Consumption commitments and spousal labour supply

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This paper uses data from the German Socio-Economic Panel (G-SOEP) to investigate the extent to which mortgage commitments can amplify the insurance role of spousal labour supply in the context of married couples. First, the paper explains the underlying mechanism through augmenting the two-good consumption commitments model of Chetty and Szeidl (2007, May) with endogenous labour supply. The paper then seeks to overcome various modelling challenges associated with testing the model predictions, including unobserved individual heterogeneity and censoring, through applying various parametric and semi-parametric estimators. The regression results from the preferred specifications do not lend support to a significant effect of mortgage commitments on the hours worked by the secondary earner.

I. INTRODUCTION

Conventional economics models assume a single composite good such that households can costlessly adjust their consumption bundle at all times. This simplification overlooks consumption commitments, whose adjustments entail significant transaction costs, making their consumption patterns relatively insensitive to moderate wealth shocks. Consumption commitments such as mortgage payments comprise a considerable share of household expenditure. Chetty and Szeidl (2007, May), for instance, found that an average US household keeps approximately half of the budget fixed, whilst concentrating their wealth reductions to adjustables such as food and entertainment. The presence of commitments essentially poses a binding liquidity constraint to the optimisation problems faced by households, thereby magnifying the welfare costs of moderate wealth shocks.

A potential source of insurance against these shocks is spousal labour supply in the context of married couples. When a member of a couple experiences an unemployment spell, increased spousal labour supply can mitigate the resulting wealth reduction. Since the primary earner (the head) is often already working full-time, previous work focuses on the response of the secondary earner (the spouse) to the head's job loss. This insurance role of spousal labour supply may be particularly important for households that are constrained by commitments such as mortgages. Specifically, mortgages might incentivise the spouse to work as they must be paid periodically and often constitute a substantial fraction of the household budget, insofar as the household wishes to maintain their commitments. When the household instead opts to reduce their housing commitment by moving home or defaulting on their mortgage payments, this incentive effect of commitments on spousal labour supply would be weakened. Nevertheless, little is known about the empirical plausibility of these hypotheses, partially due to data limitations.

To fill this gap in the literature, this paper seeks to test whether mortgage commitments can amplify the insurance role of spousal labour supply, using a panel of married couples from the German Socio-Economic Panel (G-SOEP). I address a wide range of econometric challenges, notably left-censoring of hours and unobserved heterogeneity, by implementing several parametric and semi-parametric estimators, including Tobit, Heckit, FE and trimmed LAD estimators. While there is mixed evidence of an incentive effect on the extensive margin, my findings from the preferred specifications generally do not support the model predictions.

The rest of this paper proceeds as follows. Section II reviews the existing literature. Section III presents a stylised model and assesses the empirical relevance of consumption commitments. Section IV–VI present my empirical strategy and data, followed by a discussion of my estimation results. Finally, section VII concludes.

II. LITERATURE REVIEW

The linkage between consumption commitments and spousal labour supply builds on several strands of research.

First, ever since the pioneering work of Woytinsky (1940) and Mincer (1962), a large number of studies have attempted to test for the added worker effect (AWE), i.e., an increase in spousal labour supply in response to the head's unemployment shocks. Empirical tests of the AWE have often been unable to identify a sizable contemporaneous AWE, with mixed success in addressing the challenges of controlling for unobserved heterogeneity in individuals' taste for working and the discouraged worker effect. For example, Maloney (1987) found little evidence for the AWE in the US using a cross-sectional analysis, whereas Layard, Barton and Zabalza (1980) found evidence contrary to the AWE among UK households, which may in part result from the incentive costs of means-tested welfare programmes in the UK.

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As noted by Heckman and MaCurdy (1980; 2014), these findings might be a reflection of the transitory nature of the unemployment spells which in a life-cycle context should not distort the intertemporal consumption-leisure decisions of the spouse. Lundberg (1985) performed a dynamic simulation of short-run transition probabilities across non-participation, employment and unemployment, finding a small AWE for white wives. Stephens (2002, July) specifically examined the AWE associated with involuntary worker displacement as a more permanent wealth shock. He did find a substantial, persistent AWE and some small anticipation effect pre-displacement. More recently, Cullen and Gruber (2000) found that the benefits provision in the US materially crowds out the AWE through instrumenting UI receipts with potential UI entitlement. In short, spousal labour supply per se might still be an important mechanism for intra-household risk sharing if isolated from confounding channels. One channel that remains largely untested is the presence of consumption commitments and liquidity constraints, as highlighted by Mincer (1962), Lundberg (1985) and Chetty and Szeidl (2007, May). My analysis can therefore provide new perspectives into the AWE literature.

Second, the consumption commitments model as formalised by Chetty and Szeidl (2007, May; 2016, March) can potentially help explain wide-ranging empirical regularities such as wage rigidities (Postlewaite, Samuelson and Silverman, 2008), gambling and the AWE (Chetty and Szeidl, 2007, May). There is a growing body of literature seeking to derive economic insights from housing commitments in particular. For example, Flavin and Nakagawa (2008) set up an asset-pricing model with complementary housing and food and non-convex adjustment costs. Their model generates a substantially better empirical fit to the observed consumption dynamics compared to standard consumption-CAPM models. Their model also does not impose an implausibly high degree of risk aversion as would be the case with habit formation models that produce similar predictions. As another example, Shore and Sinai (2010) found empirical support for the prediction that high adjustment costs induce same-occupation couples to consume more housing as they are exposed to greater unemployment risks due to highly correlated unemployment shocks than other couples.

Beyond informing on the consumption smoothing mechanisms, whether commitments affect behavioural responses to wealth shocks has important implications for the welfare cost of income taxes. A few studies found a positive relationship between land price (or mortgage commitments) and female labour market participation (Del Boca and Lusardi, 2003, December; Fortin, 1995; Yoshikawa and Ohtaka, 1989). However, causality might run both ways, and the cross-sectional analysis performed in these studies are ill-suited for capturing the taste heterogeneity nor the underlying dynamics. The only study that utilises a panel estimator recorded a practically negligible effect of commitments on participation (Bottazzi, 2004, January). Furthermore, these studies do not examine the interaction of commitments and the AWE on the intensive margin, which is arguably more relevant than the extensive margin given the great majority of households have dual earners nowadays. My subsequent analysis in this paper aims to fill this gap.

III. THEORETICAL MODEL

A. Model setup

The commitments model developed by Chetty and Szeidl is a consumption model with exogenous labour supply in which agents consume two types of goods, a "commitment" (e.g., housing) and an "adjustable" (e.g., food), subject to wealth shocks after commiting to a housing choice. This section sets up an augmented version with endogenous spousal labour supply.

