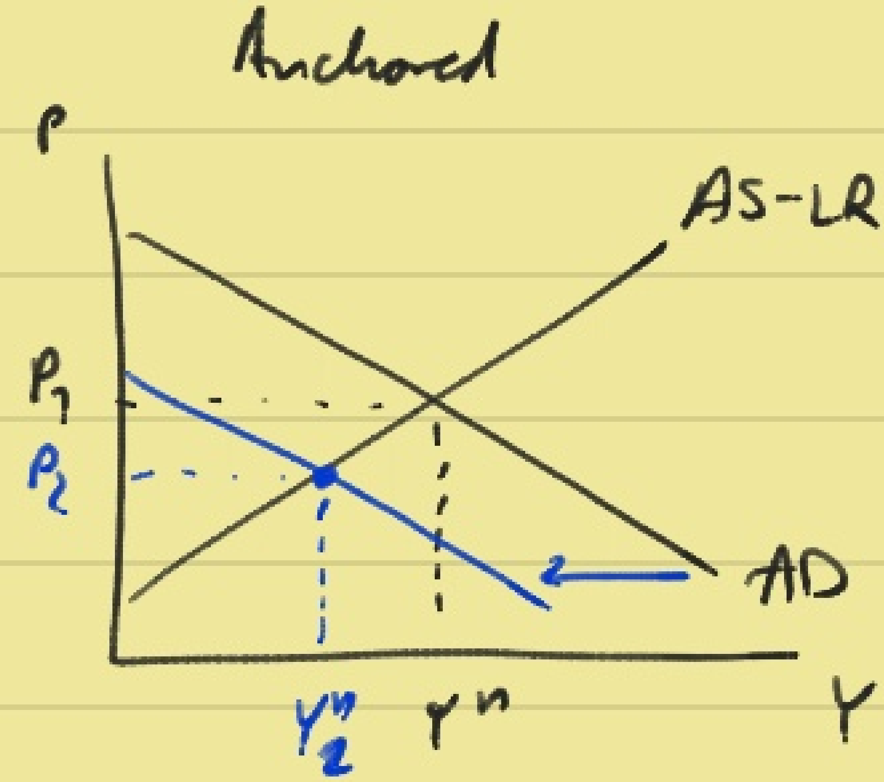
$\rightarrow$ but then they deanchor!

$\Rightarrow$ so <u>anchoring makes MP powerful</u> b/c you can

- keep the output gap positive for a longer time
- shift AD back to smooth a crisis before the extra deflation occurs.

Let's look at this situ more:

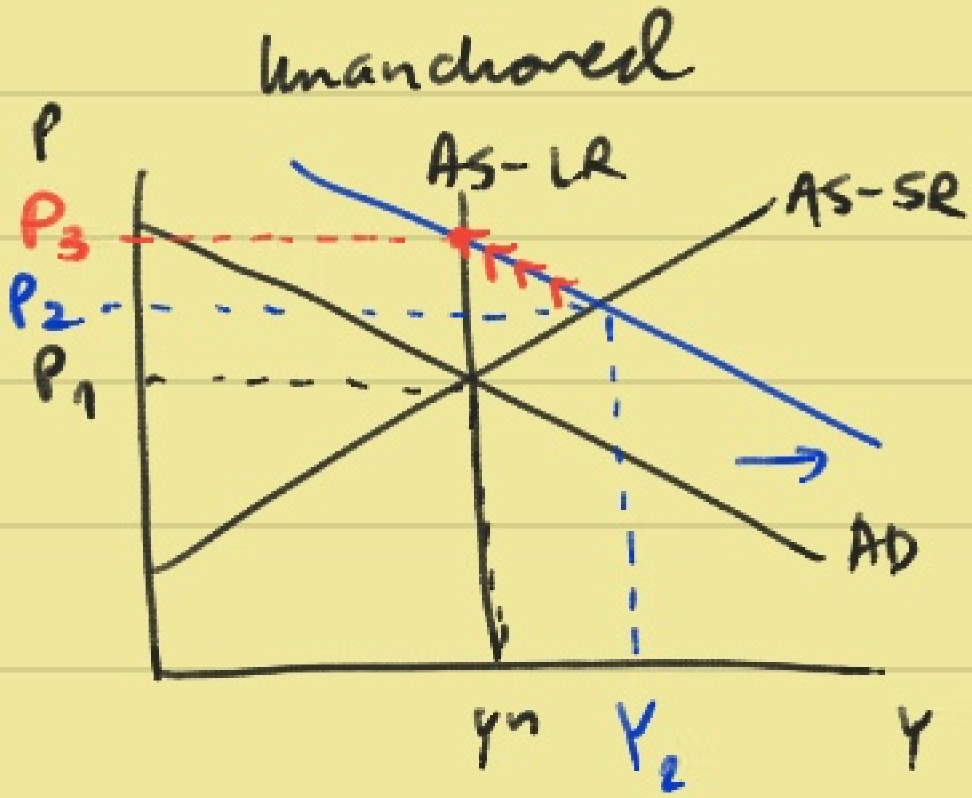


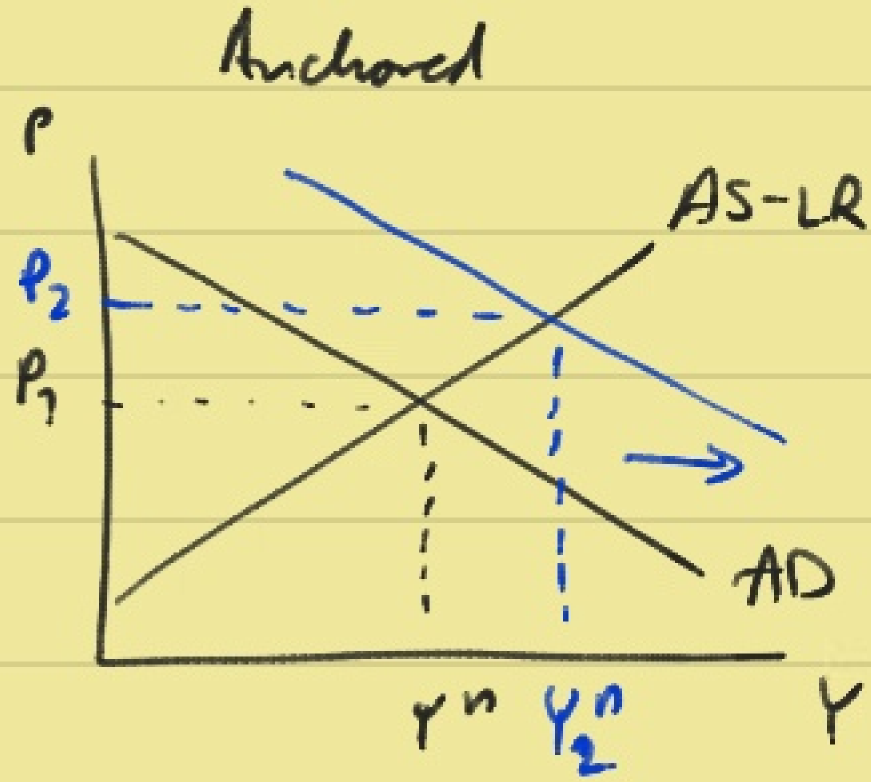


Already here being anchored is two-sided:

- Curse: in principle the unanchored econ would go back to a higher Y^n, lower u^n situ by itself.
- Blessing: prices stay higher. And given that mon pol needs to intervene to get back to target π^* (and implicitly u^*, Y^* too), it's better that prices haven't fallen as much. Also, if unanchored, adjustment is costlier (you need bigger shocks to move AD by the same amount)

Let's look at the reverse situ: an inflation scare:





→ mon pol, when E(·) anchored, could "persistently" push up output (open the output gap). The problem is that there is a threshold surprise, or a threshold length of time of consecutive surprises such that E(·) deanchor → you set off inflation (red)

⇒ it is to keep that from happening that you raise int. rates and thus dampen momentum although you might not even have hit the bound → credibility

Ryan meeting 28 Aug 2019

Pol: • Inflation-targeting countries (some mixed evidence)

• Benhabib global → diff.

