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SPOUSAL LABOR SUPPLY AS INSURANCE: DOES UNEMPLOYMENT INSURANCE CROWD OUT THE ADDED WORKER EFFECT?

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ABSTRACT

We consider the role of spousal labor supply as insurance against spells of unemployment. Standard theory suggests that women should work more when their husbands are out of work (the "Added Worker Effect" or AWE), but there has been little empirical support for this contention. We too find little evidence of an AWE over the 1984-1993 period. We suggest that one reason for the absence of the AWE may be that unemployment insurance (UI) is providing a state-contingent income stream that counteracts the negative income shock from the husband's unemployment. We in fact find that increases in the generosity of UI lower labor supply among wives of unemployed husbands. Our results suggest that UI is crowding out a sizeable fraction of offsetting spousal earnings in response to unemployment spells, although even in the absence of a UI system the spousal response would only make up a small share of the associated reduction in family income. We also find evidence that families are making labor supply decisions in a life cycle context, since there are effects of UI on the labor supply of wives of employed husbands who face high unemployment risk. Yet, couples do not appear able to smooth the labor supply response to UI income flows equally over periods of employment and unemployment, suggesting the presence of liquidity constraints. Finally, wives in families with small children are more responsive to UI benefits in their labor supply decisions, which is consistent with the notion that they have a higher opportunity cost of market work.

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Government intervention in private insurance markets in the U.S. has grown rapidly over the past 30 years. This rapid growth, along with increased budgetary pressure at both the state and federal level, has raised the question of whether such interventions are necessary. Answering this question requires assessing whether, in the absence of government intervention, private arrangements would emerge to provide insurance against adverse events. One natural private arrangement is insurance through the labor supply of family members. In the face of a negative shock to the earnings of one family member, other family members can increase their labor supply to compensate for the resulting income loss.[1]

There have been a number of studies which have focused specifically on spousal labor supply as insurance against the unemployment of the household head. These studies test for the "added worker effect" (AWE), which is the entry of wives into the labor force when their husbands lose their jobs. Somewhat surprisingly, however, empirical studies of the AWE have generally not found evidence consistent with this contention.

One potential explanation for these findings is that previous studies have ignored the presence of public insurance for unemployed workers through the unemployment insurance (UI) program. UI provides a state contingent income stream which counteracts the income effect from the husband's job loss. As such, it may mitigate the wife's labor supply response, so that these estimates

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[1]There are a number of other private arrangements that may be crowded out by public insurance, but they are not the focus of our study. The source of private insurance that has received the most attention in previous studies is own savings. Both theoretical and empirical research suggests that savings is reduced by the generosity of the social insurance system in the U.S., although the extent of the estimated "crowdout" varies. Hubbard et al. (1995) use a stochastic life-cycle model to demonstrate that government social insurance programs substantially reduce personal savings. For empirical evidence that social insurance programs crowd out personal savings, see Feldstein (1974) for the case of Social Security; Fishback and Kantor (1994) for the case of workers' compensation; and Engen and Gruber (1995) for the case of unemployment insurance. We return in the conclusion to consider the interaction of spousal insurance and wealth accumulation.

understate the potential for wives to insure their family income while their husbands are unemployed.[2] If this is true, it suggests that in the absence of public insurance spouses would insure to a larger extent the income risk from unemployment facing the family.

The purpose of this paper is to re-assess the insurance role played by spousal labor supply, focusing particularly on role of unemployment insurance. For the sake of comparability to previous literature we retain the focus on the response of wives to their husbands' unemployment. While this focus is somewhat anachronistic in light of the increased labor force participation of married women, in 87 percent of married couples the husband earns more, and in 73 percent the husband works more hours (over a two year period) than the wife.[3] Thus, this remains a natural starting point for examining the response of secondary earners.