Consider a married couple consuming housing (X) and food (C), individually supplying labour (H^1, H^2) at wages (W^1, W^2). The head's labour supply $\{H^1\}_t^T$ is exogenously determined (say, by employment contracts). For simplicity, interest rates, income taxes and discount rates are set to zero, prices of goods to one. Given a lifetime of T, A_0 and$A_{T+1} = 0$, the couple chooses a consumption-labour path to maximise the present value of lifetime utility:

$$\max_{\{C_t, X_t, H_t^2\}_{t=0}^T} U_0 = E_0 \sum_{t=0}^{T} u(C_t, X_t, H_t^2), \tag{1}$$

where u is strictly increasing in consumption, strictly decreasing in labour supply, strictly concave and twice-differentiable; consumption and leisure are normal goods ($u_{C,X} \geq 0$, $u_{C,H} \leq 0$, $u_{X,H} \leq 0$).

The dynamic budget constraint is

$$A_{t+1} = A_t + W_t^1 H_t^1 + W_t^2 H_t^2 - C_t - X_t - \kappa X_{t-1} \cdot \mathbf{1}(X_t \neq X_{t-1}), \tag{2}$$

where A denotes wealth and $\kappa X_{t-1} \cdot \mathbf{1}(X_t \neq X_{t-1})$ captures the cost of deviating from prior commitments (i.e. moving), with $\mathbf{1}(.)$ being an indicator function. That cost may be financial (e.g., monetary cost associated with moving home), or non-financial (e.g., due to habit formation).

The couple first chooses X_0 given A_0 and knowledge of $\{W_t^1, W_t^2\}_{t=0}^T$, after which it determines the consumption-labour path each period treating X_0 as given. To focus on linking commitments and spousal labour supply, I abstract from the optimal choice of X_0 and henceforth assume a perfect capital market and no uncertainty after period 1. This simplification implies a

flat optimal path, and that if the couple ever decides to move, it moves in period 1.

Chetty and Szeidl showed that the model with exogenous labour supply can be solved analytically under two sufficient conditions: (A1) the Inada conditions for food and housing; and (A2) that the marginal utility of food is non-decreasing in housing consumption $u_{C,X} \geq 0$. These conditions ensure that the couple would only move if the income shock are sufficiently large. Intuitively, the decision to move depends on a trade-off between the marginal utility gain from optimising on both food and housing consumption and the adjustment costs associated with attaining that optimum. Given a moderate or temporary income loss, the couple would optimally avoid the moving costs and cut food expenses only, for example by eating out less and switching to less expensive grocery brands. In contrast, for a large or persistent income loss, retaining the commitments requires large cuts in food expenses whose marginal utility cost tends to infinity as food expenses are reduced to zero. Therefore, the couple would instead pay the adjustment cost and move into a smaller house to reduce the pressure on cutting food expenses. Simiarly, for an income increase, spending the extra income on food only begets a diminishing marginal utility gain, so the couple would eventually move to a better house if the extra income is large enough. More formally, the optimal consumption path follows an (s, S) rule – there exists an inaction region (s, S) for lifetime income M, wherein the couple would not move; only when $M \notin (s, S)$ does the couple move. The size of this inaction region increases with the adjustment cost κ. In the limiting case of $\kappa \to 0$, we are back to a standard single-good model.

A1 and A2 are guaranteed by the CES utility and the separable power utility under quite general parameterisation. Nonetheless, the resulting proposition that commitment choice is unresponsive to moderate shocks might apply to an even wider class of preferences. The model can shed light on the puzzlingly high degrees of risk aversion to moderate risks, with a rather unusual implication that the optimal social insurance should compensate more generously for short-term unemployment than long-term disability – insofar as the latter is considered a larger shock than the former.

With endogenous labour supply, the intuition underlying the (s, S) rule follows through, with an extension that the couple can also alter labour supply to potentially offset the wealth fluctuations. The presence of commitments can help explain why the wealth elasticity of spousal labour supply could be much larger when commitments are fixed than in the longer run when commitments are eventually adjusted. Intuitively, when the head experiences a temporary unemployment spell, a desire to maintain the prior commitments might induce the spouse to enter the labour force and/or work longer hours. When the shock is large or permanent, the couple would re-optimise on both housing and food to alleviate the need for working excessive hours. In this sense, commitments may amplify the AWE.

This mechanism should be evaluated in light of the following caveats. First, if spousal labour supply is constrained by for example sluggish labour demand or search frictions at least in the short run, the couple is compelled to dramatically reduce food consumption as in the model with exogenous H^2. Second, other sources of insurance such as unemployment insurance and severance pay might crowd out the spousal labour supply response (Cullen and Gruber, 2000). Third, spouses are found to have Frisch-complementary leisure demand as they enjoy spending time together (Blundell, Pistaferri and Saporta-Eksten, 2017), weakening the incentive to make big adjustments in spousal labour supply. Fourth, time inputs of husbands and wives may not be perfect substitutes in home production. The final two concerns, however, are at odds with recent evidence from Blundell, Pistaferri and Saporta-Eksten (2017) that couple's childcare times are substitutes and that leisure are Marshallian substitutes. Perhaps as a result of the abundant cofounding forces, there remains a significant disagreement over how one should isolate the AWE, and thus over the efficacy of spousal labour supply as a self-insurance.

The next section presents evidence for distinguishing between commitments and adjustables from the G-SOEP data which will later be used to test several model predictions.

B. Classifying commitments and adjustables

From 2010 to 2013, the G-SOEP consistently records monthly and annual household expenditures by categories, including food at home, dining/drinking out, toiletries, health, telecommunications, education, culture, transportation, furniture, life insurance, other insurance, car repair and holiday. Using such records to classify commitments and adjustables may help assess the predictive power of the commitment model.

One way to identify commitments is to find categories whose expenditure is infrequently adjusted. In Figure 1, I plot histograms for the distribution of expenditure growth for the different categories. Following Chetty and Szeidl (2007), expenditure growth is defined as the log change in nominal expenditures per annum for nondurables, level change for durables (e.g., housing, furniture, clothes and shoes). I infer the annual expenditure by multiplying the monthly expenditure by 12 whenever only the latter is available. Housing expenditures are proxied by rent for renters (excluding heating, hot water, electricity, water and garbage removal), and maintenance costs and mortgage and interest payments for homeowners. Zero consumption growth is assigned to households that did not report moving. Reduction in expenses on durables is only possible through second-hand sales (i.e. negative expenditure growth).

