NEXT   <u>26 Aug 2019</u>

See Experimentation Notes 21 August 2019 Ryan Meeting

<u>Benhabib, Schmitt-Grohe & Uribe (1999)</u>
- challenges the conventional notion that active pol.
  (mon. pol that responds more than 1:1 to π) is
  stabilizing $\longrightarrow$ it is only stable (unique) in a "very"
  local neighborhood of the st. s.
$\longrightarrow$ π fluctuates around its "stable" value for a while
  before converging to the passive st. st.
  $\hookrightarrow$ makes emerging liq. traps hard to detect!
Needs ZLB.

$\hookrightarrow$ 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 increases 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 (1993):

    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 <u>Svensson</u> is essentially saying that the "LR-PC becomes non-vertical when infl. E(·) are anchored" i.e. he's saying that the $u-\pi$ tradeoff survives into the LR then

→ money non-neutrality in the LR.

CEMP-view of π-development:

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

$$\uparrow_{MC} \quad \uparrow_{mon.pol}$$

Svensson is saying

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

$$\uparrow_{MP \text{ didn't do enough!}}$$

$\Longrightarrow$ 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)

$\Longrightarrow$ self-referentiality.

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

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

→ the "Svensson scenario" doesn't describe current

US manpol well, though:

• $\pi < \bar{\pi}$ while $u < u^n$

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

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

④↑ ↑⑤   ③↑   ②↑

vs

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

④↑   ②↑

→ π doesn't increase as much while labor

market effects are huge!

But this is where the "where are they anchored?"

comes in:

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

<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 upp. class's
expectations overshoot less than those of HHs.

But ok, at least I can rationalize Svensson's story
— and maybe fast 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

- <u>ZLB</u>

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

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

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

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

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

what if they get anchored 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
$\pi - \exp(\cdot) \Rightarrow \pi$ vs. output in the SR !

Peter meeting   27 Aug. 2015

· diss - fills.   & 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 (1995)
   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 $(\bar{\epsilon}$ or $\epsilon^n)$
·   — 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 later (estimate using the gain-result from CEMP
so you don't have to estimate the beast)

Erceg & Levin JME (2003)
   Volcker - disinflation not explannable 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 there (Powell & John Williams & Clarida)
 "we don't wanna get into low $E(\ )$"
 "better act now then 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

Erceg & 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 b/w mon. objective and credibility → this can explain US mon history as well as the recent int. rate cut (July 2015) 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.

