**Referee report**
**'Indirect Inference: a Local Projection approach'**
**by J. Castellanos and R. Cooper**

The paper proposes a new approach to estimate the parameters of a DSGE model. The procedure is based on indirect inference using local projections. The estimate of a set of structural parameters of the DSGE is obtained by minimizing the distance between the local projection impulse response functions (the coefficient associated to the shock in the local projection equation) obtained with the data and the same response obtained using artificial data generated from the DSGE model. Some Monte Carlo simulations are used to assess the performance of the procedure.

## Comments

1. In order for the procedure to work, a shock has to be available to the econometrician. If a proxy of the shock is not directly available, then the shock has to be estimated, as it is done in the empirical applications of subsections 5.1 and 5.2. From my point of view, estimating the shock opens the door to a series of potential problems.

   (a) In section 5.1 and 5.2 the shock is estimated using a structural VAR model. It is well known that the results, both in terms of IRFs and estimated shocks, in many cases, heavily depend on several features of the VAR specification, like the variables included in the model, the identifying restrictions, etc. Precisely, technology shocks and government spending shocks are two notable examples: depending on the VAR specification, technology can increase (Christiano et al., 2003) or reduce (Gali 1999) hours; government spending shocks, depending on the identification restrictions, can increase (Blanchard and Perotti, 2003) or reduce (Ramey, 2011) consumption. This implies that the structural parameter estimation becomes identification-sensitive: different ways of identifying the shock or different VAR specifications, can, in principle, deliver different estimates. I understand this issue is not specific to this approach but arises even in other available methods, like standard IRF matching estimators.

My recommendation is to extend the paper investigating and discussing this issue explicitly. First of all, under the null that the DSGE is the true DGP, there are at least two conditions that the initial shock estimation has to fulfill. First, the variables used in the VAR are enough informative to estimate the shock of interest, and, second, the empirical identifying restrictions are consistent with those of the DSGE. The latter is relatively easy to deal with. The former rquires that the shock is partially invertible for the VAR variables and this can be checked using the measure discussed in Forni et al. (2019).

(b) When the shock has to be estimated in an initial VAR I also see a sort of logical issue. In order for the LP to work, an implicit requirement is that the shock has to be well estimated in this first step (first VAR). This implies that also the IRF in this first step have to be well estimated. Thus, why is the LP step needed? The correct responses should be those already obtained from the first VAR (of course augmented with the variables of interest). In addition, the LP step should deliver less efficient estimates because of a generated regressor problem.

(c) Related to the previous points, it would be interesting to understand the performance of the approach using simulations where the shock is not available but is estimated from an initial SVAR.

In my view, the procedure becomes much more sensible and reliable when a proxy of the shock is available, as in the SVAR-IV literature, since the initial step of identifying the shock using a SVAR is no longer needed. This I think it should be stressed. I would recommend you to explore this dimension further maybe using other existing instruments (see comment 3 below).

2. This is a paper on structural DSGE parameter estimation. While there is a long comparison with the internal instrument approach (the VAR of section 3.2), I do not see any discussion about the pros and cons of the method proposed here versus other available methods like the full information Bayesian estimation or the IRFs matching estimator. I think the paper would substantially benefit from a comparison of the performance of your method with other standard approaches. It would be nice to see a case where the proposed method outperforms the existing ones.

3. I think the part on monetary policy shocks should be slightly modified. The choice of the Romer and Romer shock can be problematic since, as shown in Mirranda-Agrippino and Ricco (2021), the shock delivers very unstable results. I would experiment using the Mirranda-Agrippino and Ricco (2021) variable, which is available on their website and is a much more reliable proxy of the monetary policy shock. Also, I do not fully understand the goal of using a threshold LP for actual data and a linear LP for artificial data. What is the exact interpretation of the estimated parameter in this case? Why not focusing on a simple linear framework? If you think that having a state dependent effect is interesting, then I guess it would be interesting also for the government spending shock.

4. Identification of the technology shock in section 5.1 should be better explained. What is the VAR specification? What are the identifying restrictions?v Also why not using the standard approach proposed by Gali (1999)?

## Minor Comments

1. Equation (2) refers to a general model, but you finally restrict the attention to linear solutions. You should clarify this.

2. In equation (3) $\xi_{h,t}$ is not defined.

3. $w_t$ should be a column vector, unlike defined on page 9, line 13.

4. Page 10, line 10: $C(L)$ is infinite order and not $p$-th order: $\sum_{\ell=0}^{\infty} C_\ell L^\ell = C(L) = A(L)^{-1}$

## References


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Gali, J. (1999), Technology, Employment, and the Business Cycle: Do Technology Shocks Explain Aggregate Fluctuations? American Economic Review, 89(1), 249-271.

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