The charts demonstrate clear variations in households' expenditure adjustment patterns, even at a yearly frequency. Discretionary spending on eating out, toiletries, health, culture, education, holiday and car repair appears to be adjusted much more frequently than spending on food at home, telecommunication, insurance, transportation and durables. Depending on the degree of imperfections in the corresponding second-hand market, durables can be good candidates for commitments. For housing in particular, there are substantial transaction costs in terms of brokerage fees, search cost and penalties for missed mortgage or rent payments and early contract termination. Indeed, moving is rare, with 75,246 out of 78,608 homeowner-year observations and 9,846 out of 17,700 renter-year observations staying where they lived the year before. In a similar spirit, transportation cost can be regarded as a commitment good insofar as it is primarily derived from commuting from work to home and vice versa, the location of which are fixed in the near term. Services like telecommunication and insurance can also be difficult to adjust due to contract re-negotiation and inertia. Finally, the distinction of food at home and eating out is straightforward, the former being more of a necessity that tracks certain household habits and the latter a luxury much like the other adjustables.

This frequency-based classification method, nonetheless, fails to trace out how households adjust their consumption bundle in response to wealth shocks. An alternative method may take an event study approach, as proposed by Chetty and Szeidl (2007), comparing the average annual growth rates in food and housing expenditure responses of homeowners and renters, normalising the incidence of shocks as year 0. Nevertheless, the large number of gaps in the G-SOEP records for a given household-year makes it difficult to apply such shock-based analysis.

In a larger context of Germany, housing expenditures also emerge as a good measure of commitment consumption. In Germany, duration of mortgage loans typically lasts for 25–30 years, similar to the US and Great Britain. Recurring mortgage and interest payments take up a great proportion of homeowners' expected expenditure. German mortgage providers require comparatively higher downpayments, as households can only borrow up to 60–80% of house value. There are also substantial real-estate transaction costs in terms of brokerage fees and taxes, with the latter costing approximately 12% of house value, which is much higher than in Great Britain (4.5%) and moderately lower in France (16%) and Italy (18%). These features of the German housing market make housing expenditure an ideal indicator of a household's commitment level.

IV. EMPIRICAL STRATEGY

The commitment model generates interesting predictions regarding the relationship between spousal labour supply and commitments. This section outlines my empirical strategy for testing those predictions, with a focus on the intensive margin in light of the recent rise in dual-earner couples.

Consider a bivariate structural model formed by a Mincerian wage equation and a semi-log hours equation. By substituting (3) into (4), I obtain the prototypical reduced-form hours equation (5)

$$H_{it} = \beta_1 x_{it} + \beta_2 Y_{it} + \beta_3 \ln W_{it} + \boldsymbol{Z}_{it}\boldsymbol{\delta} + u_{Hit} \tag{3}$$

$$\ln W_{it} = \eta Y_{it} + \boldsymbol{Z}_{it}\boldsymbol{\gamma} + u_{Wit} \tag{4}$$

$$H_{it} = \beta_1 x_{it} + (\beta_2 + \beta_3\eta) Y_{it} + \boldsymbol{Z}_{it} (\boldsymbol{\delta} + \beta_3\boldsymbol{\gamma}) + u_{Hit} + \beta_3 u_{Wit} \tag{5}$$

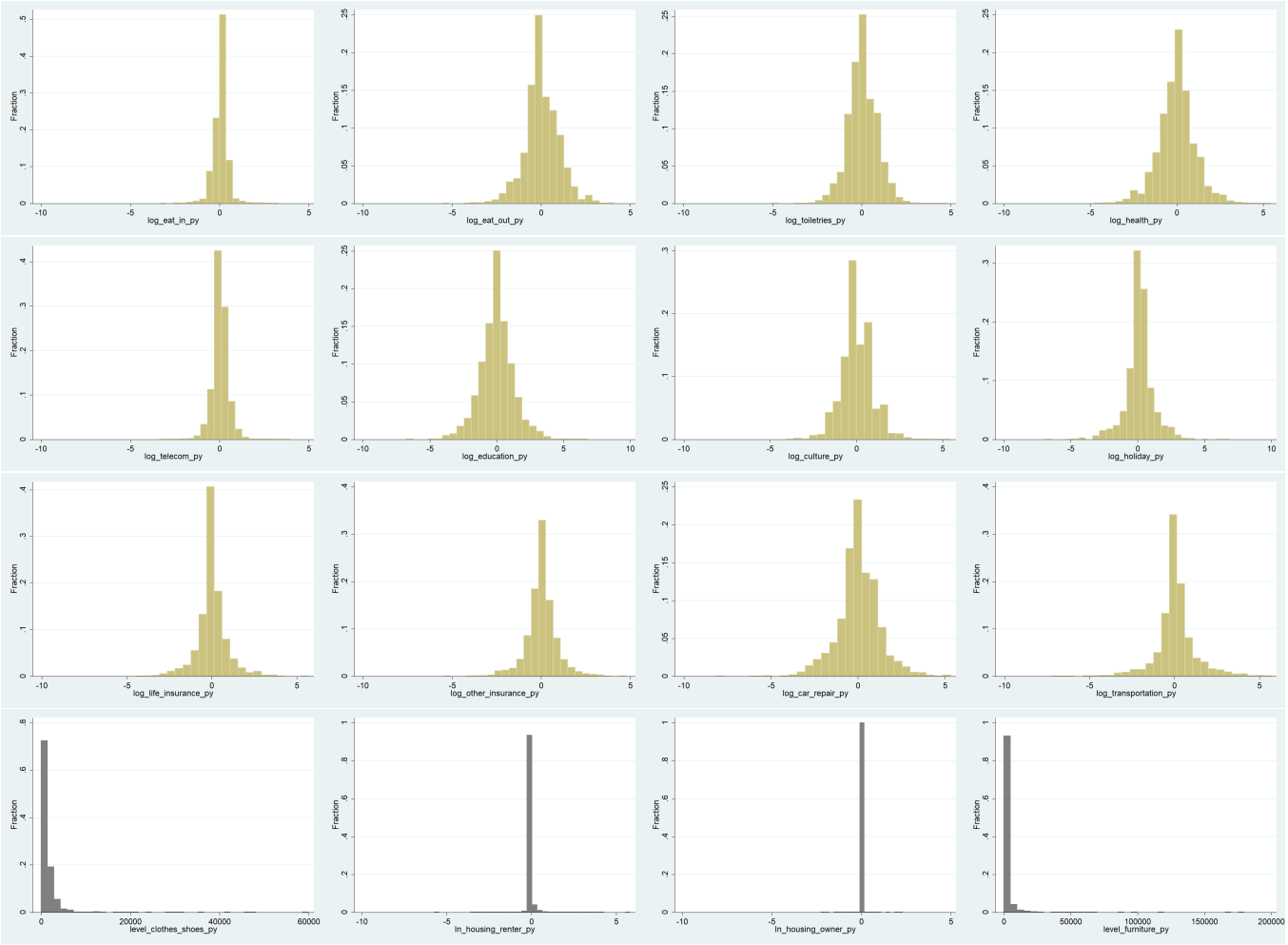
where x_{it} is the ratio of commitment consumption to net household income, Y_{it} is unearned/non-labour income, $\boldsymbol{Z}_{it}$ are control variables, and u_{Hit} and u_{Wit} are (unobserved) tastes for working.