We do so using the Survey of Income and Program Participation (SIPP), a large nationally representative survey which follows families over a period of 2-3 years. The longitudinal aspect of this data allows us to control for underlying heterogeneity in tastes for work in measuring the AWE, and to follow wives during their husbands' unemployment spells. And we can match to the SIPP information about the UI regime in the state in which a family resides to consider the role that UI plays in determining the magnitude of the measured AWE.

Our analysis proceeds in three steps. First, we briefly revisit the AWE question: do wives work more when their husbands lose their jobs? We consider a fairly rich empirical model which addresses a number of problems with previous empirical work, but we are also unable to document a significant AWE. We then assess whether there is a crowdout role being played by UI. In fact,

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[2]A similar point is made by Topel (1984), and by Viscusi and Moore (1987) and Fishback and Kantor (1995), who show that the availability of unemployment insurance/workers' compensation lowers the compensating wage differentials provided for unemployment risk/injury risk respectively.

[3]Figures tabulated from SIPP data for 1983-1993 described below.

we find that there is quite a large crowdout; our estimates imply that in the absence of UI, wives' total hours of work would rise by 30% during their husbands' spells of unemployment. At the same time, however, we find that this spousal response would make up only a small share (about 14%) of the associated reduction in family income.

Finally, we note that the exogenous variation in income across couples induced by underlying differences in the UI system provides a window onto a number of additional questions about how spousal labor supply decisions are made. For example, is there a response to the contingent income stream from UI during periods of employment as well, as the life cycle model of labor supply would imply? To the extent that there is such a life cycle response to UI, does it indicate that couples are able to perfectly smooth the wife's labor supply through time, or do they appear to face liquidity constraints which induce intertemporal distortions in labor supply allocation? And how does the responsiveness to UI vary with family structure, and in particular the presence of small children?

The paper is divided into six sections. In Part I, we discuss the theoretical motivation for the AWE, and previous work on this question. In Part II, we describe our data and empirical strategy. In Part III, we present our basic AWE estimates. In Part IV, we examine the effect of UI on spousal labor supply. Part V presents the extensions. Part VI concludes.

Part I: Background

Theoretical Considerations

The theory underlying the notion of spousal labor supply as insurance against unemployment is developed in Ashenfelter (1980), Heckman and MaCurdy (1980), and Lundberg (1985). In a simple static model, there is presumed to be an Added Worker Effect because the reduced transitory income of the family will raise the labor supply of the wife (if her leisure is a normal good). This

effect may be enhanced if increased non-market time for the husband lowers the opportunity cost of market work for the wife, through substitution in home production; but the extent of complementarity or substitutability of spousal labor supply remains an unresolved question.[4]

As Heckman and MaCurdy highlight, however, the AWE should be small in the context of a life cycle model, so long as the income loss from unemployment is small relative to the husband's lifetime earnings. In this case, wives of husbands who face higher unemployment risk should increase their earnings at all times, and not just while the husband is unemployed. That is, to the extent that unemployment is a transitory shock, it should not distort the intertemporal allocation of the wife's labor; there will be more market labor supplied by wives whose husbands face greater unemployment risk, and not necessarily wives whose husbands are unemployed at a point in time.

There are at least three reasons, however, why the event of unemployment per se might be expected to cause wives to work more. First, as noted above, the leisure of wives and husbands may be substitutable through household production. Second, as noted by Mincer (1962) and Lundberg (1985), if families are liquidity constrained or face fixed consumption commitments, they may not have the resources to smooth consumption over the husband's unemployment spell, so that wives will have to work more during the spell. Finally, as Dynarski and Shefrin (1987) show in their analysis of unemployment and consumption, even in a life cycle model with perfect capital markets there will be a response of consumption to both job loss and reemployment if these events convey information about lifetime prospects. This same intuition applies to spousal labor supply as well.