↕ opposition

• Davig & Leeper — related thought to Evans & H (2003)

A synthesis: investigate the tradeoff btw stabilization & credibility ⇒ propose a modified TR under learning

Addresses: – current US policy

– current talk of anchoring

– flat NKPC

– ZLB a bit → a case for unconventional pol.

Doesn't address:

– divine coincidence (I don't know!)

– why E(·) didn't become unanchored in SWE / Riksbank undershooting

A confusion: anchoring doesn't mean in CEMP that SR-Exp. don't move!

The actual mechanism:

CB min $\mathcal{L}$ s.t. credibility stock

Try to do this:

- think through how a cost push shock works in an NK model
- do the same in an NK model w/ learning where the cost push shock affects π and thus also $E(\pi)$ → what's diff?

Note: a cost push shock is a shock to desired markups
→ it increases the wedge between perfect competition and mon. comp, and then the flex. price isn't efficient (b/c distortions). So we don't want to stabilize to flex prices b/c that outcome isn't optimal.

Work after

what are cost push shocks?

Ryan said: shocks to the desired level of the markup. (μ)

$$\mu^* = \frac{\theta}{\theta - 1}$$ where θ = el of subst between varieties.

Peter in his 2004 NBER WP "Tech Shocks..." indeed defs it as

$$\ln(\theta_t) = (1-\rho_\theta)\ln(\theta) + \rho_\theta \ln(\theta_{t-1}) + \varepsilon_{\theta,t}$$

→ i.e. Ψ_t process of CEMP.

And this is what makes sense to me!

But Peter refers to Clarida, Gali & Gertler (1999), who define it as:

NKPC: $\pi_t = \lambda x_t + \beta E_t \pi_{t+1} + u_t$ (2.2)

which "captures anything else [than excess demand, which in turn is captured by the output gap, x_t] that might affect expected marginal costs."

or: u_t is the deviations from the condition $mc_t = \kappa x_t$.

Why are markups endog. in a sticky prices world?

And let's see the condition $mc_t = \lambda x_t$

- $\hat{Y}_t \uparrow \rightarrow W \& R \uparrow \rightarrow \hat{\varphi}_t\ (mc) \uparrow \Rightarrow \hat{\mu}_t \downarrow$

  (b/c prices sticky)

- and Barro Sims 2, p. 47 & 49, we have

$$\hat{\varphi}_t = \hat{mc}_t = (\eta + b)\,\hat{x}_t$$

Further on p. 49:

target markup $= \frac{\theta}{\theta - 1}$ (always constant)

vs. actual markup $\hat{\mu}_t = -\hat{\varphi}_t$

25 Aug 2019

at the same location, I find the following

"From PS we see that sticky prices, that is variable MC or markups, means that $\hat{P}_t^* \neq \hat{P}_t \quad \forall t$ (!)"

and the wedge between those is MC:

$$\hat{P}_t^* - \hat{P}_t = \hat{\varphi}_t \qquad \text{(PS)}.$$

I think the argument is sthg like this:

w/ mon. competition, the PS is:

$$\frac{P_t^*}{P_t} = \frac{\theta}{\theta-1}\varphi_t \qquad \text{(PS, Basu-Sum Part 2, p. 47)}$$

(already ass-ing symmetry btw i & j)

Loglin gives:

$$\hat{P_t^*} - \hat{P_t} = \hat{\varphi_t}$$

With flex prices, $\frac{P_t^*}{P_t} = 1$ ⇒ all firms set a price that's equal to the agg. price ⇒ $\hat{P}_t^* - \hat{P}_t = 0$ ⇒ MC constant!

2 reasons for confusion

- In Rotemberg, isn't $P_{it}^* = P_{jt}^* = P_t$?
- And it's fine that $\hat{P}_t^* - \hat{P}_t \neq 0$, but why does that change my cost structure?

↳ answer this might be ("CalvoOnceForAll.pdf")

MC

$$\hat{s}_{it} = \hat{s}_t + \frac{1-\alpha}{\alpha}(\hat{y}_{it} - \hat{y}_t)$$

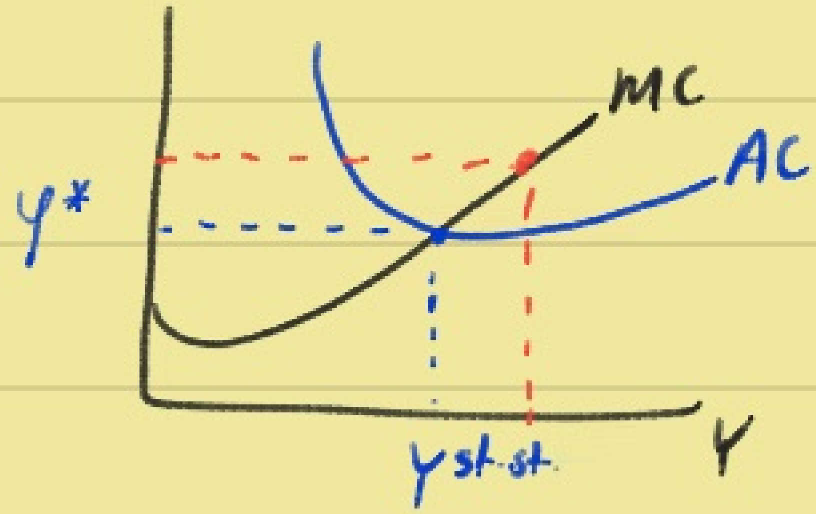
"my MC is that of everyone plus a demand term"

the demand term tho is: $\hat{y}_{it} - \hat{y}_t = -\theta(\hat{P}_{it}^* - \hat{P}_t)$

My demand is
- how much cheaper my good is $(\hat{P}_{it}^* - \hat{P}_t)$
- how much different the good is (θ)

↳ but that brings me back to the same question: Why does this change my cost?

→ I think I see actually: Recall from micro that MC is upward-sloping:



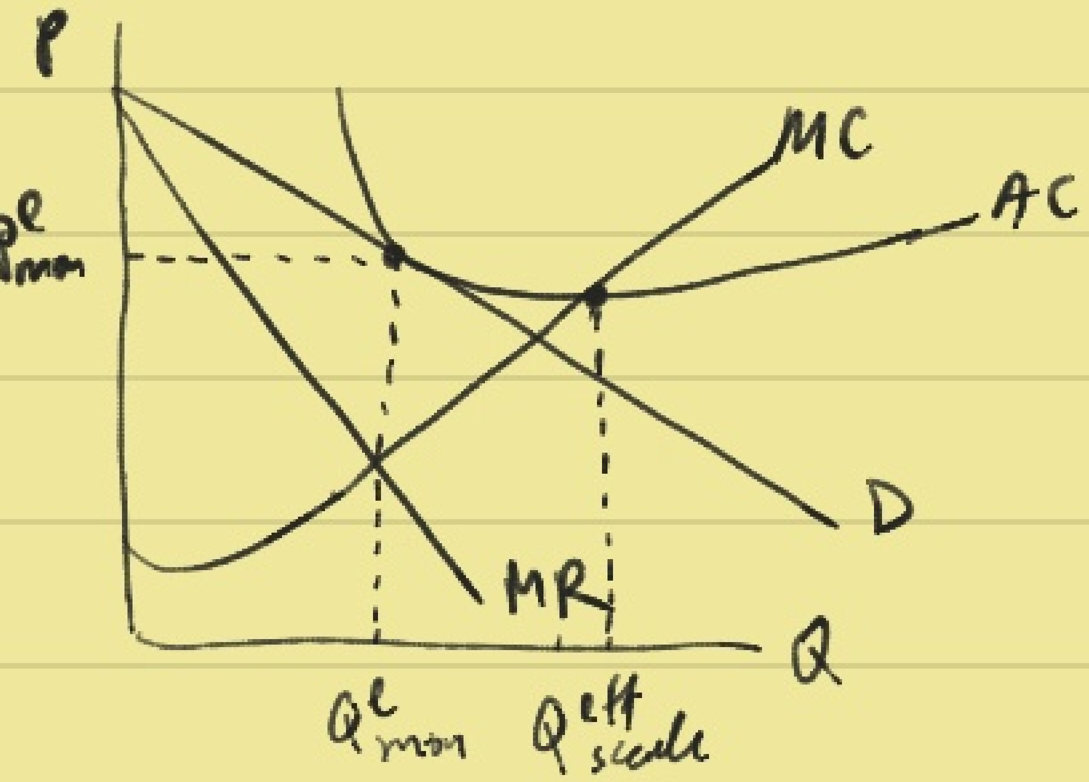
⇒ the higher the prod, the higher MC.