The commitment model generates several potentially testable predictions. First, due to the negative wealth effect of retained commitments, devoting a greater proportion of income to commitments may require increased spousal labour supply, ceteris paribus. Second, so long as commitments are retained, a negative wealth shock can induce the spouses to work more (i.e. the AWE). Hence, for non-movers, the model predicts $\beta_1 > 0$ and $\beta_2 < 0$. Third, as we expect couples who opt to move home and break away from their commitments to behave differently, the equation is to be estimated separately for "movers" and "non-movers".

Generally, reduced-form estimation cannot provide a good test of economic theories without imposing assumptions regarding the underlying structure. Here, the main coefficient of interest β_1 might be identified if mortgages can be excluded from the wage equation, conditional on unearned income and background controls, which seems a plausible restriction. Identification of β_2 by setting $\eta = 0$ is unlikely to be valid as one would expect some cross-wage effect – the change in the wife's labor supply due to a change in the husband's wage, for a constant marginal utility of income – unless the spouse's leisure is separable from the head's, which is theoretically and empirically questionable (Blundell, Pistaferri and Saporta-Eksten, 2017). As a result, my estimates of (5) would capture both the AWE (β_2) and the cross-wage effect ($\beta_3\eta$). To disentangle the two through specifying a nonseparable preference, I would have to drop observations with non-positive unearned income corresponding to heads that are most affected by the shocks, which could lead to a downward bias in the estimated spousal response (Stephens, 2002).

OLS estimates of (5) will be biased and inconsistent. First, labour supply is censored at zero due to non-participation. Sample selection bias may arise from using a self-selected sample of working spouses. The inability to observe the offered wage distribution for those

FIG. 1. Expenditure adjustment patterns, yearly



who are not working makes it difficult to separately model the extensive margin (participation) and the intensive margin (hours). A general solution is to invoke exclusion restrictions by identifying factors that determine behaviours on either margin but not both. However, in the context of labour supply, identical regressor sets are often used for both margins due to the difficulty of conceiving plausible restrictions.

Second, spouses of heads who experience a wealth shock or take on high level of mortgages may have different tastes for working than spouses of heads who do not. To capture the observable heterogeneity, researchers have commonly incorporated a rich set of household and individual characteristics which are taken as predetermined. For instance, the number of young children in the household affects the opportunity cost of working for both spouses (Mincer 1962), despite the suspicion that fertility might be endogenous to the life-cycle allocation of time within a household. Furthermore, whenever panel datasets are available, researchers have also used individual fixed effects to purge heterogeneity that is time-invariant over the length of the panel (Gruber and Cullen, 1996, June). Yet, time-varying individual heterogeneity would still be left in the error term.

The inclusion of some controls might even introduce endogeneity – for example, due to 'assortative mating,' couples tend to share among themselves similar unobservables like ability and motivation which might also determine unearned income and demographic decisions. Whilst there is little evidence of endogeneity of other control variables, the exogeneity of work experience is indeed rejected in the US data (Heckman and MaCurdy, 1980; Mroz, 1987). Again, to the extent that these unobservables are time-invariant, using individual fixed effects can provide a good remedy.

Another concern is reverse causality: it is possible that spouses are working longer to fulfil certain eligibility criteria for taking on mortgages. Area-specific property prices and timing of borrowing might be good instruments for mortgages, in that they serve as powerful predictors for households' propensity to take on mortgages and plausibly do not have a direct impact on spousal labour supply independent of their

effect on appropriate controls. However, researchers have frequently struggled to find panels with matching prices series. Some reassurance might be taken from Bottazzi's (2004) inability to reject the null of exogeneity using a British panel.

Finally, macro factors such as regional unemployment probably play a role in determining the labour market opportunities facing the spouses (Lundberg, 1985). Unemployment shocks to the heads might indicate unfavourable macroeconomic conditions which could discourage their spouses from job search. It is thus important to control for time and region-specific effects.

The following subsections demonstrate my attempts to address these empirical challenges. As a starting point, I estimate variants of (5) by FE, Tobit and Heckit. Tobit and Heckit can serve to correct for selectivity bias whereas FE might help deal with time-invariant unobserved heterogeneity. Since both issues are important and Tobit with fixed effects suffers from the incidental parameter problem, I implement a semi-parametric alternative, Honoré's (1992) trimmed least-absolute-deviations (LAD) estimator.

A. FE

The linear FE estimator can help account for time-invariant unobserved heterogeneity which potentially plays a central role in explaining hours. Unlike the RE estimator, it allows for correlation between the individual-specific intercepts and the regressors, which is expected in this context (say, between ability and education). As the FE estimator essentially within-transforms all variables, its consistency as $N \to \infty$ requires strict exogeneity (conditional on individual fixed effects and selection) and the exclusion of constant regressors (and constant-difference regressors like age if time dummies are added). In an unbalanced panel, nother requirement is that to rule out partial correlation between selection and the idiosyncratic errors (Wooldridge, 2002). On grounds of preserving observations, within transformation is preferred to first differencing in a panel with gaps.

Nevertheless, the FE method have notable limitations. First, it cannot correct the sample selection bias unless the selection process is time-constant. Furthermore, strict exogeneity is likely to be violated even though contemporaneous exogeneity may hold. Finally, it cannot correct time-varying heterogeneity especially with longer panels.

B. Tobit

At an annual frequency, the large number of zero hours mostly reflect a corner solution to the utility maximisation problem given market wages. To the extent that we are interested in the desired hours response, Tobit-type models are useful for account for censored hours. The econometric problem is that we cannot observe the latent dependent variable $I_{it}^* = \mathbf{x}_{it}\boldsymbol{\delta} + u_{Iit}$, which reflects the utility difference between working and not working. Instead, we observe labour force participation $D_{it} = \mathbf{1}(I_{it}^* > 0)$, and if spouses do participate, their hours and wages. Intuitively, zero hours corresponds to a corner solution to the utility maximisation problem.