Thus, the previous literature suggests that there may be both a life cycle response to unemployment risk, as well as a differential increase in labor supply during the spell of

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[4]Knieser (1976) suggests that the labor supply of spouses is complementary for older couples, but there is no more recent evidence of which we are aware.

unemployment.[5] But none of these studies have considered the role of UI. By providing a state-contingent income stream, UI mitigates the income loss from unemployment. This will have effects on both decisions over the life cycle and decisions during the unemployment spell as well. We focus initially below on the effect of UI during the spell of unemployment; in the extensions, we consider life cycle effects outside of the spell.

Previous Evidence

Most empirical work on the Added Worker Effect focuses on the implications of this response for aggregate unemployment dynamics.[6] A smaller literature has attempted to estimate the specific labor supply response of wives to their husband's unemployment.[7] This literature has faced two methodological problems, and has dealt with them with mixed success. The first is that there may be underlying differences in the taste for work between wives of men who become unemployed and wives of men who do not. The bias imparted by this heterogeneity is not obvious, but if there is "assortative mating" in tastes for work, then it will bias against finding an AWE in a cross-section of data. The second is the "discouraged worker effect" (DWE): if the husband's unemployment is

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[5]One interesting question is why wives go to work at all, as opposed to the husband simply taking a new job immediately. The answer implicit in the previous literature, and in our paper, is that there is some required period of search by the husband for a new job match, and that there may be a transitory shortfall in family income during that search process, requiring market work by the wife. We do not model the husband's search behavior in this paper; we simply condition on a separation, and not on the behavior during the resulting spell of unemployment (ie. duration of the spell). We do this because search behavior among the unemployed has been shown repeatedly to be a function of UI generosity (for example, see Meyer, 1990), so that it would be an endogenous regressor in our models of spousal labor supply.

[6]See, for example, Cain (1966), Mincer (1966), and Wachter (1974).

[7]No study of which we are aware considers the response of husbands to wives labor supply, although a number of papers do model joint family labor supply more generally; see Killingsworth and Heckman (1986) for a review.

due to a general economic downturn, then the wife's shadow wage may be falling as well, making her less likely to work.[8]

Previous work has generally been unable to document a significant AWE. Heckman and MaCurdy (1980,1982) use a life cycle model of labor supply with couple fixed effects that control for heterogeneity. They initially find no response of wives to their husbands' unemployment; this conclusion is reversed by their 1982 revision, which finds a negative (but insignificant) effect of income and a significant positive effect of the husband's hours of unemployment, both of which are consistent with an AWE. Layard et al. (1980) perform a cross-sectional analysis for the UK, and find that wives' labor supply actually falls when the husband becomes unemployed. They conjecture that this may be due to the means-tested welfare entitlement program in the U.K., which resulted in a high tax rate on spousal labor supply during spells of unemployment by the husband. Maloney (1987) pursues a similar cross-sectional approach with U.S. data, and also finds no evidence for an AWE. Lundberg (1985) uses a quasi-difference framework, modelling the wife's transition to and from states of non-participation, employment, and unemployment as a function of the employment status of their husbands. She does find evidence of a small AWE for whites, but she finds the opposite effect for blacks. Most recently, Maloney (1991) uses a selection model to try to capture heterogeneity in tastes for work, and he concludes that there remains no evidence of an AWE.

The mixed conclusions from these studies may reflect their differential success in dealing with the problems noted above. Moreover, none of these studies has been carried out on data from more recently than 1982 (and only one since 1976); the dramatic shift in female labor force participation

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[8]The aggregate literature on the AWE has generally concluded that the discouraged worker effect dominates the added worker effect in economic downturns. For an attempt to distinguish between the added and discouraged worker effects using aggregated micro-data across areas, see Mitchell (1979).

since then may substantially change any earlier conclusion. Finally, as noted, none of this studies has considered the role of UI. We address these issues in our empirical work.

Part II: Data and Empirical Strategy for Estimating the AWE

Data

We use the 1984-1988 and 1990-1992 panels of the Survey of Income and Program Participation (SIPP). The SIPP is a nationally representative survey which collects information from a large sample of households every four months (waves) over a period of two to two-and-one-half years. The interviews that we use therefore span the period from the middle of 1983 to the end of 1993. At each interview, households are asked questions about the entire previous four month period as well as each month in that period. Data are collected on the demographic and economic characteristics of each household member and of the household as a whole.