And that's a constant st.st. level.

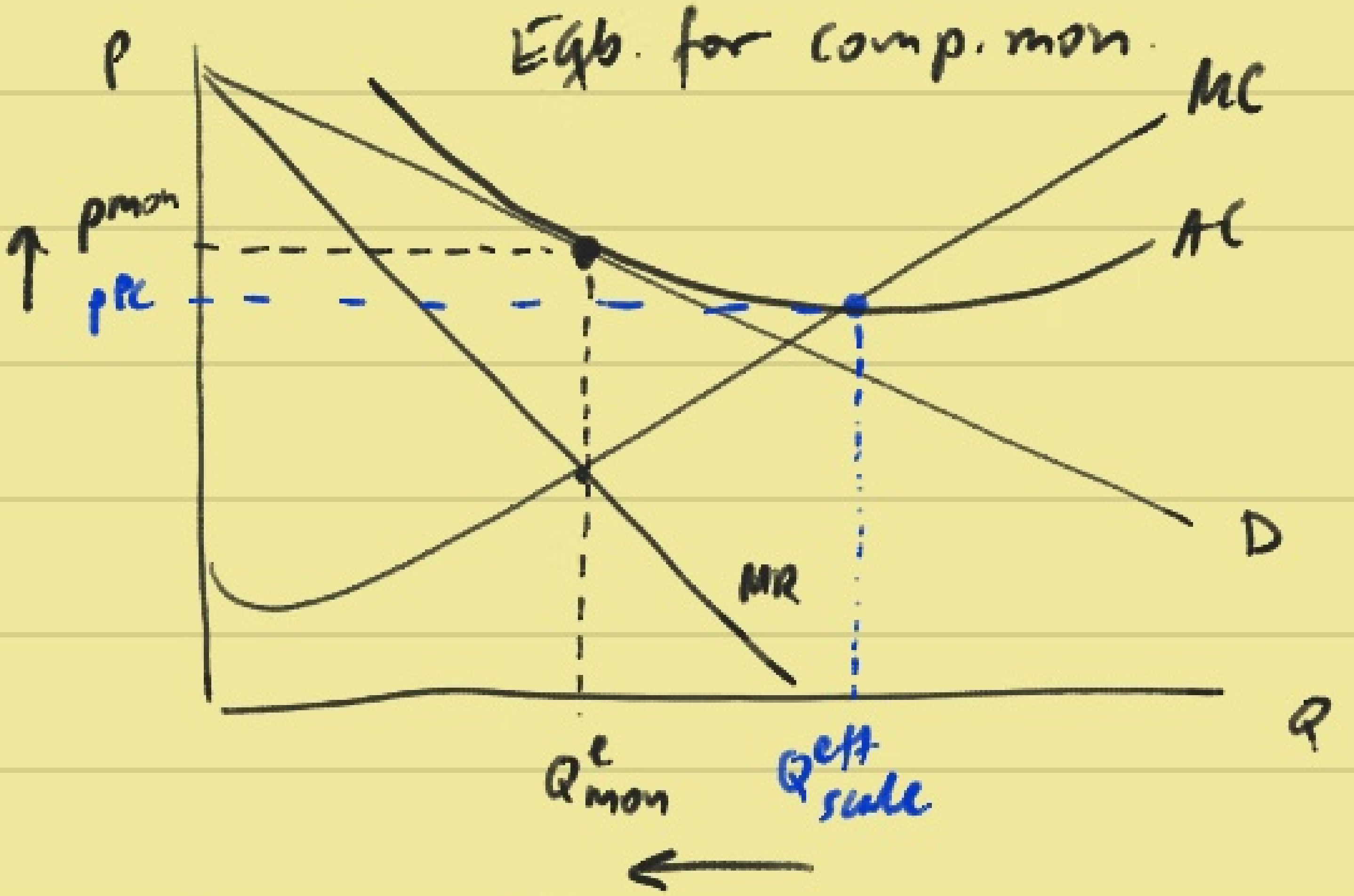
But when $\hat{P}^{*}_{it} \neq \hat{P}_t$, as is the case in Calvo, this causes $Y_{it} \neq Y_t$, i.e. your demand isn't = to the agg, and so your level of production isn't either.

If your prod. level exceeds the agg, so will your MC.

Let's go back to micro principles: competitive mon.