Sample selection bias in OLS estimates results from the correlation between ω_{it} and the structural errors from (3) and (4): $u_{jit} = \rho_j u_{Iit} + v_{jit}$, where v_{jit} is mean zero and orthogonal to $\mathbf{x}_{it}$ and u_{Iit}, $j = H, W$. Tobit-type models amount to imposing distributional restrictions onto ω_{it}, such that δ is estimable by the maximum likelihood method up to scaling. The Tobit model adopted by Heckman (1974), for example, assumes (1) $\rho_H = 1$, $Var(v_{Hit}) = 0$; (2) δ incorporates the parameters in the reduced form up to a constant of proportionality $(Var(u_{Hit}))^{1/2}$; and (3) u_{Hit} and u_{Wit} are jointly normal. These assumptions conveniently permit the construction of a log-likelihood function but can be restrictive. Blundell et al (1987) contended that some zeros could be due to involuntary unemployment and misclassifying them as non-participation causes inconsistency. Cogan (1981) and Mroz (1987) rejected this Tobit specification in favour of some generalised Tobit procedures which also diminish the bias due to the endogenous experience variable.

To highlight the consequences of inadequate distributional assumptions, I will present results from the Type-I Tobit as a naive model. It restrictively presumes both participation and hours to be governed by a single mechanism, forcing the coefficient sign on a given regressor and the ratio of marginal effects of different regressors to be equal for both margins.

C. Heckit

Similar to the more general Tobits, the Heckit model differentiates the two margins. It accounts for the sample selection bias by using a Probit for the participation equation and then augmenting the hours equation with the inverse Mills ratio (IMR).

Given the unavailability of reasonable exclusion restrictions, I allow participation and hours to be determined by the same set of regressors. Since second-stage identification relies on non-linearities in the IMR which is now a linear function of the regressors, the resulting multicollinearity problem can severely inflate the standard errors, and a t-test on the IMR coefficient has low power (Leung and Yu, 1996). In terms of efficiency, a two-part model might be preferred which is generally more efficient at modelling actual rather than potential outcomes (Duan, Manning, Morris and Newhouse, 1984). From a policymaking perspective, however, potential spousal response is arguably more

relevant in the context of labour supply. Policymakers cannot directly influence the actual mortgage take-up or spousal response to incentives; they can only seek to provide the optimal incentives that motivate potential responses.

Another limitation of the Heckit model is the imposition of normality assumption, the violation of which can lead to inconsistency and spurious inference. In these cases, semi-parametric alternatives like the trimmed LAD method can be more robust.

D. Trimmed LAD

Honoré's (1992) semi-parametric trimmed LAD estimator (TLAD) can potentially deal with both censoring and individual-specific intercepts. It is consistent and asymptotically normal as $N \to \infty$, under the Manski (1987) assumption that the errors of the latent variable are i.i.d. conditional on individual-specific intercepts and time-varying regressors.

The intuition behind the TLAD estimator is that it exploits a symmetry in the distribution of an identically censored latent variable. Analogous to the linear FE model, The estimator proceeds by differencing observations across time periods for every household, which removes individual fixed effects and yields differenced latent errors which are symmetrically distributed around zero. If the population parameter was known, trimming the observed errors – which will be asymmetric due to censoring – could bring about the same symmetry, which produces moment conditions that must hold at the true value of the parameter. These conditions can subsequently constitute the first-order conditions for the LAD minimisation problem.

Insofar as the TLAD method deals with the selectivity bias and the individual fixed effects simultaneously, its estimation results can serve as a test of the robustness of the proposed parametric estimators. The symmetry assumption required for its consistency is much less restrictive than typical parametric restrictions including normality and homoskedasticity. Another attractive feature is its tolerance of unbalanced panels. Yet, the resulting estimates may still be plagued by other sources of endogeneity, and its assumption of no serial correlation appears unrealistic as I expect an individual's past, current and future unexplained variations in labour market outcomes to be correlated.

V. DISCUSSION AND LIMITATION

The G-SOEP is the largest, longest-running representative longitudinal study in Germany, tracking the socioeconomic conditions of approximately 30,000 individuals from around 15,000 households annually since 1984. It collects household and individual-level information on a vast range of multidisciplinary topics, with a focus on family, housing, work, education, income, assets and subjective well-being. What makes it particularly attractive for my analysis is the availability of linkable couple pairs in the public-use data and measures of reasons for job separation starting from 1999 which allows me to focus on a more relevant sample of working couples. I have excluded cases of maternity/study/sabbatical leave, early retirement and termination of self employment as well as unemployment spells that do not result in job search. My final sample consists of 5,765 married couples of working age (20–65), who reported homeownership and no changes in marital status over the period 1999–2014. In accordance with the commitment model, the sample is then split into a subsample for 5,010 non-movers (households that have never moved) and another for 755 movers (households that have ever moved). However, there are insufficient year-on-year observations for the majority of the movers subsample, limiting my regression analysis to the non-movers subsample in which panel estimators (including FE and the preferred TLAD) might perform better.

Drawing on insights from the classification exercise in section III B, I use annual mortgage and interest payments as a proxy for homeowners' housing commitment. An alternative proxy could be housing expenditure; unfortunately, the G-SOEP lacks a consistent measure of housing expenditure for the period concerned. My dependent variable is spouses' annual working hours, from both full and part-time jobs. I define spouses as the secondary earners in terms of average earnings over the sample period, not necessarily wives which is a definition often adopted in the literature. My definition can shed new light into whether commitments induce spousal labour supply when the heads are constrained in their ability to adjust their hours: some wives are in fact primary earners, especially given my more recent sample, so classifying them as 'spouses' will tend to underestimate the spousal response. Nevertheless, my analysis remains largely faithful to the female labour supply literature in that the secondary earners are predominantly wives (80–90% of observations). Gender differences can be partially absorbed in a gender dummy, though they might have a differential impact on other regressors, a line of research which is not central to my investigation of the commitment.

My control set includes age, work experience, education and their squared terms, to capture their possibly nonlinear effects on hours; the number of children younger than five years old and the number of children between six and eighteen, to proxy for the opportunity costs of working in terms of home production; dummies for survey years and states of residence, as well as state-level unemployment rates from the Federal Statistical Office (Destatis), which is a key confounding macro factor; and a female dummy. Unearned income is taken to be the difference between household income and the spouse's earnings. The

former is the sum of earnings, asset flows and private transfers, with earnings being wages and salary from all employment plus bonuses, overtime and profit-sharing. All my time-invariant and constant-difference controls including age, state dummies and female dummy are dropped from the FE and the TLAD.

