Report on "Asset Transfers and Household Neediness"
by Elliott Collins and Ethan Ligon

Summary

There is much to like in this ambitious and innovative paper, the most original contribution of which the novel way of estimating the welfare impact of an external intervention.

The main shortcoming of the paper is that it suffers from a kind of multiple personality disorder, perhaps reflecting differences in writing style between the two authors. The introduction and conclusion bring to the fore the original conceptual framework summarized – somewhat hastily – in sections 4 and 5. But the long empirical analysis section 6 largely fails to impress.

This long section meanders somewhat haplessly between model predictions, regression results, and conjectures, often losing the sharp focus present in Sections 4 and 5. By squinting long enough, the reader can kind of guess a grand design underneath the estimation sequencing. But this grand design is largely buried under a clutter of distracting detail. Someone needs to take this section apart and rewrite it in a focused way. In particular, we need to see a much clearer statement of what the model predicts to be the response of treated households to the TUP. It is quite frustrating for the reader to put up with quite a bit of fairly intricate math in Section 4 to then see the authors flaunt it in Section 6 in favor of the reduced-form discursive style common of run-of-the-mill RCT papers.

Main recommendations

1. The paper can be better organized. The most novel feature is the derivation and application of a method for the estimation of log marginal utility at the individual level. This estimate is then used for several purposes: (1) to test the effect of treatment on welfare (last row of Table 2); (2) to estimate income elasticity of demand for various goods (last column of Table 3); (3) to estimate the distribution of welfare with and without treatment (Figure 1); to test whether a rise in wealth is the channel through which treatment affects asset holdings (comparing columns 3 and 4 of Tables 3 and 4); and (4) to perform a similar analysis on occupation (comparing columns 3 and 4 of Table 5 and 6).

   Of these, only purposes 1 and 2 are presented in the conceptual framework. The distribution of welfare in Figure 1 is extremely interesting, yet it only receives a cursory look, as if it was but an afterthought – when it could in fact be a centerpiece of the paper.

   In contrast, the logic behind purposes 3 and 4 is not properly explained anywhere. Furthermore, in which sense is controlling for an estimated

parameter $\log \lambda$ a meaningful econometric procedure? Surely there is measurement error in $\log \lambda$ and this individual-specific error may be correlated with other observed and unobserved quantities in ways that could affect the test. This needs to be better explained.

2. Section 4 is fascinating but also quite terse and hard to read. Some of the algebra could be relegated to an appendix, releasing space to summarize clearly the main predictions of the model. It is also somewhat bizarre that the two building blocks of this Section come from two unpublished (and hence not fully scrutinized) papers. For instance, I do not understand in what sense the household model of Bandiera et al. differs from other similar models. To take but one example, Townsend has been offering models of this kind for several decades, so I do not see what is so special about Bandiera et al.

   Building on a recent unpublished paper by one of the authors is problematic for another reason. I suppose that other paper develops the $\log \lambda$ logic in more detail. But the reader cannot be expected to have read that paper in order to understand this one. We should be able to understand the logic of the model predictions so as to be able to judge the validity of the empirical tests presented in the paper. We do not need to see all the algebra which, I suppose, is in the other paper, but we need to be given enough information in order to judge how sound the testing strategy of this paper is.

3. Only a small fraction of the empirical section is foreshadowed in Section 4 and 5. There are quite a few tests reported in Section 6 that are not based on any model derivation, and are little different from standard reduced-form RCT analysis – except with somewhat less care and sophistication applied to the econometrics. This is of course distracting because, when investigating $\log \lambda$ the paper is quite structural. This mismatch of strongly structural features and pure reduced-form features leaves the reader confused.

4. Even as a reduced-form analysis, Section 5 disappoints. There is a lot of going back and forth between evidence and model, and many side results are commented on in what are little more than (wild) conjectures. They also talk of 'surprises', but the reader does not know what this means: surprise relative to what? At the very least, I would expect the authors to clearly state what predictions their model – or the literature, or even 'common sense' – make about treatment outcomes. Otherwise the exercise feels too much like a fishing expedition, e.g., 'let's regress everything we have on treatment and see what happens'. If this is indeed the authors' approach, then they must start reporting Bonferroni-style corrections and the like.

Other comments

a. I was shocked by the cavalier attitude of the authors to the issue of corner solutions. This is most apparent in Table 2 where, due to zero expenditures,

each good-specific regression is estimated on a different set of observations and yet the estimates are pooled to yield an estimate of the average log λ. Unless I am very mistaken, when a household does not consume a particular good because it is in a corner, then the demand system is different, which also implies that all income elasticities for that household are different. Hence I do not see how Diff estimates can be pooled across regressions to yield log λ. If the authors wish to stick to this approach, they need to demonstrate that it is valid – e.g., at the very least using Monte Carlo simulations to validate their approach.