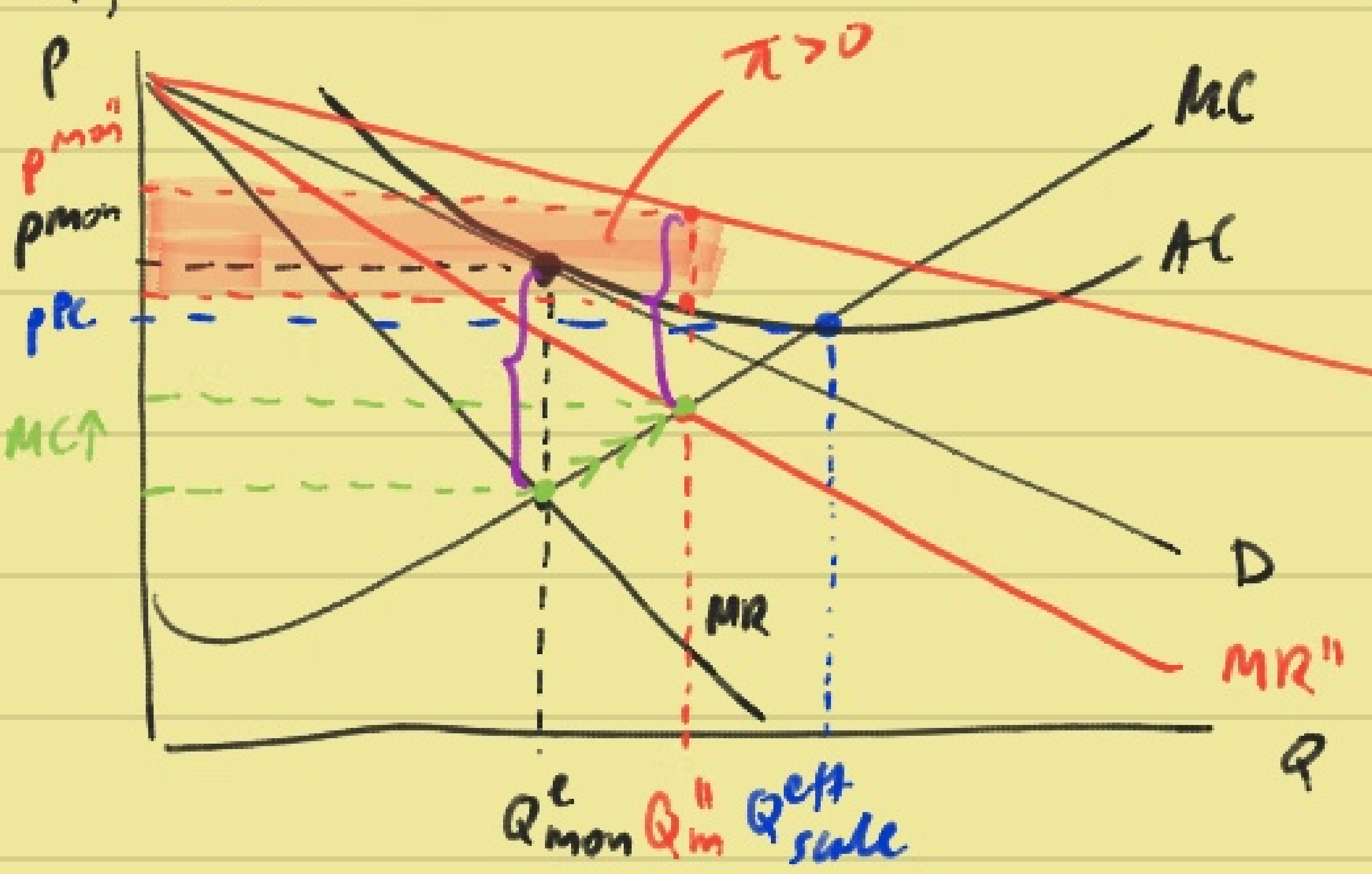


Let's compare that w/ perfect comp.



(p^{mon}, Q^{mon}) is the st. st. value.

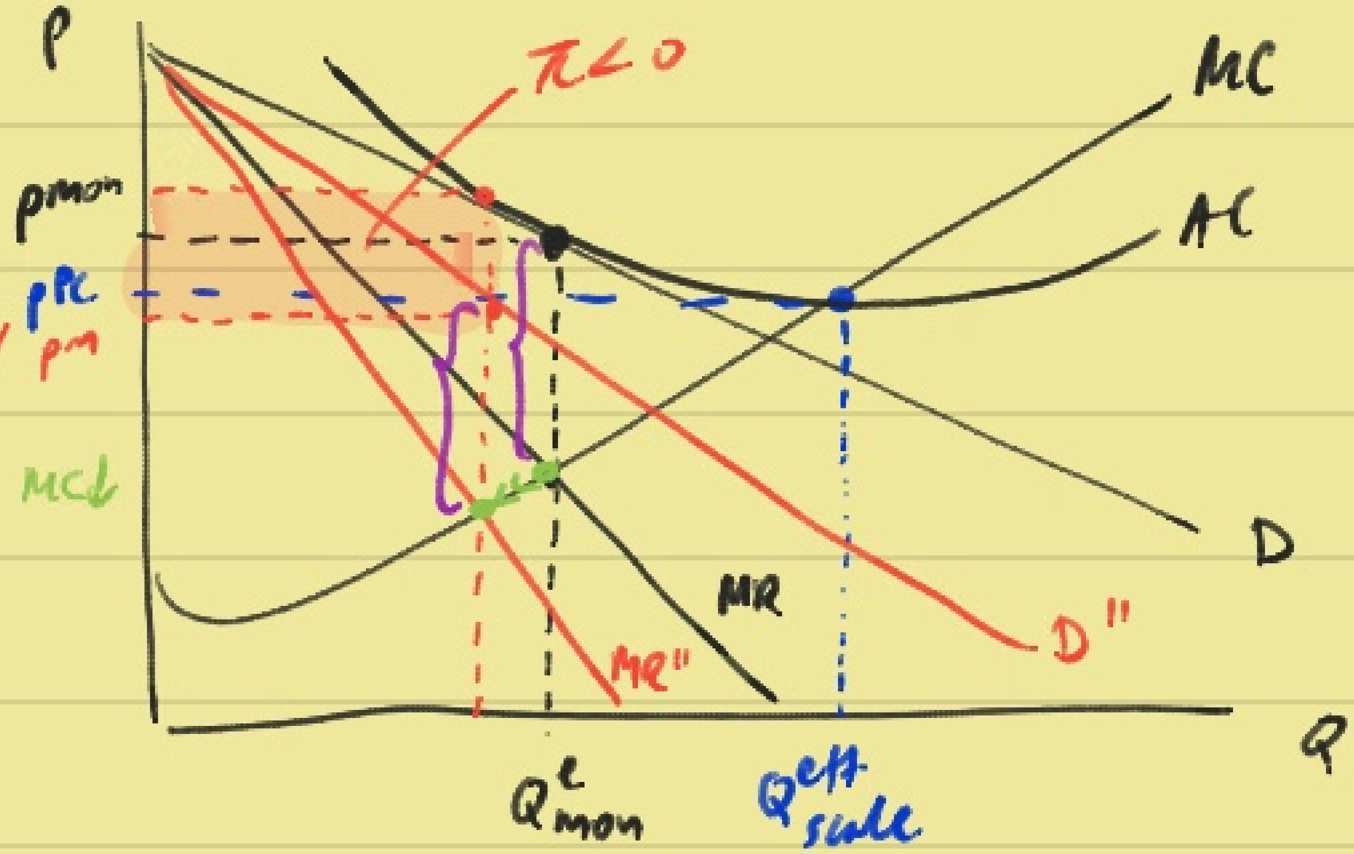
I think Susanto's logic is that there are idio movements in Y_{it}, i.e. demand: here: D↑ → MC↑ → μ↓



Isn't this a feature of any imperfect comp. model?

→ yes, but w/ flex prices, eqb is restored immediately, whereas sticky prices keep the adjustment from happening right away.

Let's do a ⊖ demand shock now for fun:



D↓ → MC↓ → μ↑

⇒ ok so we know now why sticky prices introduce endog. time-varying markups/mc!

Both Eric Sims & Collard notes indicate more or less explicitly that in Rotemberg:

$$P_{it} = P_{jt} = P_t$$

Maybe the point is that this is an eqb condition, but out of eqb, it doesn't hold b/c indiv. demands may be different, so the analysis of the previous pages carries through!

Ok so we've settled those 2 problems. So let's get back to the cost push shock.

We were kinda saying that 2 things can move mc:

- demand $\to$ moves "out-of-st-st" mc
- θ (el. of subst.) $\to$ moves st. st. mc

I think cost push refers to the latter based on Peter's def, and based on Clarida, Gali & Gertler's statement that it's "anything else than demand" that moves mc.

Clarida, Gali, Gertler (1999), Result 1:

with cost push shocks, there's a SR tradeoff
btwn inflation and output variability.
→ so I think the divine coincidence doesn't hold!

And look at this: Basu Sum 2, p. 57
Divine coincidence in DNK model:
NKPC: $\pi_t = \beta E_t \pi_{t+1} + \kappa \hat{x}_t$
if $\pi_t = 0$, $\Rightarrow \hat{x}_t = 0$!
Divine coincidence breaks down however if the NKPC
has a shock...
... which is exactly how Clarida, Gali & Gertler
define the cost push shock! It's

$$\pi_t = \beta E_t \pi_{t+1} + \kappa \hat{x}_t + u_t$$

and I betcha it comes from the El. of substi!
and you know what... look at the fucking NKPC
of CEMP:

$$\pi_t = \gamma \pi_{t-1} + (1-\gamma)\Gamma \bar{\pi}_t + \underbrace{\rho \varphi_{t-1} + \eta_t}_{\text{What's this?}}$$

↑ hybrid

expectation term

$\Rightarrow$ it's a shock in the NKPC!!