Table I presents the sample mean by households' moving-homeownership status. As expected, homeowners tend to have a higher income than renters, with an average differential of ¤15,804 per annum for non-movers and ¤12,761 for movers. Non-movers also appear to have an income premium over movers (¤1,591 for renters and ¤4,634 homeowners) and, among homeowners, take on higher mortgage and interest payments which in part reflect the moving costs as well as a liquidity constraint. The presence of young kids seems to be a key driver for moving for both renters and homeowners, whereas household size, state unemployment rate and region of residence do not notably differ by moving status. In terms of individual-level charateristics, compared to movers in the same homeownership category, non-movers appear to be older by –7 years, have roughly twice as much work experience, earn more in terms of both gross and hourly earnings, despite quite similar education levels. Renters who moved have a higher probability of job loss (due to plant closure and layoff) than those who did not, whilst homeowners tend to experience job loss less often regardless of their moving status. An interpretation is that some of the rare events of moving might have been motivated by the incidence of involuntary unemployment, subject to whether the affected households could afford to move.

VI. RESULTS

A. Basic results

The basic results split by moving status for my reduced-form regression based on (5) are presented in Table II. When interpreting these results, it is important to keep in mind that the observed mortgage ratio are typically rather small, and the coefficient estimates from the nonlinear models are not the same as the marginal effects which depend on the values of the regressors. The coefficients on the year and state dummies are not reported, although they do play an important part in explaining the observed variations in hours.

Across all models except the naive Tobit for the non-movers, there is a statistically insignificant effect of mortgage ratio on spousal hours of work, although the Heckit first-stage estimate in panel (3) gives tentative evidence that commitments may increase spousal participation. This is expected given that the naive Tobit model assumes a single mechanism underlying both participation and hours, which seems to fail in the data as the Heckit coefficients for a given regressor do not always have identical signs on both margins. In the corresponding F test or χ^2 test, I fail to reject the null hypothesis of joint insignificance of mortgage ratio and unearned income for the FE and TLAD specifications, lending credence to this insignificance result. This absence of a causal link from commitments to spousal hours may seem at odds with Fortin's (1995) finding using Canadian data, yet her cross-sectional analysis is not particularly comparable to mine, for it can neither account for individual fixed effects nor (partially) inform the extent of reverse causality which might bias both of our findings.

To this end, I exploit the panel nature of my dataset and perform some Granger causality-type analysis. Conditional on other controls, contemporaneous and lags of spousal labour supply appear to be weak predictors of both the take-up and the amount of mortgages, either on their own or jointly. Households in which the spouse has stronger labour force attachment do not take on systemically higher mortgage relative to the household income. These results, although not conclusive, provide some reassurance that reverse causality may not be a major concern in my sample. The absence of a theoretially plausible reverse causality relationship might in part reflect the comparatively renter-friendly institutional framework of Germany. Unlike many other developed countries with a large housing sector, Germany has a more extensive social housing policy, higher real-estate transaction taxes and a lack of mortgage interest tax deductibility ever since 1986 (Voigtländer, 2009). These unique features may partly explain why Germany has one of the lowest rates of homeownership and owner-occupation in Europe and beyond, at around 40–45 percent (ECB 2013). Tax breaks, which in 1986 replaced the more valuable general interest tax relief, only applied to first-time buyers, and they were halved and then removed during the first half of the 2000s. Furthermore, the German mortgage market is characterised by a high degree of prudence, as prospective house buyers who require mortgage financing are expected to commit relatively big downpayment. These financial hurdles to homeownership, therefore, may suffice to prevent households with a weaker willingness and ability to buy a property from taking out mortgages. In other words, it is possible that the observations of positive mortgage payments in my German sample mostly consist of wealthier households whose secondary earner would not have to work longer hours to afford the mortgage commitments (i.e. the liquidity constraint).

There appears to be virtually no cross-wage and added worker effects as one examines the coefficient on unearned income Heckit results indicate that unearned income has a statistically significant negative impact on hours and participation. Yet, this cannot be interpreted as evidence for the added worker effect because of the difficulty with signing the cross-wage effects a priori. Furthermore, at the means of the regressors, the marginal effect of per ¤1,000 increase in unearned income is a 0.222 reduction

TABLE I. Sample means by moving and homeownership status

	Movers (N = 755)		Non-movers (N = 5010)	
	Renters	Homeowners	Renters	Homeowners
Household-level variables				
Household size	3.392	3.502	3.244	3.458
Net household income	35,869	48,630	37,460	53,264
Mortgage and interest	374.7	1,168	299.4	1,736
No. of kids aged 0-4	0.407	0.426	0.212	0.149
No. of kids aged 5-18	0.792	0.861	0.722	0.838
State unemployment rate, %	9.542	9.135	9.822	9.042
West Germany dummy	0.801	0.825	0.750	0.790
Spouse-specific variables				
Work experience, yrs	9.740	9.318	12.91	12.81
Age, yrs	39.42	38.94	44.44	46.69
Education, yrs	11.54	12.71	11.64	12.49
Hours of work	851.5	1,043	945.5	1,065
Earnings	9,316	13,194	10,501	14,167
Female dummy	0.835	0.897	0.797	0.855
Job loss dummy	0.0420	0.0205	0.0330	0.0197
Head-specific variables				
Work experience, yrs	17.48	17.79	21.87	24.71
Age, yrs	40.99	41.14	45.79	48.42
Education, yrs	11.84	13.05	11.97	13.06
Earnings	32,636	48,498	33,936	50,241
Job loss dummy	0.0670	0.0114	0.0270	0.0128
Earnings ratio (head/spouse)	3.503	3.676	3.232	3.546
Observations	896	789	12,571	24,535

in hours conditional on working, which is practically negligible especially given that an extra year of education increases hours by 43.5 and the presence of another child aged 0–4 lowers hours by −510. Moreover, once the individual fixed effects are removed using the FE and TLAD estimator, the effect becomes insignificant both statistically and economically. This conclusion is largely consistent with a large body of AWE literature that found no evidence that corroborates the theory (e.g. Heckman and MaCurdy 1980). Yet, my result cannot be interpreted as conclusive evidence that spousal labour supply is an ineffective source of self-insurance against wealth fluctuations – the degree of complementarity or substitutability of leisure and home production times of husbands and wives remains a debate, and considerable attenuation bias may stem from the measurement errors in the income variables used for the construction of mortgage ratio and unearned income.

For the non-movers subsample, the pattern of estimates from the preferred TLAD regression is fairly close to that of the FE regression, suggesting individual time-invariant heterogeneity might be a far bigger concern relative to censoring. Whilst the IMR coefficient is marginally significant (and insignificant for the movers subsample) – meaning sample selection bias cannot be safely ignored and partipation incurs some fixed costs – the test suffers from the low-power problem associated with using an identical set of regressors in both steps of the Heckit model. This finding of only a slight sample selection bias is in accordance with that of many past studies including Fortin (1995).