A concern with our analysis is the potential endogeneity of the husband's labor supply to decisions made by the wife, such as through joint retirement behavior. We attempt to mitigate this problem in three ways. First, we restrict our analysis to couples where both the husband and wife are between the ages of 25 and 54.[9] Second, we also restrict our sample to months after we observe at least three months of employment by the husband. Husbands in this age range who have worked for at least three consecutive months are likely to be very strongly attached to the labor force, and therefore it is unlikely that they are leaving their jobs because of work decisions by their wives.[10]

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[9]We choose only couples that are married for the duration of the SIPP panel, thereby ignoring the endogeneity of marital status to either the unemployment of the husband or the generosity of UI. These are worthy topics for future analysis.

[10]This left-censoring restriction only excludes 9% of the observations in this age range. Note that these excluded months include all months for persons who never work in three consecutive months, as well as the initial months for persons whose first three months of work come at some

Furthermore, this information on past earnings histories is necessary to assign potential UI benefits to the husband. Lastly, we restrict our analysis of unemployment to spells where the husband is looking for work in at least some months, so that we are not capturing early retirement or other endogenous labor force leaving.

We also exclude couples where the husband is always self-employed, since he cannot avail himself of the UI system. And we exclude outlying observations with hourly wages below $1/hour and above $100/hour, and monthly hours of more than 400. Finally, we consider couples who are in the SIPP for at least two years, in order to have sufficient information to identify the fixed effects models that we estimate, and to consider longer run effects of UI.

Regression Framework

Our basic regression framework for analyzing the AWE is:

$$(1) \qquad LS_{it} = \alpha + \beta_1 X_{it} + \beta_2 UNEM_{it} + \beta_3 \delta_j + \beta_4 \tau_p + \epsilon_{it}$$

Our unit of observation is a spell of employment or unemployment (t) for a person (i). That is, all consecutive months of employment or unemployment are collapsed into a single observation, and our dependent variable is average spousal labor supply during the spell; so for each person there is t observations corresponding to their t spells of employment or unemployment.

This spell-based approach is important for two reasons. First, there is likely to be significant

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point during the SIPP panel. We also attempted to deal with endogeneity through an instrumental variables strategy, using as instruments the unemployment rate for the husband's occupation and industry in the local area, in a model which controlled for female-specific local labor market conditions. This approach was not successful, however; it yielded implausibly large negative coefficients on the husband's unemployment, indicating that, even conditioning on measures of the female-specific labor market conditions (as we do below), the labor market conditions facing the husband are correlated with the wife's job opportunities.

correlation in the behavior of couples during a given spell, so that it would be inappropriate to treat monthly observations on labor supply during that spell as independent. Moreover, for the analysis of UI, our independent variable of interest, UI benefits, is constant throughout the spell, so that we really only have variation across spells and not within them. Second, by using one observation per spell, we do not "overweight" long spells in our analysis of UI. There is a large literature which strongly suggests that the duration of unemployment spells is a function of UI generosity (ie. Meyer, 1990), so that using data on each month of unemployment could lead to selection bias since we would have more months of observations from cases where UI is disproportionately generous. By the same logic, we do not want to overweight short spells in our regressions, by having more observations for individuals who flow in and out of employment; we therefore weight each observation by the reciprocal of the number of spells for that person, so that each person's weights sum to one.[11]

We use two measures of the labor supply of wives (LS) from couple i as dependent variables: employment and the number of hours worked per month. These correspond to the measures of labor force attachment used in many previous studies of the AWE. Since we are aggregating over spells of employment and unemployment, our dependent variables are the share of months employed and average hours/month during the spell.

Our key independent variable is a dummy for whether the husband is currently unemployed.