Specifically, $\eta_t = \dfrac{\xi_p}{1-\beta\rho} \varepsilon_t + \mu_t$

ε_t: mc-shock

$\mu_t = -\xi_p \dfrac{\hat{\psi}_t}{\bar{\psi}-1}$

where ψ_t = el. subst. of demand!

$\rightarrow$ η_t __is__ a cost-push shock! And this is what they mean by saying that their model doesn't distinguish between cost-push (μ_t) and mc-shocks (ε_t)!

So let's look at Clarida, Gali & Gertler in detail:

CB loss: $\max -\frac{1}{2} E_t \left\{ \sum_{i=0}^{\infty} \beta^i \left[\alpha x_{t+i}^2 + \pi_{t+i}^2 \right] \right\}$ (2.7)

CB's problem:

max (2.7) st. (2.1) (NKIS) and (2.2) (NKPC).

Result 1 referred to optimal mon. pol under discretion.

Result 2 as well:

"Optimal mon. pol is a kind of "LR inflation targeting" in which π converges to the target only over time. "Extreme inflation targeting", in which you reach the target immediately, is only optimal if

1) No cost-push shocks, or

2.) $\alpha = 0$ (weight on output gap)

Result 3 = Taylor-Principle

Result 4 The optimal policy has the int. rate move so as to:

1. offset demand shocks ($AD\uparrow \Rightarrow i_t \uparrow$)

$\Rightarrow$ b/c $i_t\uparrow$ pushes both $x_t\downarrow$ and $\pi_t\downarrow$

(no SR tradeoff here)

2. accomodate shocks to potential output ($\rightarrow \bar{i}_t$)

b/c $Y_t^n\uparrow$ but $Y_t\uparrow$ 1:1 (PIH) $\rightarrow x_t = 0$ still.

Result 5: If under discretion the CB wishes to have $Y_t > Y_t^n$, then we get higher inflation w/o a gain in output (inflationary bias)

A side note: in Basu Sum Part 2, p. 85:

"To get results qualitatively diff. from an RBC, we need suboptimal mon. pol. b/c the Taylor-rule is too good!" → maybe that can change under learning?

Ok so let's think thru a cost-push shock in the DNK model. Would it look like the $\hat{\mu}_t \downarrow$ -shock in Basu Sum 2, p. 7?

$$\pi_t = \beta E_t \pi_{t+1} + \kappa \hat{x}_t + u_t \qquad \text{(NKPC)}$$

$$\hat{x}_t = \hat{E}_t x_{t+1} - \frac{1}{\sigma}\left[i_t - E_t \pi_{t+1}\right] + \frac{1}{\sigma} r_t^n \qquad \text{(NKIC)}$$

$$\hat{i}_t = \delta_\pi \pi_t + \delta_x \hat{x}_t \qquad \text{(TR)}$$

$$\kappa = \frac{(\eta + \sigma)(1-\omega)(1-\beta\omega)}{\omega}$$

ω = Calvo param.

η = Frisch

σ = IES param in $U(\cdot)$ fct.

θ = El. of subst. in demand

$$\rightarrow \mu = \frac{\theta}{\theta - 1} \qquad \text{(I think } \theta > 1\text{)}$$

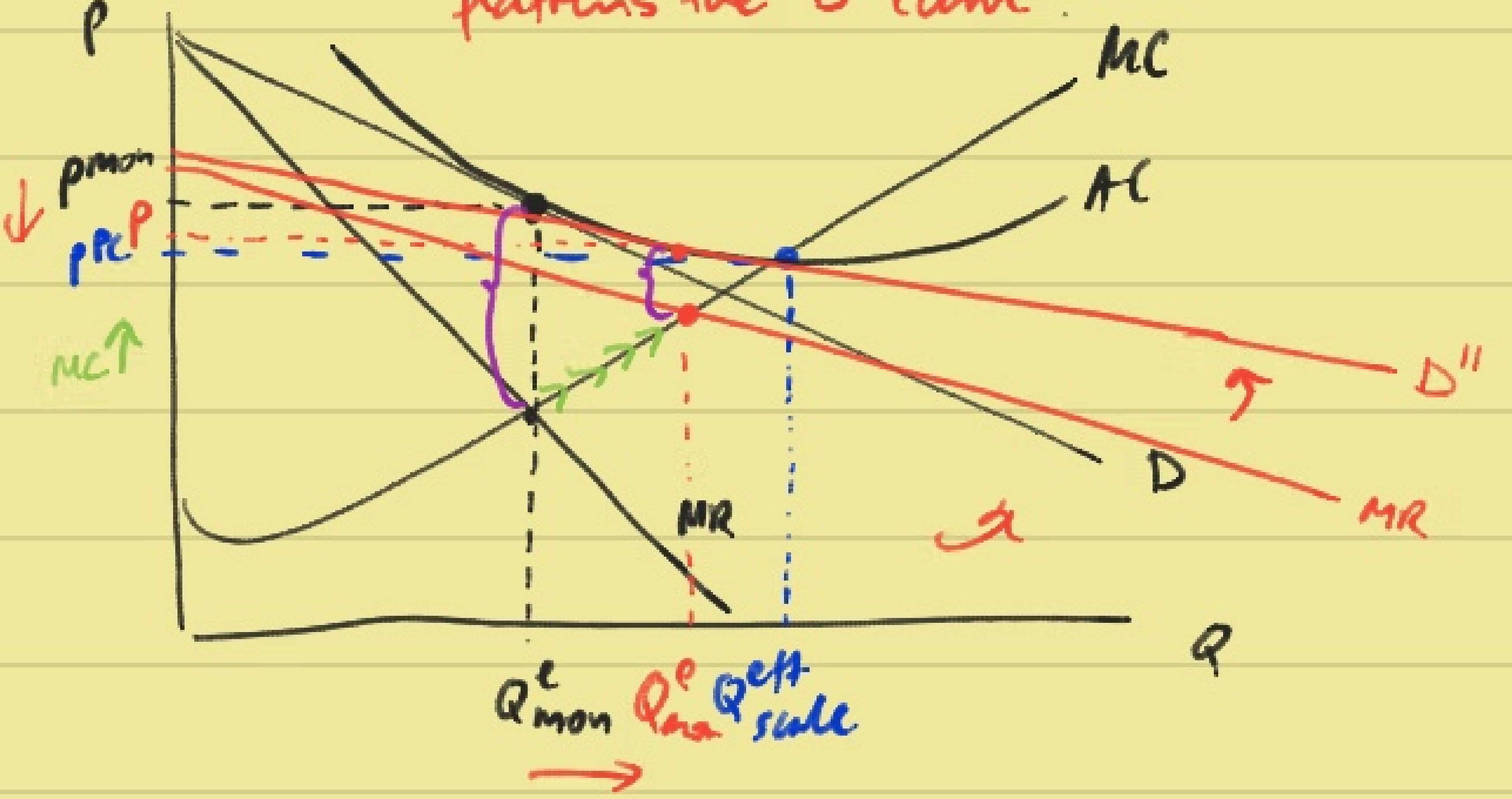
$$\frac{\partial \mu}{\partial \theta} = \underbrace{\frac{1}{\theta - 1}}_{>0} + (-1)\frac{\theta}{(\theta-1)^2} = \frac{\theta - 1 - \theta}{(\theta-1)^2} = -\frac{1}{(\theta-1)^2} < 0$$

So when $\theta \uparrow \rightarrow \mu \downarrow$

which makes sense btw. b/c as $\theta \to \infty$, the goods b/c perfect substitutes and so you lose mon. power.

