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FEVD: Just IV or Just Mistaken?

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Fixed effects vector decomposition (FEVD) is simply an instrumental variables (IV) estimator with a particular choice of instruments and a special case of the well-known Hausman-Taylor IV procedure. Plümper and Troeger (PT) now acknowledge this point and disown the three-stage procedure that previously defined FEVD. Their old recipe for SEs, which has regrettably been used in dozens of published research papers, produces dramatic overconfidence in the estimates. Again PT concede the point and now adopt the standard IV formula for SEs. Knowing that FEVD is an application of IV also has the benefit of focusing attention on the choice of instruments. Now it seems PT claim that the FEVD instruments are always the best choice, on the grounds that one cannot know whether any potential instrument is correlated with the unit effect. One could just as readily make the same specious claim about other estimators, such as ordinary least squares, and support it with similar Monte Carlo assumptions and evidence.

1 Introduction

Thomas Plümper and Vera Troeger in this issue attempt to defend the fixed effects vector decomposition (FEVD) estimator as introduced in *Political Analysis* in 2007. This statistical procedure for models with both time series and cross-section dimensions has proved popular with researchers in many fields where such data structures arise. The motivation for FEVD was mostly heuristic, with evidence from Monte Carlo experiments in which FEVD appeared to display better mean-squared error properties than other estimators. The two critiques of FEVD in this issue, one by William Greene (2011) and the other by us, instead provide formal analyses of the FEVD estimator and its SE recipe. Although the approach we used in our critique differs from Greene's, our findings are consistent with his on all points where the analyses overlap.

The response by Plümper and Troeger (2011) does not find any error in these critiques. Instead of offering formal analysis, they again rely on simulation experiments based on *ad hoc* assumptions, without precise definitions of the conditions of the debate and without mathematics to lend precision to the discussion. In this rejoinder, we do not attempt to address every issue in the wide-ranging response. However, we have a duty to inform those who might otherwise rely on empirical results obtained with FEVD and those who might be attracted to use the method in the future.

2 Model

The model in vector form is:

$$y = X\beta + Z\gamma + u + e. \tag{1}$$

The Xs are time-varying explanatory variables; the Zs are time-invariant explanatory variables; there is an unobserved group or unit effect u which is also time invariant and an overall unobserved error e that varies in both dimensions. It is possible that u might be correlated with some X or Z variables, in which case the variables so affected are described as endogenous, otherwise they are exogenous.

3 Instrumental Variables

The most transparent definition of the FEVD coefficient estimator is as linear instrumental variables (IV) with instruments $[Q_D X, Z]$. Here Q_D is the projection matrix that converts a data vector into deviations from group means. Other familiar estimators also have descriptions as IV estimators: fixed effects (FE) uses

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instruments $Q_D X$, and pooled OLS uses instruments $[X, Z]$. The Hausman-Taylor (HT) estimator is another, although it requires the variables to be partitioned, so the instruments are $[Q_D X, X_1, Z_1]$, where subscript "1" refers to assumed exogenous and "2" refers to assumed endogenous. Relative to HT, the FEVD instrument set omits the exogenous time-varying variables X_1 but includes the endogenous time-invariant variables Z_2.

FEVD and HT coincide exactly if both $X_1$and Z_2 are empty. Put differently, FEVD is the HT estimator under the assumption that all the time-varying *X*s are possibly endogenous and all the time-invariant *Z*s are exogenous.[1] There is nothing exceptional or objectionable about the coefficient estimates produced by FEVD: they are perfectly sensible IV estimates under a particular exogeneity assumption. The difference between HT and FEVD, as estimation *strategies*, is that HT would employ the FEVD instruments under an explicit exogeneity assumption, whereas FEVD would use a predetermined set of instruments as a "canned" solution, without regard to any such reasoning. On this view, PT's contribution is to recommend a standard estimator in what is likely an inappropriate context.[2]

The IV interpretation of FEVD also simplifies life for the applied researcher. The internal *Stata* commands *ivregress* and *xtivreg* can calculate the same coefficient estimates as FEVD—and they provide appropriate SEs.[3]

4 Variances and SEs

Plümper and Troeger (2011) state that "our original PA article does not discuss SEs." That is patently untrue. In Plümper and Troeger (2007), FEVD is defined by a three-stage regression procedure, where the sole purpose of the third stage is to adjust the SEs. The coefficients produced by the first two stages remain unchanged, so the final stage is irrelevant in estimating the coefficients. The need for the third stage to obtain correct SEs is discussed in multiple places in Plümper and Troeger (2007), starting with the abstract.[4] Clearly the recipe in Plümper and Troeger (2007) is to use the SEs produced by the last stage, perhaps after adjustments for residual correlations and degrees of freedom.