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[11]This is done within the sample being used. That is, when we use both spells of employment and unemployment for the AWE analysis, the weight is one over the total number of spells. When we use only spells of unemployment for the UI analysis, the weight is one over the number of unemployment spells. An alternative, which deals with the intra-spell correlation but not with the potential bias from endogenous duration, is to simply pursue the analysis using monthly observations on labor supply, and to correct the standard errors for the correlation within spells. Doing so yields results which are similar to those reported below, albeit with somewhat smaller (although still significant) estimated effects of UI.

We define spells of unemployment as beginning with the first full month that a husband is without a job, and ending with the first full month that the husband is working and with a job, and as containing at least one month where the husband is looking for work.[12] In doing so, we deviate from the usual definition of unemployment by excluding any spells which include months where the husband is with a job but is on layoff (temporary layoffs). We do so both because wives may respond differently to temporary and permanent layoffs, and because this type of unemployment has been shown to be particularly responsive to UI generosity (Feldstein, 1978; Topel, 1983), so that including temporary layoffs might make the interpretation of our UI results more problematic. Our basic results are similar when we include those spells that are temporary layoffs.

We also focus on all job leavers, and not just job losers. This is because there is no reliable measure of reason for separation in the SIPP. This is unfortunate, because the AWE discussion refers to job loss, not quits. This issue is addressed to some extent by our exclusion of spells that do not result in job search. It is also addressed in our UI analysis by our use of actual UI benefits received in some models, since only job losers can receive UI; thus, in these models, we identify the impact on the population of interest, job losers.

X is a set of demographic covariates: the age and education of the husband and the wife; the race of the wife; and the number of children of ages 0-1, 2-5, and 6-18. We also control for a number of (lagged) characteristics of the husband's job: a 14 piece spline in his quarterly earnings,

[12]We exclude months where the husband is unemployed for part of the month from the analysis, since it would be difficult to appropriately scale the spousal labor supply response for the share of the month unemployed, and since many of the spells lasting less than one month may be false transitions in the SIPP data.

industry, and occupation.[13] The lagged wage will be an important control for the determinants of UI benefits, and all three factors help to control for heterogeneity across husbands who do and do not leave their jobs. The control variables take on their lagged value from the three months before a given (employment or unemployment) spell begins, and are held constant for the duration of the spell.[14] Finally, we include a full set of controls for state of residence (δ_j) and calendar time (we have dummies for each year period, denoted by τ_p).

Our hours of work models include both zero and positive hours, since our goal is to measure the total hours response. We therefore estimate the models as Tobits, to account for the bunching of observations at zero. For measuring the spousal response, however, we are only concerned with the response of observed hours, and not the "latent" hours decision that is modelled by the Tobit; we therefore report both Tobit coefficients, and the implied effect on observed hours of work.[15] It is also of interest to separate the effect of unemployment on the wife's decision to work and the

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[13]The spline points are at the 1st, 5th, 10th, 20th, ..., 90th, 95th, and 99th percentiles of the earnings distribution for the relevant sample under analysis. We deviate somewhat from previous labor literature by not including a predicted wage for the wife. We do so because we include in our reduced form equation all of the variables that we would use to form such a prediction, so that the predicted wage itself would be identified solely from functional form assumptions on the prediction equation. When we include a predicted wage as well, there is no effect on our basic results.

[14]The one exception to this approach is the wage over spells of employment. In some models, we will regress the wife's average work effort during spells of employment on average UI benefits available during spells of employment. As such, it is important to control for average wages during these spells, since (as we discuss in more detail below) wages are an important determinant of both UI benefits and spousal labor supply. Thus, in these cases, we control for a spline in average lagged wage during the spell, as opposed to lagged wage at the start of the spell.