The covariates broadly have the expected signs. Spousal labour supply rises with the spouse's own education, work experience and age at a diminishing rate. The number of kids, especially younger kids, has a pronounced negative effect on hours and participation. The signs on state unemployment rate are surprisingly positive using the FE and TLAD estimators, but the effects are insignificant. Female spouses work fewer hours than male spouses conditional on working, although the latter may be less likely to participate. An explanation is the self selection of males with a greater distaste for working and a lower labour market attachment, given the scarcity of male secondary earners (16.9% of observations among non-movers).

The lack of a contemporaneous relationship between hours and the variables of interest, however, does not rule out the possiblity of a delayed response. Due to prevalent labour market frictions or longer term labour contracts, for instance, spouses might not be able to increase their hours as flexibly as they would hope to. Therefore, the influence of wealth shocks or liquidity constraints posed by mortgage commitments may take time to show in the actual hours. Hence, adding a generous number of lags of mortgage ratio and unearned income may help pick up some of the delayed response,

TABLE II. Regression results for non-movers and movers, 1999–2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Non-movers					Movers		
VARIABLES	Tobit	Heckit	Heckit 1st stage	FE	Trimmed LAD	Tobit	Heckit	Heckit 1st stage
Mortgage	174.6**	-14.20	0.292**	-113.0	-116.9	-171.7	245.5	-1.017
ratio	(83.45)	(64.59)	(0.123)	(85.14)	(108.6)	(528.5)	(452.2)	(0.820)
Unearned	-0.434***	-0.222**	-0.000649***	0.0687	0.163	-0.526	-0.963*	2.50e-06
income/10^3	(0.133)	(0.111)	(0.000189)	(0.0794)	(0.162)	(0.770)	(0.566)	(0.00142)
Education, yrs	133.8***	43.48**	0.194***	61.57	96.28	281.6**	75.35	0.318
	(26.71)	(22.07)	(0.0385)	(113.5)	(186.1)	(134.2)	(115.3)	(0.210)
Education sqrd	-1.693*	0.419	-0.00389***	-1.983	-3.128	-7.746	-2.254	-0.00771
	(0.962)	(0.752)	(0.00141)	(4.552)	(7.447)	(4.888)	(4.079)	(0.00775)
Work experience,	45.23***	42.33***	0.0276***	86.13***	87.60***	107.6***	67.20***	0.115***
yrs	(2.244)	(1.830)	(0.00326)	(12.09)	(12.40)	(14.08)	(14.95)	(0.0231)
Work experience	-0.0984	-0.281***	6.25e-05	-1.717***	-1.634***	-1.904***	-1.135***	-0.00230***
sqrd	(0.0622)	(0.0481)	(8.95e-05)	(0.242)	(0.241)	(0.457)	(0.419)	(0.000742)
Age, yrs	285.4***	107.2***	0.274***			92.51*	-76.28	0.172**
	(10.98)	(14.45)	(0.0152)			(52.63)	(48.24)	(0.0834)
Age sqrd	-3.717***	-1.497***	-0.00357***			-1.707***	0.599	-0.00265***
	(0.116)	(0.173)	(0.000160)			(0.626)	(0.604)	(0.000989)
No. of kids	-839.3***	-510.3***	-0.850***	-444.6***	-636.1***	-824.8***	-441.3***	-0.835***
aged 0-4	(19.80)	(36.18)	(0.0268)	(24.71)	(33.60)	(66.35)	(99.79)	(0.105)
No. of kids a	-252.0***	-150.9***	-0.250***	-87.30***	-116.1***	-200.8***	-106.8***	-0.193***
ged 5-18	(7.781)	(10.22)	(0.0115)	(13.82)	(16.91)	(41.29)	(37.63)	(0.0648)
Dummy for	-59.43**	-222.1***	0.244***			-152.3	-212.6**	0.00652
female	(23.12)	(18.84)	(0.0364)			(131.8)	(98.54)	(0.271)
State unem-	-15.18**	-2.520	-0.0234**	5.686	9.868	0.182	-60.71**	0.101
ployment, %	(6.140)	(4.493)	(0.00989)	(8.535)	(10.02)	(35.31)	(27.91)	(0.0694)
Head's age	-104.9***	-68.94***	-0.0508***			17.87	-16.42	0.0883
	(11.87)	(9.460)	(0.0168)			(51.48)	(42.55)	(0.0816)
Head's age sqrd	1.093***	0.744***	0.000488***			-0.161	0.183	-0.000943
	(0.121)	(0.0959)	(0.000171)			(0.582)	(0.485)	(0.000921)
Head's education	-34.23	-50.08**	-0.00863	-45.26	-66.57	-120.7	-49.06	-0.139
	(28.31)	(21.09)	(0.0415)	(102.8)	(172.6)	(140.2)	(114.8)	(0.226)
Head's education	1.602	2.053***	0.000476	1.351	1.953	4.148	2.376	0.00414
sqrd	(1.008)	(0.750)	(0.00148)	(3.782)	(6.281)	(5.059)	(4.117)	(0.00824)
IMR			155.1*					-179.6
			(84.40)					(262.9)
Observations	24,535	24,535	24,535	24,535	24,535	789	789	789

Standard errors in parentheses, adjusted to allow for arbitrary forms of heteroskedasticity and serial correlation within households over time.
*** p<0.01, ** p<0.05, * p<0.1

although this will come at the cost of reducing sample size and potentially introducing sample selection bias. Nonetheless, my estimation fails to yield any evidence of a delayed response, with the TLAD estimator running into immense computational difficulties. The following subsection performs two further specification checks.

B. Robustness checks

1. *The global financial crisis as a structural break*

So far I have pooled together observations across a relatively long period, ignoring possible structural breaks and business cycle effects. A candidate for a break in household borrowing and labour supply behaviour is the 2007 financial crisis which swept across the world and caused long-lasting socio-economic distress and institutional changes. Table III presents the regression

results for the period before and after the crisis as a first specification check. Indeed, implementing a Chow test gives a strong rejection of the null that the two sets of coefficients are equal, suggesting the presence of a break around 2007.

Between Heckit and Tobit, the results are now depicting a more consistent pattern after splitting the sample, and it appears the previous out-of-ordinary Tobit estimates which are significant and of an opposite sign to the rest are driven by post-crisis observations. An comparison of the first-stage Heckit results reveals that mortgage ratio might induce previously non-working spouses to participate, although this commitment effect is absent on the intensive margin. This may in part because households that opt for mortgage financing are less vulnerable to unemployment shocks in the first place, in terms of both incidence and duration (Coulson and Fisher, 2009), especially given a relatively prudent German mortgage market. Intuitively, households with non-working spouses are more likely to experience liquidity constraints than households with dual earners, so perhaps the commitment hypothesis is more relevant for the extensive margin. Indeed, existing evidence that lends support to a plausibly causal connection between mortgages and labour supply is mostly concerned with the participation rates.