We showed that this published recipe provides SEs that are dramatically too small, leading to overconfidence in the results. We wish to be perfectly clear on one point: our analysis was of the algorithm in the published paper. Through an abundance of caution, we also confirmed that the same defective recipe is implemented in the version of their software *xtfevd.ado* that was in distribution up until early 2010. The method of calculating SEs is the issue here, not a particular software implementation.[5]

Plümper and Troeger (2011) now disown their three-stage procedure as a source of SEs. Instead, they adopt a standard IV approach, exactly as recommended in our equations (13) and (14). However, now they claim a substantive disagreement confined to the appropriate estimate of the variance of the

[1]The additional step in the HT estimator of weighting by an estimate of the covariance matrix of the errors is redundant here, because with this instrument set the estimates are just identified.

[2]One aspect of Plümper and Troeger (2007) that is largely overlooked in the subsequent debate is the treatment of "rarely changing" explanatory variables, that is, variables that have little within-group variability over time, in other words *X*s that are close to being *Z*s. As far as we can tell, the proposal is to treat such *X*s as if they were in fact *Z*s. Thus, even though as *X*s they are thought to be endogenous, they will nevertheless be used directly as instruments, rather than using only their within-group variation $Q_D X$. That proposal is an obvious extension of the result in the literature on "weak instruments" in simple models that OLS might in some cases be a better estimator than IV, even when the former is inconsistent. The survey by Murray (2006) is a readable introduction to this, now extensive, literature.

[3]The internal *Stata* command for the HT estimator called *xthtaylor* reports an error when the field for listing the X_1 variables is left empty (at least up to *Stata* Release 11). There is no logical reason for that restriction, in that the estimator is still well defined provided the Z_2 field is similarly empty, although there may be practical reasons in the implementation of the computer code.

[4]On page 125, they say more explicitly: "This third stage allows computing correct SEs for the coefficients of the (almost) invariant variables." Further discussion of SEs occurs on page 129: "The estimation of stage 3 proves necessary for various reasons. First of all, only the third stage allows obtaining the correct SEs. Not correcting the degrees of freedom leads to a potentially serious underestimation of SEs and overconfidence in the results." An even more detailed and explicit discussion of these third-stage SEs is presented in the working paper version publicly posted by the authors (Plümper and Troeger 2006).

[5]PT incorrectly attribute the error as a software issue, and then claim our analysis is "obsolete" since they have updated the software. They further disclaim responsibility for the software, stating "beta versions are meant to be invitations for users to identify bugs and errors and help developing the code further" (Plümper and Troeger 2011, footnote 4). While the software faithfully implemented the written recipe, this declaration of *Caveat Emptor* will nonetheless surprise many of those applied workers who used this code to produce published research. The only indication of the status of the software as "beta" is that single word located in the header comment that is embedded in the code itself—a level of detail that most users would overlook and a use of programming jargon that few would understand. We note that the current code (version 4.0, Plümper and Troeger, 2010) is also described as beta in the same way. Moreover, neither this code itself nor the associated help file contains any notice of the previously erroneous SEs—almost a year after PT first removed version 2 from circulation, while citing our critique as the cause.

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unit effect, σ_u^2. Moreover, they devote much of Section 5 to discussing their Monte Carlo evidence that our recommendation is underconfident. Since PT provide no definition of the crucial symbols $\hat{\eta}$ and $\hat{u}$ in their equations (10) and (11), respectively, we can only turn to their software implementation to deduce the meaning. There we discover that $\sigma_{\hat{u}}^2$, in the formula attributed to us, is an estimate of the variance of $Z\gamma+u$ in equation (1) above. This is clearly inconsistent with our definition of σ_u^2, which is the variance of u alone. Moreover, we find that $\sigma_{\hat{\eta}}^2$ in their code is an estimate of the variance of u alone. In short, while PT rightly criticize a nonsensical variance estimator, they wrongly attribute it to us. At the same time, they adopt exactly the approach we recommend, while calling it their own.

5 Exogeneity Assumptions

The many claims of superiority of FEVD over other estimators that are made in Plümper and Troeger (2007), and which remarkably are expanded upon in Plümper and Troeger (2011), become clearer through the analytical lens we provide. The FEVD coefficient estimator is just linear IV with a particular predetermined instrument set. Pooled OLS and FE are also IV but with different instruments. There are infinitely many other IV estimators, including HT that requires other input of exogeneity assumptions to specify subsets of the explanatory variables.

Of course, the simplicity of FEVD is attractive to the applied worker: a canned or packaged solution needs no effort to justify its exogeneity assumptions. But the same simplicity is provided by OLS, so simplicity alone is not reassuring. The properties of any particular IV estimator depend on the validity and relevance of its instruments as measured, respectively, by being uncorrelated with the error term and strongly correlated with the endogenous explanatory variables. For any particular choice of instruments, it is always possible to invent Monte Carlo simulation experiments in which your chosen IV estimator does well, or where it does badly compared to a different choice of instruments. Other procedures that choose instruments or mix estimators based on empirical evidence will be less efficient when the predetermined instrument choice is nearly optimal, but might be safer bets when the prejudice is badly wrong. None of these statements relies on the asymptotic theory being a fully reliable guide to actual performance in small samples.