[15]The Tobit coefficient corresponds to the latent index model, and therefore measures the marginal impact on desired hours. The implied effect on observed hours involves a transformation of this coefficient, as described by McDonald and Moffitt (1980). The implied effect is the sum of the effect on the participation and hours of work decisions: the change in the likelihood of working positive hours, times average hours among those who work; and the change in hours among those working, times the likelihood of working.

amount of hours conditional on work. We therefore estimate both models of employment, by OLS,[16] and of conditional hours of work, using a "Heckit"-type sample selection model (Heckman, 1979) to account for selection into positive hours. We have no excluded instruments that separately identify selection, however, so that this model is identified solely from functional form assumptions.

In Part I, we noted two problems with inferring the added worker effect from equations such as (1). The first is differential tastes for work among the wives whose husbands become unemployed, relative to those wives whose husbands stay employed. We have tried to correct for observable heterogeneity by including a number of characteristics of the couple, and of the husband's job. An alternative, which deals with heterogeneity in tastes that is fixed over the length of the SIPP panel, is to include couple-specific fixed effects in our models. This fixed effects model can be readily estimated using the longitudinal data available in the SIPP.

The second is the discouraged worker effect. Previous attempts to control for the DWE have generally relied on average area-specific unemployment rates. These types of controls may be particularly problematic if women face different job opportunities than men, since variations in the overall unemployment rate will be dominated by changes in male job opportunities.[17] We therefore include two indicators of labor market conditions that are specific to wives: the unemployment rate and the average wage of women with the wife's education level in that state at that point in time. These measures are computed from the Current Population Survey's Merged Outgoing Rotation Group data, the largest monthly employment micro-data set available, which allows us to precisely

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[16]We have also repeated the employment analysis using a dummy for any work during the spell, and estimating a probit model; the AWE results are very similar and the UI effects are identical.

[17]This is not a problem with the studies of Layard et al. (1980) and Lundberg (1985), who use area-specific fixed effects. We have estimated models which include SMSA fixed effects, as well as our state fixed effects, with little effect on the results.

measure these indicators at a four month frequency.

Part III: AWE Results

Means

The means of our dataset are presented in Table 1. In the first column, we present the means for all of the spells in our sample. In the second and third we divide these into employment spells and unemployment spells. Finally, in the fourth and fifth columns we present information from the first three months of the sample separately for couples where the husband will and will not become unemployed at some point during the SIPP panel. On average, wives work in 63% of the months in the SIPP data. Including those who work zero hours, the average wife works slightly under 100 hours per month. Among those that work, the average amount of work per month is 131 hours; average earnings/hour is $7.24. The next two columns offer only mixed support for an AWE. Wives are actually less likely to work when their husbands are unemployed, but conditional on working they work more hours; overall, there is only a small change in hours.

However, the remainder of the table also suggests the potential importance of heterogeneity. Wives of husbands who are unemployed are less educated; husbands who are unemployed also have much lower earnings prior to becoming unemployed. This is further illustrated in the final two columns of the table. Even in their first three months in the SIPP, when all husbands are employed, there is a lower likelihood of employment and higher hours conditional on working for wives whose husbands are going to become unemployed (but are not yet unemployed). Moreover, there is also a potentially important role for the DWE here as well. The average female wage is lower, and female unemployment is higher, in the places where wives of unemployed husbands live. This is true both at the point in time when the husbands are unemployed (comparing columns (2) and (3)),

and before the unemployment has occurred (comparing columns (4) and (5)).

Regression Estimates

Our results from estimating model (1) are presented in Table 2. In the first three columns, we do not control for heterogeneity with fixed effects. The first column reports the Tobit coefficient, with the implied effect on observed hours in square brackets. The estimates are quite similar to what emerged from the table of means: there is actually a negative effect of unemployment on average hours of work and on the likelihood of work, but neither estimate is significant. There is a positive effect on hours of work conditional on working, but the estimated effect is small; the rise of 8.22 hours is only 5.5% of the baseline hours of work among women who are working while their husbands are unemployed.