A similar criticism applies to the Sections 6.2 and 6.3 as well. In those sections, the authors keep all observations in the estimation, which I suppose is more satisfactory. But this still leaves open the issue of corner solutions and whether corners affect the logic of using log λ as control to test wealth channels.

b. Section 6.2 is quite a bit more tentative than the other two empirical sections. I cannot help but think that the authors do not believe that their model applies to assets and durables. It is not clear why not. Is it because they believe that not enough time has passed since treatment, and hence households are not yet on some kind of steady state? This deserves more discussion. Otherwise, the authors may dispense with that sub-section – or relegate it to an online appendix.

c. Section 6.3 makes a superficial effort at deriving formal predictions about occupation choices from the model. Unfortunately, this effort fails to convince. A better approach would be to derive these predictions formally in an appendix. Section 6.3 could then explain how these predictions are going to be tested. As written, the reader senses that there is a lot of interesting material in the section, but it somehow remains mysterious and out of reach.

d. On page 19 (end of 2nd paragraph), the authors write that Figure 1/stochastic dominance implies that mean neediness falls for *everyone*. This is of course incorrect: people could have changed their rank in the welfare distribution as a result of treatment. For instance, someone at the 50% percentile before treatment had a log λ of around -0.2, judging from Figure 1. But this person could have fallen at the 25% percentile after treatment, with a log λ of, say, 0.

e. At the very end of the paper, the authors argue that knowing log λ is useful for various purposes, such as conducting counterfactual exercises. I agree. Unfortunately, the authors fail to deliver on this front, and instead spend much time and energy running a fairly standard impact evaluation (without many of the bells and whistles found in other papers). This feels like a wasted opportunity.

Small comments

-p.2 par 3: how much does $1.62 represent compared with controls?

-p.2 bottom: eduation -> education

-p.3 top par: this is confusing: what is BRAC doing running a pilot if its operations have been closed?

-p.4 par 3: the cost of physical transfers is probably more than $350 when we include delivery/administrative costs

-p.7 end of section 3: there is considerable attrition yet the paper is entirely silent about this. Could this have affected inference?

-p.8 B(K) function: this is a bizarre way of writing a credit/liquidity constraint. Are there any restrictions on that function?

-p.9: K' is never defined – I suppose this is a switch of notation from the earlier time-indexed notation, to a first difference notation.

-p.9: price functions P(.) and q(.) are a bit unusual – normally we expect to find prices to be multiplicative. This notational trick seems to be inspired by the desire to allow for non-traded goods. What is distracting is that if P(.) is indeed a combination of actual prices and shadow prices, then it has a specific functional form that depends on the model. I would prefer relegating some of the algebra on p.9 to an Appendix but being more open about these modeling choices. And I will not accept as justification the fact that they appear in an unpublished paper (or even published paper, for that matter).

-p.9: L>=0 implies that households are unable to hire in labor. How realistic is this assumption?

-p.10: there is a direct link between neediness and standard social welfare functions (e.g., Atkinson, Duclos). The authors clearly have in mind a model in which people have the same utility function – or at least the same curvature of their utility function relative to income. This is a valid philosophical approach, but the link with the social welfare function literature should at least be acknowledged.

-Section 4.1: why do we need such a cumbersome presentation of treatment effect estimation? The reader will know what a dif-in-dif estimator is, there is no need to present it in such an abstract manner.

-Equations (6): the assumption that Qshat(K') = Qstilde(K')+Khat looks very strange. I would have expected Khat to be included within function Q, since it is a production function and capital contributes to output.

-p.12 ATE definition: Please note that the two summation terms are actually welfare functions, with equal weights assigned to all, and a curvature/inequality aversion imposed by the log function. In other words, the ATE compares two values of a specific social welfare function, which the authors call 'neediness'. At the very least we need a link to the literature at this point. If the authors are unfamiliar to this body of work, Chapter 4 of the book by Duclos and Araar (Poverty and Equity, Springer 2006) contains an excellent and concise introduction.

-p.12 bottom par: I failed to understand the switch in notation regarding s, given that each household presumably experiences a different state of the world.

-Section 5: must present clear predictions regarding (1) marginal utilities (presumably, they should fall, reflecting an increase in consumption welfare); (2) assets, durables, and portfolios; and (3) occupation and labor.

-p.17: define Frisch elasticity