Monetary Policy Shocks: What Have We Learned and to What End?

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Handbook of Macroeconomics
Idea: Assessing monetary policy cannot be done through statistical experimentation due to the Lucas critique.

What does the Lucas critique say?

Question: Hence, we have to use a structural theoretical model to evaluate monetary policy. How do we choose which model?
R.E. Lucas (1980)

… need to test them [models] as useful imitation of reality by subjecting them to shocks for which we are fairly certain how actual economies or part of economies would react. The more dimensions on which the model mimics the answers actual economies give to simple questions, the more we trust its answers to harder questions.

How to carry this program in practice?
1. one isolates monetary policy shocks in actual economies and characterizes the nature of corresponding monetary experiments (identified VAR).
2. characterize the response of the actual economy to these monetary experiments (impulse responses).
3. Perform the same experiments in the model economies to be evaluated and “compare” with the outcomes of actual economies.
General identification strategies:

1. **Short-run identification:** Cholesky, recursiveness assumption.

2. **Event studies:** a la Romer and Romer (1989) – however, the Fed’s behavior is always endogenous (Hoover and Perez, 1994a, b).


4. **Demiralp and Hoover (2003):** Graph analysis
Overview of the results:

Although there is no agreement on the correct way of identifying exogenous shocks, alternative identification assumptions seem to deliver very similar conclusions:

<table>
<thead>
<tr>
<th></th>
<th>EM</th>
<th>P</th>
<th>PCOM</th>
<th>FF</th>
<th>NBRX</th>
<th>DM2</th>
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<tr>
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<td>-0.04</td>
<td>0.11</td>
<td>0.11</td>
<td>-0.06</td>
<td>0.06</td>
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<tr>
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<td>-0.04</td>
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<tr>
<td>PCOM</td>
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<td>0.11</td>
<td>1.00</td>
<td>0.21</td>
<td>-0.14</td>
<td>0.02</td>
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<tr>
<td>FF</td>
<td>0.11</td>
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<td>0.21</td>
<td>1.00</td>
<td>-0.52</td>
<td>-0.16</td>
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<td>-0.14</td>
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In response to a contractionary monetary policy shock:

- short-term interest rates rise
- output, employment, profits, and money aggregates decline

Response to Cholesky One S.D. Innovations ± 2 S.E.
- prices respond very slowly

- wages fall but little
Monetary policy shocks account for a modest portion of output and price volatility.

<table>
<thead>
<tr>
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<th>% FEV of EM</th>
<th>% FEV of P</th>
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<tr>
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<td>0.06</td>
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<tr>
<td>8</td>
<td>3</td>
<td>0.03</td>
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<tr>
<td>12</td>
<td>9</td>
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<tr>
<td>16</td>
<td>15</td>
<td>0.8</td>
</tr>
<tr>
<td>20</td>
<td>20</td>
<td>1.3</td>
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</table>
Economic interpretations of “exogenous” monetary policy shocks

1. As a shock to the preferences of the monetary authority
2. As a way for the Fed to accommodate private sector expectations
3. Measurement error in the preliminary data available to the FOMC
Remarks

- Impulse responses are elasticities or “slopes” estimated from the data – residuals should not be interpreted.

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<th>Variable</th>
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<th>Std. Error</th>
<th>t-Statistic</th>
<th>Prob.</th>
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<td>0.24</td>
<td>5.7</td>
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<td>X</td>
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<td>0.22</td>
<td>8.5</td>
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Puzzles:

1. **Price Puzzle**: prices rise in response to a positive shock to FF

2. **Liquidity Puzzle**: FF increases in response to an increase in narrow money.

3. **UIP Puzzle**: positive shocks to FF result in persistent deviations in UIP (in favor of the US).
“We conclude this subsection by noting that even if monetary policy shocks have played only a very small role in business cycle fluctuations, it does not follow that the *systematic* component of monetary policy has played a small role”
**Imposing non-recursive assumptions on a VAR**

Consider a system with \( \{y_t, p_t, i_t\} \) where we achieve identification by assuming:

\[
C_0 = \begin{bmatrix}
* & 0 & 0 \\
0 & * & 0 \\
* & * & * \\
\end{bmatrix}
\]

Notice that the system is over-identified: there is an extra 0 restriction. Hence, this is testable.
Estimating the model that imposes the extra restriction:

\[ y_t = \beta_{yy} y_{t-1} + \beta_{yp} p_{t-1} + \beta_{yi} i_{t-1} + \varepsilon_{yt} \]

\[ p_t = \beta_{py} y_{t-1} + \beta_{pp} p_{t-1} + \beta_{pi} i_{t-1} + \varepsilon_{pt} \]

\[ i_t = \alpha_{iy} y_t + \alpha_{ip} p_t + \beta_{iy} y_{t-1} + \beta_{ip} p_{t-1} + \beta_{ii} i_{t-1} + \varepsilon_{it} \]