In our micro principles class, a $\theta\uparrow / \mu\downarrow$ shock:

flattens the D-curve.



$\Rightarrow \mu\downarrow$

Now in the NK model:

$$\uparrow \pi_t = \beta E_t \pi_{t+1} + \kappa \hat{x}_t \downarrow + u_t \uparrow \qquad \text{(NKPC)}$$

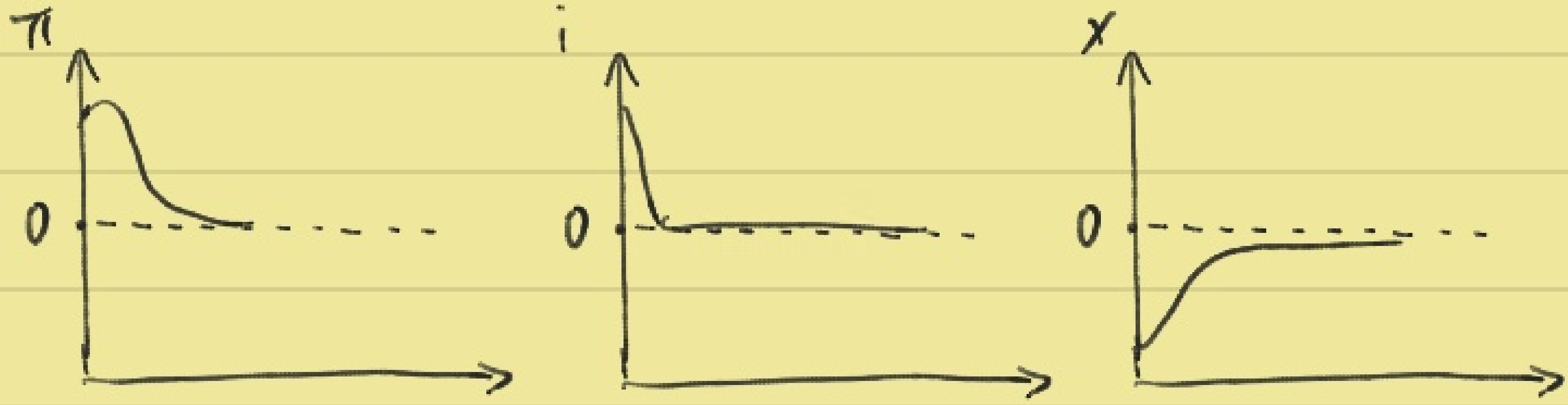
$$\downarrow \hat{x}_t = \hat{E}_t x_{t+1} - \frac{1}{\sigma}\left[i_t \uparrow - E_t \pi_{t+1}\right] + \frac{1}{\sigma} r_t^n \qquad \text{(NKIC)}$$

$$\uparrow \hat{i}_t = \delta_\pi \pi_t \uparrow + \delta_x \hat{x}_t \qquad \text{(TR)}$$

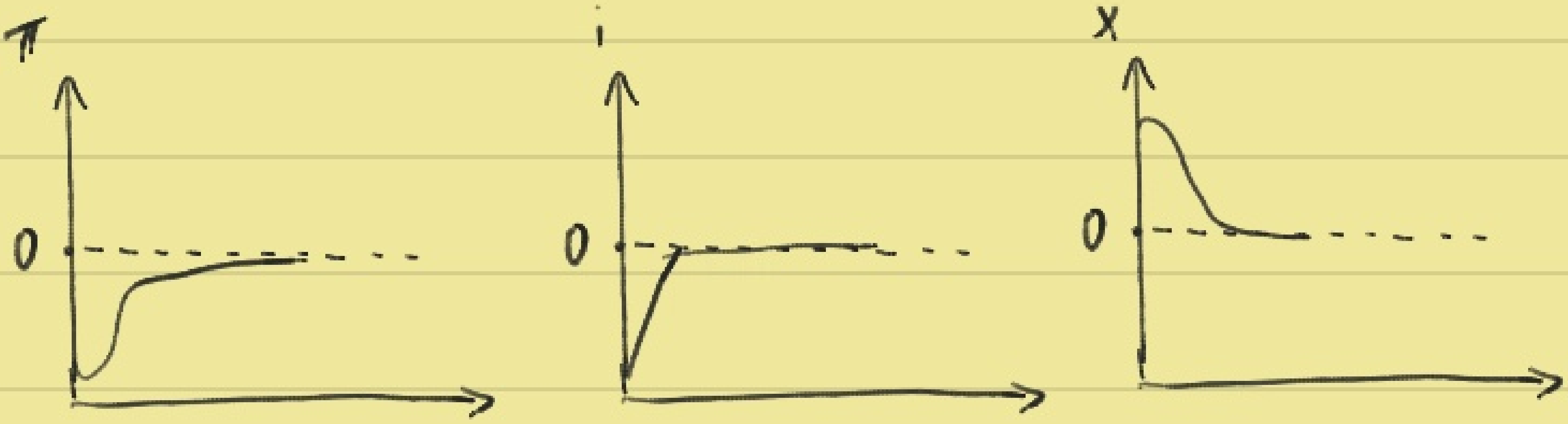
and stop.

(All shocks are one-time shocks here!)

So it would look like this in IRFs: $\theta\downarrow / \mu\uparrow$



$\theta\uparrow / \mu\downarrow$ (closer to perfect comp.)



It doesn't look like the μ-shock b/c here mon. pol. responds
But if there was no TR, we'd transition to a new eqb.
w/ lower prices and higher output, so in that sense
the θ/μ-shock does resemble the one on Basu
p.7 in that it resembles a tech shock.

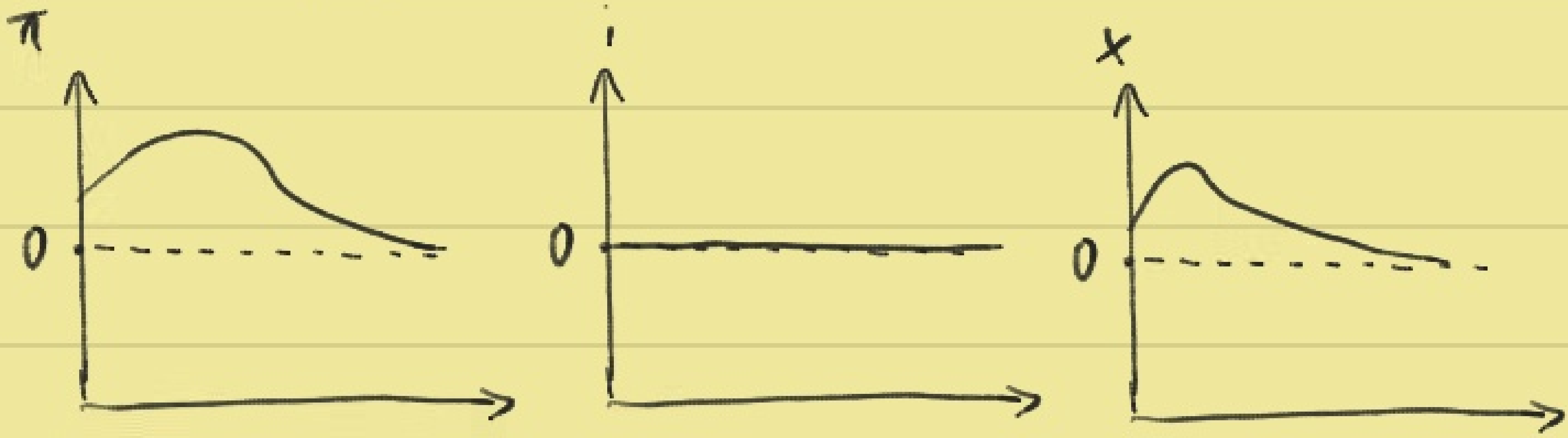
→ you can also see the SR nature of the (π, x)-tradeoff:

If mon. pol. doesn't respond:

$$\uparrow\uparrow \pi_t = \beta E_t \pi_{t+1}\uparrow + \kappa \hat{x}_t \uparrow + u_t \uparrow \qquad \text{(NKPC)}$$

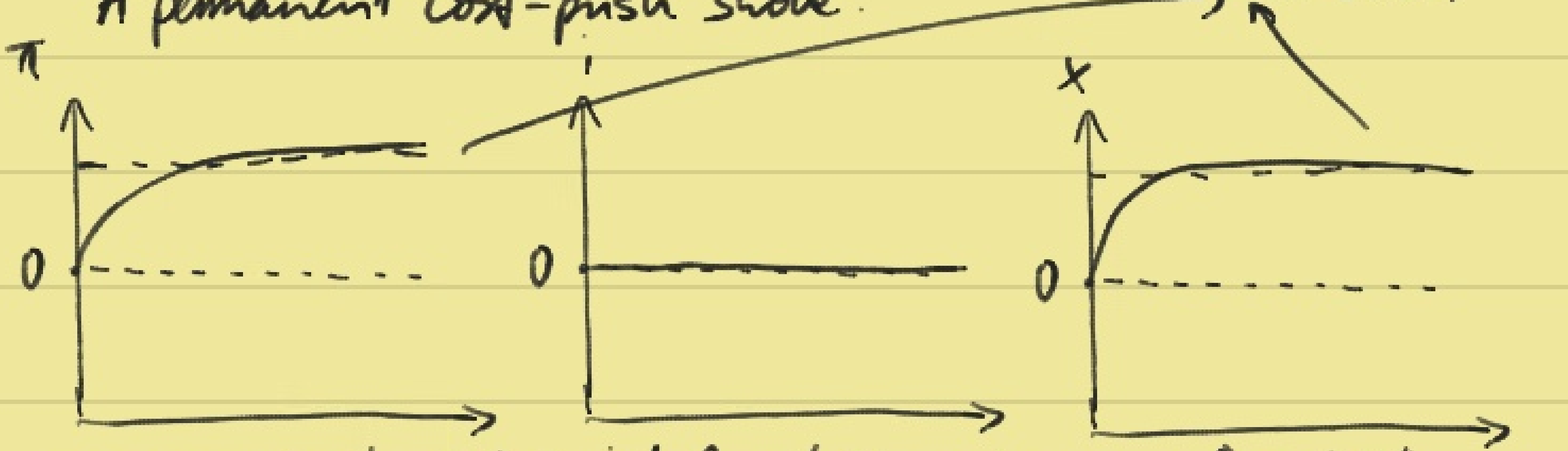
$$\uparrow \hat{x}_t = E_t \hat{x}_{t+1} - \frac{1}{\sigma}\left[i_t - E_t \pi_{t+1}\uparrow\right] + \frac{1}{\sigma} r_t^n \qquad \text{(NKIS)}$$

$$\hat{i}_t = \delta_\pi \pi_t + \delta_x \hat{x}_t \qquad \text{(TR)}$$



→ permanently higher price-level.

A permanent cost-push shock:



⇒ you don't want to interfere if the cost-push is "positive"

in the sense that it moves towards perfect competition.

However if it's a negative cost-push ($\theta\downarrow$/$\mu\uparrow$), the Fed can react

(and this is what Peter gets in his paper!)

the problem I'm now noticing is that here, I'm analyzing the SR ($\pi - x$)-tradeoff that happens when $\exists$ cost-push shocks in NK models, i.e. a failure of divine coincidence GIVEN that we want to close the output gap.

Ryan said however that we don't want to close the output gap in this case b/c the flex price is inefficient.

→ Eric Sims says that in the NK model $\exists$ 2 distortions

- SR distortion due to nominal frictions
- LR distortion due to imperfect comp.

"We ass. the CB is concerned w/ the SR one and that the LR one has been taken care of w/ some Pigouvian tax".

→ and I think that's the std. way of business:
also in Basu we simply wanna close the output gap.
Also in absence of cost-push shocks I argue that
y^{flex} is suboptimal (too low) in the NK model.
→ but you can't stabilize to it b/c the Phillips curve
breaks down: the neutrality of money happens.

Justiniano & Primiceri, National Bank of Beligium.
Slides of Presi, 22 March 2010 "presentatie_..._pdf"
__Efficient output__ := Y under perfect comp. (Y^{pc})
__Potential output__ := flex price Y under imperfect comp (Y^{p})
and constant markups
__Natural output__ := flex price Y under imperfect comp (Y^{n})
but time-varying markups
$Y^{pc} - Y^{p}$ → importance of nom. rigidities
$Y^{p} - Y^{n}$ → importance of exog. markup variation → cost-push!
($\pi - Y$) tradeoff for monpol!

If markup shocks (i.e. cost push) is interpreted as tech shocks:

$Y^P = Y^n$ (!)

Their prezi observes that while Y^P is very smooth (and close to actual observed Y), Y^n is crazy volatile, implying implausibly big markup shocks.

$\Rightarrow$ JP argue that much of this is meas. error, and show that a model w/ meas. error obtains $Y^P \approx Y^n$, and a much better fit to data.

(Since they consider wage markups, the idea of meas. error is plausible b/c wage series aren't great.)

If Y^P is smooth, it means that tech shocks aren't very volatile

If Y^n is volatile, it means that BCs are big $\Leftrightarrow$ markups fluctuate a lot. But IMO that doesn't need to come from cost-push exclusively: it can also come from demand!

My point is: mon pol cannot stabilize to Y^{PC} if there's imperfect comp. → If in addition there are time-varying markups, then Y^P is the only thing we can stabilize to. And to the extent that there are also nominal rigidities, we have time-varying markups.

What the output gap is depends on what we're stabilizing to:

E.g. if $X := Y^{PC} - Y_t$ then cost-push shocks don't affect Y^{PC} and we thus get a substantial output gap.

but if $X := Y^n - Y_t$ then since cost-push shocks affect Y^n, X doesn't open at all.

→ so we can really only stabilize to Y^P in a NK world.

Ok so I think I understand not too badly how the cost-push shocks work in the NK model

$\rightarrow$ since they move markups / mc, they open up the output gap

Peter's results in the Tech Shocks in NK models paper bear out that cost-push shocks behave this way, and he also shows that they've played an important role (especially in inflation) fluctuations (Var. decomp).

$\Rightarrow$ So the task is: how does the model react differently under learning? CEMP:

$$\left.\begin{aligned} k_t &= f_k \\ \bar{\pi}_t &= f_{\bar{\pi}} + f_k^{-1} \eta_{t-1} \end{aligned}\right\} \text{learning block}$$

Cost-push

$$\xi_t = f_\xi + A_\xi \xi_{t-1} + S_\xi \begin{pmatrix} \varepsilon_t \\ \mu_t \end{pmatrix} \left\} \begin{array}{l} \text{exog sum of shocks } \eta_t \\ \text{exog shock: } y_t \\ \text{NKPC: } \pi_t \end{array} \right.$$

Right now there's no mon. pol (but effect of mon pol is on Γ in AS)

I want to approach the CEMP-version   30 Aug 2019

in three ways

1.) Intuition   2) w/o mon. pol   3.) w/ mon. pol in the model

1) Intuition: there's a $\theta\downarrow / \mu\uparrow$ (less comp) cost-push shock

$$\pi_t = \beta E_t \pi_{t+1} + \kappa \hat{x}_t + u_t \qquad \text{(NKPC)}$$

$$\hat{x}_t = \hat{E}_t x_{t+1} - \frac{1}{\sigma}\left[i_t - E_t \pi_{t+1}\right] + \frac{1}{\sigma} r_t^n \qquad \text{(NKIC)}$$

$$\hat{i}_t = \phi_\pi \pi_t + \phi_x \hat{x}_t \qquad \text{(TR)}$$

Note by the way that $\hat{x}_t > 0$ here b/c Y_t^n moved down, so it's actually not clear that we wanna close this gap!