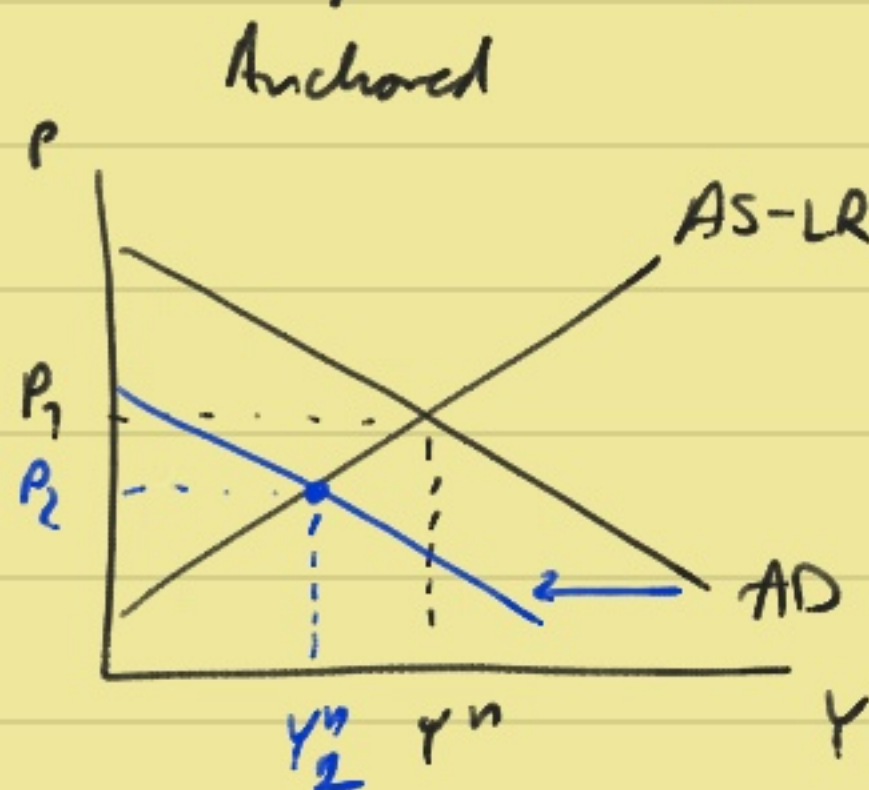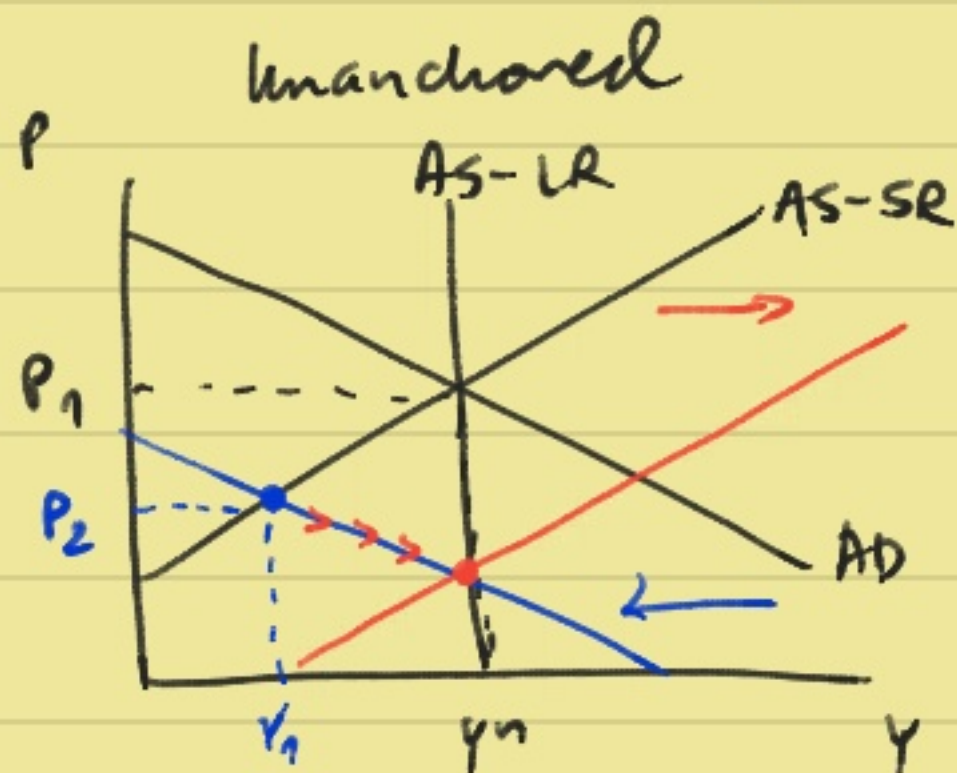Let's return to the question   26 Aug. 2019

of when monpol is powerful under learning:

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

anchored: can't move expectations

need bigger shocks to move π

(can be a blessing & a curse)