Despite notable differences in the coefficient estimates from Tobit, Heckit and FE across the two periods, the TLAD results consistently indicate no significant effects of mortgage ratio and unearned income on spousal hours. The TLAD and FE methods again produce identical signs and insignificant t-ratios for both variables, while still exhibiting a big departure from what the Heckit and Tobit estimates suggest. The magnitudes do differ substantially by sample period, and there is now stronger evidence of sample selection bias. Echoing the conclusion from the preceding subsection, this lack of support for the commitment hypotheses might be a result of an interplay between omitted variables such as unemployment benefits and the particular institutional characteristics of a German sample.

2. Age cohort effects

Household wealth reflects past and current savings and hours as well as public and private transfers which are usually contingent on labour market behaviour. Whether unearned income can be treated as exogeneous or predetermined is therefore questionable (Heckman and Killingsworth, 1986). Age might work as a good proxy. All else held equal, older couples tend to accumulate more assets than younger couples which constitute an alternative source of self-insurance available to the couples. Therefore, to moderate the potential bias associated with the inclusion of unearned income, I disaggregate the sample by more narrowly defined age cohorts (20–34, 35–40, 41–50, 51–65).

TABLE III. Regression results for non-movers, 1999–2006 and 2007–2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1999–2006					2007–2014				
VARIABLES	Tobit	Heckit	Heckit 1st stage	FE	TLAD	Tobit	Heckit	Heckit 1st stage	FE	TLAD
Mortgage ratio	-11.54	-50.59	0.0371	-173.1*	-190.5	247.9**	41.34	0.384**	-9.774	-11.72
	(109.3)	(87.39)	(0.146)	(102.0)	(135.5)	(120.1)	(93.73)	(0.190)	(140.3)	(180.2)
Unearned income/10^3	-0.349*	-0.224	-0.000477*	0.0149	0.0479	-0.504***	-0.279*	-0.000815***	0.238	0.355
	(0.183)	(0.161)	(0.000257)	(0.0736)	(0.183)	(0.181)	(0.151)	(0.000262)	(0.174)	(0.261)
IMR			300.9**					304.7***		
			(117.2)					(106.0)		
Observations	12,881	12,881	12,881	12,881	12,881	14,940	14,940	14,940	14,940	14,940

Standard errors in parentheses, adjusted to allow for arbitrary forms of heteroskedasticity and serial correlation within households over time.
*** p<0.01, ** p<0.05, * p<0.1

TABLE IV. TLAD regression results by age cohorts, 1999-2014

VARIABLES	(1) Age 20-34	(2) Age 35-40	(3) Age 41-50	(4) Age 51-65
Mortgage ratio	-68.09	-200.0	-152.2	-216.3
	(593.7)	(454.3)	(176.2)	(158.4)
Unearned income/10^3	-2.860	-0.504	-0.0353	0.416*
	(1.936)	(0.502)	(0.110)	(0.229)
Education, yrs	154.0	717.6	-138.7	-92.39
	(375.2)	(489.5)	(247.7)	(357.5)
Education sqrd	-9.246	-27.83	4.849	5.312
	(14.77)	(18.52)	(10.04)	(14.45)
Work experience, yrs	427.1***	216.7***	106.1***	168.5***
	(107.2)	(70.32)	(25.68)	(27.44)
Work experience sqrd	-22.25***	-8.072***	-2.115***	-1.886***
	(5.217)	(2.167)	(0.565)	(0.418)
No. of kids aged 0-4	-870.5***	-406.0***	-354.4***	-33.66
	(62.91)	(73.54)	(56.22)	(151.8)
No. of kids aged 5-18	-422.9***	-87.77	-8.957	-199.1***
	(89.80)	(55.85)	(20.06)	(56.92)
State unemployment, %	-28.40	-88.29**	-27.30*	4.995
	(55.99)	(40.34)	(16.28)	(24.66)
Head's education	1,073**	-476.0**	-169.7	-216.3
	(424.2)	(203.1)	(218.8)	(514.2)
Head's education sqrd	-38.88**	15.29**	4.929	7.129
	(15.93)	(6.525)	(7.634)	(20.23)
Observations	2,597	3,832	9,781	8,325

Standard errors in parentheses, adjusted to allow for arbitrary forms of heteroskedasticity and serial correlation within households over time.
*** p<0.01, ** p<0.05, * p<0.1

Table IV presents the results from the TLAD estimation, which broadly substantiate my previous findings. Conditional on the background controls, mortgage ratio has a statistically insignificant effect on spousal hours across all age groups. A similar conclusion can be made about unearned income, with the exception of the eldest age cohort for whom it has a weakly significant positive effect on spousal hours. Since early retirees are dropped from my sample, this eldest age cohort consists of individuals who choose to remain in the labour force despite their relatively high level of asset accumulation and low burden arising from the presence of dependents. These individuals might in turn have a greater propensity to work that cannot be explained by their observables and lead to longer hours. It should also be highlighted that as the G-SOEP dataset lacks a reliable measure of savings, home equity and other forms of asset accumulation, my unearned income variable might not actually capture the endogenous stock of assets accumulated over time.

There appear to be substantial variations in the impact of the covariates, which are mostly expected. For instance, the presence of kids has a notably larger influence on the younger cohorts, which is consistent with abundant evidence of a causal link between the timing of fertility and labour market outcomes (Miller, 2011).

VII. CONCLUSION

This paper has devised a test of whether mortgage commitments can amplify the added worker effect by invoking a liquidity constraint onto the household optimisation problem, as predicted by Chetty and Szeidl's consumption commitments model. To deal with censoring and individual fixed effects, I have initially applied the commonly used FE, Tobit and Heckit estimators, each of which only accounts for either source of endogeneity but not both. I have then implemented the preferred alternative, i.e. the trimmed LAD estimator, which can potentially overcome both challenges without introducing unrealistic normality restrictions. Using a rich longitudinal sample of working-age married couples in Germany, I have found little evidence that an increase in the proportion of household income devoted to mortgage commitments can induce spousal labour supply on the intensive margin. This somewhat surprising finding might be reconciled by noting the unusual institutional features of German housing market, which may undermine the external validity of my results.

However, the analysis in this paper is subject to a few limitations. First, my sample excludes divorcees, singletons and cohabiting couples, hence it ignores the effect of mortgage constraints and wealth fluctuations on marital status. Second, my results do not rule out the possibility of a significant participation response that has been documented in the literature. In fact, results from my first-stage Heckit regressions lend credence to these previous findings, though they might simply reflect an omission of individual fixed effects. Third, my reduced-form analysis may not be useful for uncovering the true structural relationship, if the exclusion of mortgages from the wage equation fails to hold. Finally, my results are sensitive to the presence of measurement errors and potential endogeneity in the control set.

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