The control variables generally have the expected signs. Wives of higher earning husbands are less likely to work; the implied income effect at the mean husband earnings is -0.14 for the total hours equation (and -0.11 for the Heckit), which is somewhat larger than the estimates in Mroz (1987). Labor supply rises with the education of the wife, but is non-linear with respect to the education of the husband; wives of husbands with low or high education work fewer hours relative to wives of husbands with average years of schooling. Employment and hours of work also much lower for those women with children, particularly young children. The controls for average wages is positive and significant; the unemployment control is negative, but it is only marginally significant in the employment equation and insignificant elsewhere.

In the next two columns, we move to control for time invariant heterogeneity across couples in our sample by including fixed effects. In this case, we estimate our total hours equation by OLS; we do not estimate Heckit models here because of the inconsistency of the first step probit with fixed

effects.[18] The fixed effects have little effect on the estimates, suggesting that our wrong-signed findings were not due to heterogeneity in tastes for work.

Our results for the AWE are therefore in line with the mixed findings of the previous literature: there is little evidence of a response in wife's labor supply to the unemployment of the husband. This may be due to insufficient controls for labor market conditions, small income effects, or even complementary leisure across spouses. Alternatively, it may be that UI is crowding out a potential spousal response. We turn next to investigating this hypothesis.

Part IV: The Role of Unemployment Insurance

Empirical Strategy

In order to investigate the crowdout role of UI, we restrict our analysis to spells of unemployment. We consider separately below the potential effects of UI on spells of employment.[19] We run regressions of the form:

$$(2) \qquad LS_{it} = \alpha + \beta_1 X_{it} + \beta_2 UI_{it} + \beta_3 \delta_j + \beta_4 \tau_p + \epsilon_i$$

where UI_{it} is the amount of unemployment insurance benefits for which the husband of couple i is eligible during unemployment spell t. If UI is crowding out any spousal response, then we would expect the coefficient $\beta_2 < 0$.

Our key regressor is the unemployment insurance benefits for which the individual is

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[18]OLS estimation of the total hours model without fixed effects yielded results very similar to the effect on observed hours from the tobit.

[19]An alternative here would be to include both spells of unemployment and employment, as in (1), and to interact the effect of UI with the unemployment indicator. But this approach would impose a common set of coefficients on the control variables (including, most importantly, the wage spline), which may be inappropriate for these quite different samples. Given that we are able to obtain reasonably precise estimates separately for the unemployed and employed spells, we pursue the more robust approach of estimating equations for these spells separately.

potentially eligible. To create this variable, we have built a simulation program which models each state's UI system for the period 1983-1993. The basis for this program is Employment and Training Administration (various years), which reports semi-annual information on state benefit schedules; in addition, it was augmented by information from a number of states and from Levine (1990). UI benefits are calculated as a function of the highest earnings quarter (called the "high quarter wage", or HQW) in the "base period" (generally the first four of the five quarters preceding the unemployment spell). Calculating benefits appropriately, therefore, requires at least five quarters of wage history, which wasn't available for most of our sample. Since we have at least one quarter of wage information, we use husband's earnings in the quarter before his unemployment spell began as the HQW, and four times that amount as the base period value. For the individuals in our sample for more than five quarters, for whom we can compute a true high-quarter wage, the correlation between the UI benefits using the true HQW wage and the UI benefits calculated under our simplifying assumption is 0.90.[20]

There are two potential concerns with estimating equations such as (2). First, the UI benefit for a given individual is a function of characteristics that might otherwise be correlated with the wife's labor supply. For example, UI replacement rates fall with the husband's wage (due to the progressive nature of state benefit schedules), and the wife's labor supply may fall as the husband's wages rise as well. In order to control for this, we include the aforementioned 14-piece spline in