unanchored

Anchored

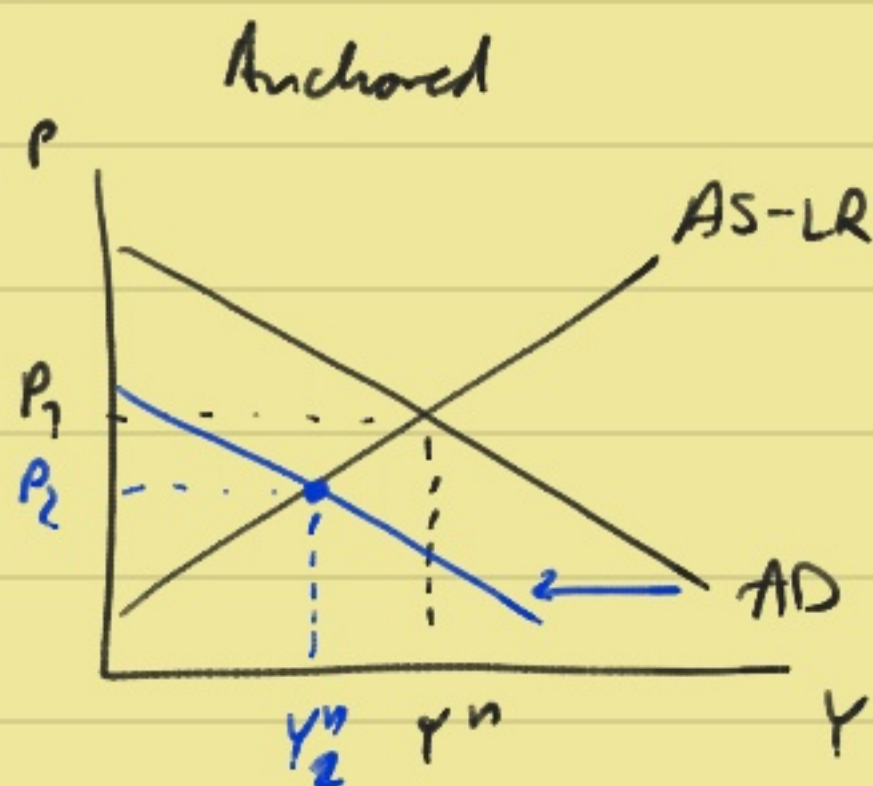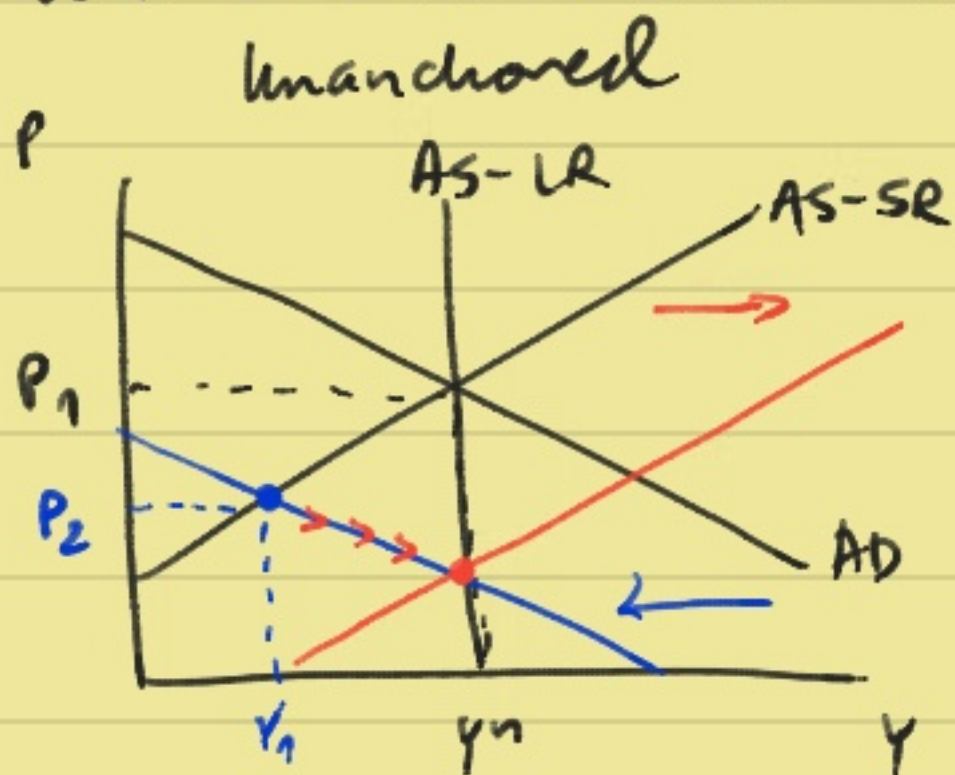$P_2 < E(P) \Rightarrow E(P)\downarrow$

$\Rightarrow$ AS-SR shifts R

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

$\Rightarrow$ but then they deanchor!