Remarkably, Plümper and Troeger (2011) argue at length that it is impossible to make informed instrument choices in a world where everything depends on everything else in ways we do not fully know. Such postmodernism will surely surprise the mainstream reader of *Political Analysis*. It is hard to see how it fits with the business of Plümper and Troeger (2007), which purports to be parameter estimation in causal statistical models. Parameters only have meaning if, at least in principle, we can identify them independently from the melange of potential influences in our model of the world. If you believe that everything-depends-on-everything-else in ways you cannot fathom, then it is impossible to talk meaningfully about parameters—let alone to estimate them. On the other hand, with appropriate theory and other knowledge of the phenomenon being studied, we can describe causes to which we can give names, and which with suitable data we can perhaps quantify.

In specifying statistical models, we routinely use theory, other knowledge, and experience to determine the variables of interest and to interpret the parameters of the relationship. Judgments of exogeneity and specifications of suitable instruments come from the same sources. We use certain instruments because we can reason about their likely efficacy, based on knowledge of the particular phenomenon being studied. Of course, this knowledge will always be imperfect, and mistakes will be made, but if you are not willing to make some claims about exogeneity you are not entitled to make claims about causal inference either.

The arguments used by PT can easily be driven to absurd extremes. They claim for instance that since you cannot observe u, you cannot know if an instrument candidate is exogenous. We could with equal justification argue that since you cannot observe e, you cannot know if $Q_D X$ is exogenous. (After all, you can just as easily have a time-varying omitted variable as a time-invariant one.) In that case, if the solution to lack of knowledge of exogeneity is to take the simplest predetermined set of instruments, regardless of the implications, simple pooled OLS will always be the preferred estimator!

6 The "Instrument Option" of IV-FEVD

Despite arguing that HT and other targeted IV estimators are illogical, on the grounds that exogeneity status is unknowable, Plümper and Troeger (2011) highlight an additional "instrument option," whereby targeted IV estimation is subsumed into the definition of FEVD:

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> FEVD allows the use of instruments for time-invariant variables in stage 2 and this option renders FEVD consistent whenever HT is consistent. . . . Using this option with internal instruments (that is with instruments taken from the set of right-hand-side variables) guarantees that the parameter estimates of FEVD and HT become identical. . . . However, in contrast to HT, IV-FEVD allows researchers to use instruments from outside the model. This is an option that HT does not provide for. (Plümper and Troeger 2011)

Thus, it is claimed, not only is FEVD as good as HT when the information needed for HT is available (indeed, it is claimed the estimates are identical), but when further information is available FEVD can make gains that HT cannot achieve because HT is limited to instruments based on the set of explanatory variables. In passing, we note that this supposed limit on HT is strange and contrived: the HT estimator is explicitly introduced as an IV technique in a paper that also discusses outside instruments.

The claims that IV-FEVD will replicate HT or better its efficiency are dubious on other grounds. The method described here applies IV to the second-stage regression (of the group means of residuals from FE onto the time-invariant *Z*s). The coefficients for the time-varying *X*s will remain simply FE from the first stage, so they will not benefit from the additional instruments and therefore cannot be more efficient than HT. The coefficients of the *Z*s from IV estimation of the second stage will in general be different from IV estimation of the full model containing both *X*s and *Z*s because there will no longer be the neat orthogonality among the explanatory variables and instruments that allows (original) FEVD to be interpreted as joint IV on the full model. Moreover, if the instruments over-identify the model, there will be further efficiencies to be exploited in the full model by weighting by the covariance matrix of the errors, as is done in the HT estimator. Without an explicit proof being given, the presumption must be that IV on the partial model of stage two will make less than fully efficient use of the instruments.

7 Conclusions

Plümper and Troeger respond to their critics and attempt to reinvent FEVD so that it transcends the criticism. They do not identify any errors in the analyses of the critics, but instead continue to assert the superiority of their invention. They acknowledge at the end of their response that the SEs calculated using the recipe of Plümper and Troeger (2007) are too small, and they now adopt the IV approach to obtaining correct SEs. In the process, the three-stage regression procedure—which ironically *defines* FEVD for many users—has been abandoned. But their claim that the FEVD instruments are always optimal is specious because the same claim can just as logically be made for simple OLS and just as easily supported with *ad hoc* Monte Carlo assumptions.

The situation where the IV estimator that corresponds to FEVD will be of interest is where the time-varying *X*s are all possibly endogenous, and will be treated as such, but no similar fears are held regarding the time-invariant *Z*s. Indeed, at the end of their response PT seem to concede as much:

> In this paper, we respond to our critics and reinforce the case for using FEVD when researchers are simultaneously interested in time-varying variables correlated with the unit effects and time-invariant variables. (Plümper and Troeger 2011)

Of course, this is precisely the exogeneity assumption under which the more complicated HT method reduces to FEVD. The estimator in this case is simple linear IV, so standard ideas will motivate the estimator and indicate its statistical properties, and standard software is available to compute the estimates and provide appropriate standard errors. Extensions such as the ''rarely changing variables'' problem of Plümper and Troeger (2007) are also familiar in the IV literature. Thus, there is no need for ''vector decomposition'' and no distinct estimator to call FEVD. With appropriate justification for the instruments, FEVD is just IV. Without that justification, FEVD is just mistaken.

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