But supp CB ignores that, & just closes.

Time-out: I think I haven't yet understood the tradeoff well:

CGG: discretion: $\max \; -\frac{1}{2}\left[\alpha x_t^2 + \pi_t^2\right] + F_t$

s.t. $\pi_t = \lambda x_t + f_t$

$$\Rightarrow \max_x \; -\frac{1}{2}\left[\alpha x_t^2 + (\lambda x_t + f_t)^2\right] + F_t$$

$$\Rightarrow \max_x \; -\frac{1}{2}\left[\alpha x_t^2 + \lambda^2 x_t^2 + 2\lambda f_t x_t + f_t^2\right] + F_t$$

FOC: $-\frac{1}{2}\left[2\alpha x_t + 2\lambda^2 x_t + 2\lambda f_t\right] = 0$

$$(\alpha+\lambda^2)x_t \overset{!}{=} -\lambda f_t$$

$$x_t = \frac{-\lambda}{\alpha+\lambda^2} f_t$$

More comp shock

$$\Rightarrow \quad \overset{\uparrow}{x_t} = -\frac{\lambda}{\alpha+\lambda^2}\left(\beta \overset{\downarrow}{E_t\pi_{t+1}} + \overset{\downarrow}{u_t}\right)$$

$$\Rightarrow \quad \pi_t = \lambda x_t + f_t = -\frac{\lambda^2}{\alpha+\lambda^2} f_t + f_t = \left(1-\frac{\lambda^2}{\alpha+\lambda^2}\right) f_t$$

$$\pi_t = \frac{\alpha}{\alpha+\lambda^2} f_t$$

$$\rightarrow \quad \overset{\downarrow}{\pi_t} = \frac{\alpha}{\alpha+\lambda^2}\left(\beta \overset{\downarrow}{E_t\pi_{t+1}} + \overset{\downarrow}{u_t}\right)$$

$\Rightarrow$ a cost-push shock looks like a supply shock

- x & π move in opposite directions following a cost-push shock: if $u_t\downarrow \rightarrow x_t\uparrow$ & $\pi_t\downarrow$; if $u_t\uparrow \rightarrow x_t\downarrow$ & $\pi_t\uparrow$

$\Rightarrow$ stabilizing π requires: $i_t\downarrow \Rightarrow x_t\uparrow\uparrow$ and $i_t\uparrow \Rightarrow x_t\downarrow\downarrow$

IF expectations don't move: a tradeoff.

- Note: that $E(\cdot)$ moving makes this worse.

- In the LR I guess the tradeoff dies out as $E_t\pi_{t+1} \rightarrow 0$.

→ intuition: a mon. pol that can stop $E(\pi)$ from moving needs to use "less ammunition" to fight the cost-push shock than one that doesn't manage expectations.

Problem: how to manage $E(\pi)$?

- bigger cuts in i_t today → but then you're back to higher costs, except you pay them today instead of tomorrow (→ intertemporal tradeoff)
  - → this still is preferable though if $var(\pi)$ is costly per se!
- abandon TR: use communication to keep $E(\pi)$ "anchored"

⇒ if you have a "credibility stock" (low gain) you don't have to resort to any of these measures as long as you don't allow π to deviate from its st. st. value by too much or for too long.

It would just be nice to see the tradeoff w/commitment too, that is, simply in the 3-eq DNK:

$$\downarrow\downarrow \pi_t = \beta E_t \pi_{t+1}\downarrow + \kappa \hat{x}_t + u_t \downarrow \qquad \text{(NKPC)}$$

$$\uparrow\downarrow \hat{x}_t = \hat{E}_t x_{t+1} - \frac{1}{\sigma}\left[i_t \downarrow - E_t \pi_{t+1} \downarrow\right] + \frac{1}{\sigma} r_t^n \downarrow \qquad \text{(NKIS)}$$

$$\downarrow \hat{i}_t = \delta_\pi \pi_t \downarrow + \delta_x \hat{x}_t \qquad \text{(TR)}$$

Maybe the natural rate is affected? $r_t^n = \sigma E_t\left[y_{t+1}^f - y_t^f\right]$
$\downarrow$ $\uparrow$

b/c closer to P.C.

grrr....

The only thing that's clear is that policy $i_t \downarrow \to x_t \uparrow$ & $\pi_t \uparrow$

But we should have $\hat{x}_t \uparrow$ as a response to the shock.

Ok, let's just supp. that a TR is in place

- then, by virtue of propagating through the system of the economy (and only then) does a cost-push shock look like a supply shock

- Movement in EC) aggravates the problem and the tradeoff.

2) Now to CEMP w/o mon. pol:

$$\left.\begin{aligned} k_t &= f_k \\ \bar{\pi}_t &= f_{\bar{\pi}} + f_k^{-1}\, \eta_{t-1} \end{aligned}\right\} \text{learning block}$$

Cost-push

$$\xi_t = f_\xi + A_\xi \xi_{t-1} + S_\xi \begin{pmatrix} \varepsilon_t \\ \mu_t \end{pmatrix} \left\}\begin{array}{l} \text{exog sum of shocks } \eta_t \\ \text{exog shock: } y_t \\ \text{NKPC: } \pi_t \end{array}\right.$$

Right now there's no mon. pol (but effect of mon pol is on Γ in AS)

Supp $\mu_t \downarrow$ ⇒ closer to P.C. cost-push shock

$\Rightarrow \eta_t \downarrow$ → $\pi_t(\eta_t) \downarrow$

→ 1.) anchored ($f_k^{-1} \to 0$): $E(\pi)$ don't move and that's it.

2) unanchored: $\bar{\pi}_{t+1} \downarrow$ ⇒ again,

the same intuition is straightforward to see

⇒ $E(\cdot)$ moving makes the stabilization costlier.

3) CEMP w/ mon. pol. "appended"

$$\left.\begin{aligned} k_t &= f_k \\ \bar{\pi}_t &= f_{\bar{\pi}} + f_k^{-1}\, \eta_{t-1} \end{aligned}\right\} \text{ learning block}$$

Cost-push

$$\xi_t = f_\xi + A_\xi \xi_{t-1} + S_\xi \begin{pmatrix} \varepsilon_t \\ \mu_t \end{pmatrix} \quad \left\} \begin{aligned} &\text{exog sum of shocks } \eta_t \\ &\text{exog shock: } y_t \\ &\text{NKPC: } \pi_t \end{aligned}\right.$$

$$\left.\begin{aligned} &\downarrow \pi_t = \text{stuff} + \uparrow \underbrace{\text{demand}}_{\text{new term} \to x_t} \\ &+ \uparrow x_t = \text{stuff} - (i_t - E_t \pi_{t+1}) \\ &+ \text{TR: } \downarrow i_t = \delta_\pi \pi_t + \delta_x x_t \end{aligned}\right\} \text{mon pol appended}$$

Supp $\mu_t \downarrow$ ⇒ closer to P.C. cost-push shock.

→ $\eta_t \downarrow$ → $\pi_t \downarrow$

→ $i_t \downarrow$ → $x_t \uparrow$ (mechanisms same as when mon. pol. isn't explicitly specified)

→ Movement in $\bar{\pi}$ amplifies $\pi_t \downarrow$ & $x_t \uparrow$

⇒ again in such a case a higher int. rate drop is necessary, worsening the tradeoff