$\Rightarrow$ so anchoring makes MP powerful 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:
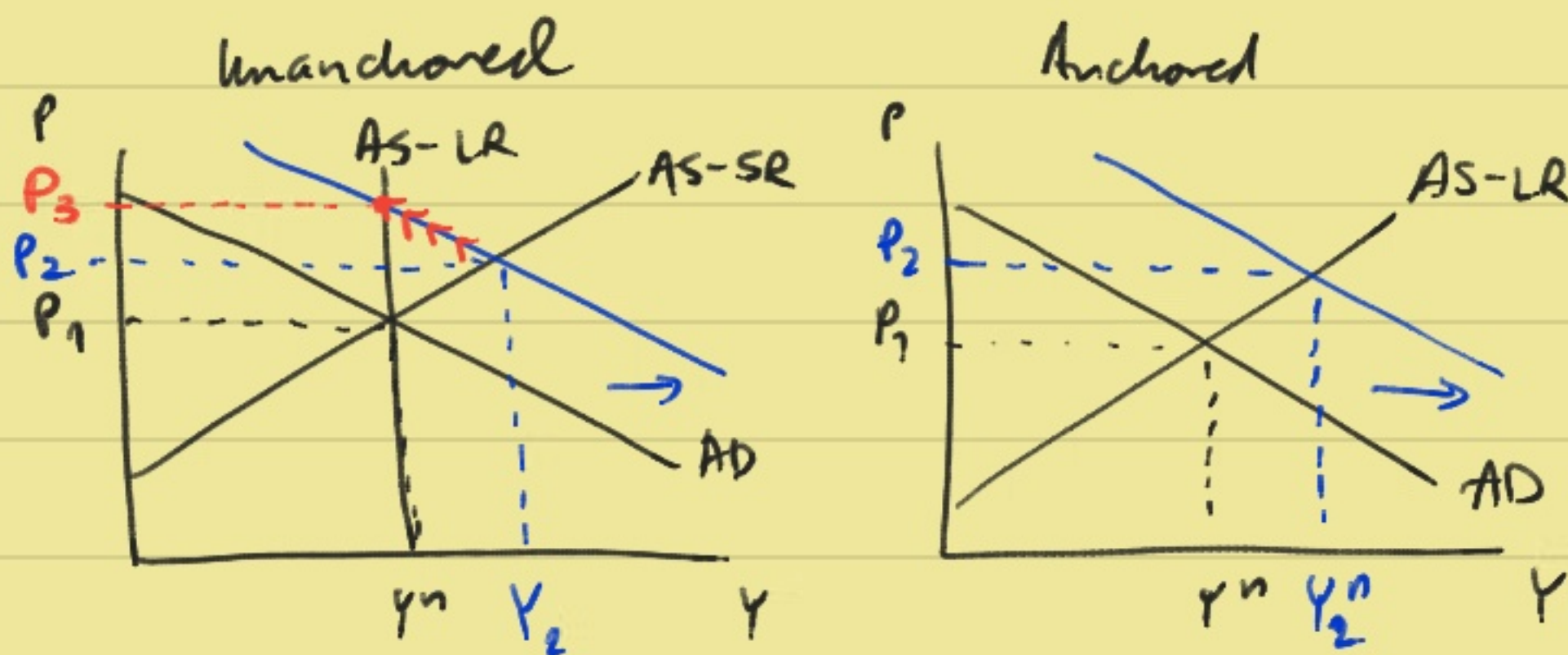
unanchored

Anchored

→ 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

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

• Benhabib   global ⇒ diff.

⇕ opposition

• Davug & Leeper — related though to Evans & H (2003)

A synthesis: investigate the tradeoff between 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 COMP that
SR-Exp. don't move!

The actual meeting:

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.