Response to the discussants' comments:

My primary intention of getting involved in this decade long controversies over the endogeneity-backed IV route is to help clarify the situation and offer a way out of the muddled and seemingly circular arguments. In order to cut through the ambiguity of the still growing complexity of the IV enterprise, a simple and alternative interpretation of the IV route is needed. When it dawned on me that the nature of the IV method was to re-specify the explanatory variable of *a priori* interest by a non-optimal predictor of the variable, I thought this radically different interpretation adequately simple and unpretentious to fulfil the task, especially for applied economists. I am clearly wrong judging from the referees' comments.

I am disappointed to find my interpretation hardly appreciated in the referees' comments. Instead, my paper is viewed as virtually another polemic attempt to extend the controversies. I therefore realise how hard it is to get my message across, especially to those seasoned modellers who have been solidly trained by econometrics textbooks. In order to try and make them rethink about the nature of endogeneity-backed IV estimators outside the box of estimation methods, I need to emphasise the following points in the paper revision.

1. The paper is focused on the *endogeneity-backed* IV route, and is thus mainly on how the *a priori* assumed symmetric conditioning (or interdependence) of the simultaneous variables has been revised by the IV estimators; in other words, it is about the implicit model re-specification effect of the IVs, rather than their estimation effect, which has been abundantly discussed in econometrics textbooks.

2. Essentially, the re-specification effect of the IV route is to make the modeller commit unequivocally to a conditional expectation model in which the *a priori* explanatory variable of interest has been rejected as a valid conditional variable and replaced by one of its non-optimal predictors, i.e. one from a multitude of IV-generated regressors. This unambiguous commitment is independent of whatever positions I take on the exogeneity definition or conditional representations.

3. The IV re-specification revises *a priori* assumed symmetric conditioning relationships into asymmetric ones and hence invoke unceasing causal disputes among applied modellers. Unfortunately, the re-specification has been disguised under various seemingly harmless identification conditions in econometrics textbooks. Once we comprehend those identification conditions outside the estimation box, we realise what the conditions amount to is nothing but fulfilment of Cox's statement (1992) that statistical models requires variables to be represented in certain 'essential asymmetry between cause and effect'. Now, the question narrows down to whether we should rely on IVs to achieve such an asymmetry? In other words, is it methodologically sound for us to rely on a bunch of IVs which are moderately correlated with the explanatory variable of interest but not substantively cause it to define such asymmetry, when we know there does not exist a unique bunch of IVs?

4. The textbook argument of possible correlation between a particular explanatory variable and the error term is flawed in justifying the use of the IV route. In addition to what has already been argued in the paper, it is worth noting that the correlation argument is particularly tenuous in microeconometric models, where model fits are generally low in spite of the use of a large number of control variables. Consider the

model: $y_i = \alpha_0 + \alpha_1 x_i + \sum_j^k \beta_j z_{ij} + u_i$ in the context of analysing large cross-section samples (Note that economists use x_i to distinguish it from z_{ij} purely from substantive reasoning whereas mathematically all the regressors in the equation share the same status). Frequently, such a model yields $R^2 < 0.3$ with $k > 5$. In cases like that, it is almost impossible to identify, from the residuals, a sizable correlation between x_i and u_i, as opposed to possibly similar correlations between any z_{ij} and u_i, even if we disregard the possibilities of omitted variables and mis-specified functional forms. Obviously, $cov(x_i u_i)$ is predicated on us endogenising x_i, i.e. extending the above single-equation model into a two-equation model with the added equation explaining x_i. However, we should also endogenise any of those z_{ij} if we pursue faithfully the Walrasian general equilibrium notion. Similarly, the measurement error argument will also leads us into an impasse since z_{ij} may equally contain measurement error as x_i. In short, the correlation-based argument which is focused on a single regressor loses weight as soon as we adopt multiple regression models with expectedly limited explanatory power for the sample data under consideration.

5. Nevertheless, the IV route enhances the 'successful' rate of getting significant estimates of the parameter of interest under the above circumstance, thanks to the non-uniqueness of IV choices, plus the fact that it is practically impossible to achieve a close fit of the assumed endogenous explanatory variable no matter how over-identified the IV equation is. The situation demands for more model evaluation and selection criteria than those commonly used in micro-econometric studies.

6. A very recent empirical experiment following the above line of arguments has revealed that the IV route is substantively more harmful than what I anticipated when I was writing the IV paper. The harm is caused by the fact that the IV route blocks the way of rigorously investigating the circumstances under which *a priori* postulated conditional relationships would hold. The details of the experiment are reported in 'What Happens to Wage Elasticities When We Strip Playometrics? Revisiting Married Women Labour Supply Model': http://www.soas.ac.uk/economics/research/workingpapers/file97784.pdf . Among other things, the empirical results have shown that lessons from macro-econometrics are indeed valuable for the micro context.

7. I am grateful for the various oversights in the working paper which have been spotted by the referees. However, I believe it essential to keep the historical and methodological discussion together in one paper, because the paper is targeted at the broad readership of applied economists, rather than scholars of the history and methodology of econometrics.