The likelihood from this model and the likelihood from the reduced form VAR can then be compared using a LR test. However, rejection of the test does not inform us about which zero coefficient restriction is incorrect.
Narrative Approach – Treatment Effects Literature

If he was less annoying in regard to his classmates, he was more so in his classrooms. He had learned from Gottlieb the trick of using the word "control" in reference to the person or animal or chemical left untreated during an experiment, as a standard for comparison: and there is no trick more infuriating. When a physician boasted of his success with this drug or that electric cabinet, Gottlieb always snorted, "Where was your control? How many cases did you have under identical conditions, and how many of them did not get the treatment?" Now Martin began to mouth it -- control, control, control, where's your control? – till most of his fellows and a few of his instructors desired to lynch him.

*Arrowsmith* (1925) by Sinclair Lewis, first American to win the Nobel Prize for literature.
Definitions:

\( y_t \equiv \text{outcome variable} \)

\( X_t = (Z_t, W_t) \equiv \text{observable variables} ; \ W_t \equiv \text{instruments} \)

\( U_t \equiv \text{unobservable variables} \)

\( D_t \equiv \text{binary indicator of treatment (policy intervention)} \)

\( y_{0t} \equiv \text{outcome variable under non-treatment} \)

\( y_{1t} \equiv \text{outcome variable under treatment} \)

\( p[D_t = 1 | X_t] \equiv p(X_t) \equiv \text{propensity score}, \) the probability of treatment given the observables.
Remark: we observe *treatment on the treated, non-treatment on the untreated* but we do not observe *non-treatment on the treated* and *treatment on the untreated*. In other words, we do not directly observe $y_{1t} - y_{0t}$

Common measures of treatment effects:

- **Average Treatment Effect (ATE):** $E(y_{1t} - y_{0t})$
- **Average Treatment Effect on the Treated (ATET):**
  \[ E(y_{1t} - y_{0t} \mid D_t = 1) \]

Remark: most of the work usually consists in estimating the counterfactual $E(y_{0t})$ or $E(y_{0t} \mid D_t = 1)$
An important assumption: “selection on observables,” “ignorability” or “unconfoundedness”: conditional on the observables, the outcomes are independent of the treatment (condition 1 in the paper).

Remark: if there is selection on unobservables then treatment participation is endogenous and we usually require some type of matching estimator to re-randomize treatment participation. It does not seem that the paper permits this scenario.
A causal map:
Conditional Independence Given Predictive Variables for Unobservable Causes, White (2005):

The instruments $W_t$ are predictors (but not causes) of the unobserved variables $U_t$ so that $U_t \perp D_t \mid X_t = (Z_t, W_t)$, i.e., $D_t$ does not Granger-cause $U_t$ given $X_t$.

Remark: this is not your usual instrumental variable! In linear regression, we instrument the regressors to avoid correlation with the residuals. Here we instrument the residuals.
An Example: Dummy Variable Regression

Notice: $y_t = D_t y_{1t} + (1 - D_t) y_{0t}$. Intuition would suggest that to measure the effect of the policy intervention we could estimate:

$$y_t = X_t' \beta + D_t \alpha + \varepsilon_t$$

Why is $\hat{\alpha}_{LS}$ usually a biased estimate of ATET?

the density of the unobservables given the observables is not the same for the treated and the untreated, i.e., there is a selection effect.
Essence of the Angrist-Kuersteiner test:

If the treatment has no effect, then outcomes and treatments should be independent. This results in equation (5):

\[
P(y_t \leq y, D_t = 1 | z_t) - P(y_t \leq y | z_t)p(z_t) = E[1(y_t \leq y)(D_t - p(z_t)) | z_t] = 0
\]

so the test is a sort of test of *uncorrelatedness* between policy innovations and outcomes.
Suggestions:

- Selection on unobservables seems a real problem in general: Is condition 1 a reasonable assumption in practice?
- The elephant in the room: researchers actually care about the treatment effect
- David Dickey: “the power of a test is the power of the test times the probability that it will be used.”
- Application: why not use futures data on the fed funds rate target to obtain direct measures on policy innovations? (see Kuttner, 2001; Demiralp and Jordà, 2004; Cochrane and Piazzesi, 2005)
Conclusions

- What happens after a monetary policy shock? – the answer to this question is used to evaluate competing macro models, NOT to measure the effects of monetary policy (since it is difficult to grapple with the effects of systematic monetary policy).

- We will see statistical methods to measure when the responses from a macro model and from the data are
close in the statistical sense – Minimum Distance Methods.

- Once a macro model is validated by the data (by comparing impulse responses) then we can do controlled experimentation in the model (not in the data).