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[20]In addition, individuals must have some minimum earnings during the base period, and in some states a certain distribution of earnings (ie. a certain amount earned outside the high quarter), to qualify for benefits. We apply the state specific rules for minimum earnings amounts to our estimated base period earnings, but we ignore the distribution rules. This results in 10% of the sample being coded as ineligible. Once again, for those who are in the survey for more than 5 quarters, we can calculate eligibility exactly; the correlation between the two eligibility measures is 0.88. UI benefits were also tax subsidized before 1987 for low income couples (those with family income below $18,000); we do not model this subsidy since family income is obviously endogenous to the labor supply decision of the spouse.

the husband's quarterly wage to pick up omitted effects of income on spousal labor supply.[21]

Second, the unemployment of the husband may not be exogenous to the generosity of the UI regime in which the family resides, either through job leaving or unemployment duration. Feldstein (1978) and Topel (1983) find that the probability that an individual is laid off is a function of the replacement rate, although Anderson and Meyer (1994) find an inconsistent relationship between layoffs and benefits. This is potentially problematic for our approach, if the wives of the men who are laid off when replacement rates rise have different tastes for work on the margin.[22] To minimize this problem, we do not include temporary layoffs in our sample of unemployed, since these are found to be the type of unemployment that is sensitive to UI benefit levels in the previous studies. Given our sample restrictions, we do not find a significant correlation between UI benefits and the likelihood that a husband has a spell of unemployment during the SIPP panel, which rules out this source of sample selection as an explanation for our findings.[23]

Finally, there is an important issue of interpretation of these results. Since our measure of UI is potential benefits, the estimated parameter β_2 measures the effect of making a husband eligible for one more dollar of UI benefits on the labor supply of his wife. This is not the same as the husband receiving one more dollar of unemployment insurance. While our sample of husbands are

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[21]Since we also include dummies for state of residence and calendar year, our model is identified only from higher order interactions of wage, state, and time, which are presumably legitimately excluded from the female labor supply equation. We present a specification check which supports this assumption below.

[22]That is, suppose that as UI rises the men who leave their jobs have especially motivated wives. Then the average labor supply response to the husband's unemployment will appear to be larger where UI is more generous, even if there is no legitimate AWE.

[23]More precisely, we ran a probit regression on all couples where the dependent variable was a dummy for the husband having a spell of unemployment during the SIPP, and the independent variables are as in (2). The estimated coefficient on unemployment benefits was wrong-signed (negative) and insignificant.

all monetarily eligible for UI (they have sufficiently high past earnings), they may not meet the non-monetary eligibility requirements (they may have quit their previous jobs). Moreover, even among the population of men which is eligible for UI, takeup of these benefits is much less than full, as has been documented by Blank and Card (1991), Anderson and Meyer (1994), McCall (1995), and others. Estimated takeup rates for UI in the literature vary. For our SIPP sample of (UI eligible) unemployed husbands, 56% of the spells of unemployment are associated with UI receipt.

On the other hand, this receipt question cannot be answered by a direct regression of spousal labor supply on UI benefits received. The studies cited above all consistently find that takeup of UI is endogenous to the level of UI generosity, leading to a potential sample selection bias to the estimates. Furthermore, there is likely to be considerable measurement error in the SIPP data on UI benefits received.

There is a natural instrumental variable for benefits received, however: potential benefits. Potential benefits are clearly correlated with benefits received, and the working hypothesis of this paper is that potential benefits are uncorrelated with spousal labor supply other than through the effects of the UI system. In addition, while potential benefits are noisily measured as well, so long as the measurement error in our imputation is independent of the measurement error in UI benefits received, instrumenting will correct the measurement error problem.

Thus, we can estimate a system of equations of the form:

$$(3) \qquad BEN_i = \alpha + \pi_1 X_i + \pi_2 UI_i + \pi_3 \delta_j + \pi_4 \tau_p + \epsilon_i$$

$$LS_i = \alpha + \sigma_1 X_i + \sigma_2 BEN_i + \sigma_3 \delta_j + \sigma_4 \tau_p + \eta_i$$

where BEN is the amount of unemployment insurance received by the husband in couple i and the other variables are defined above. Estimating this system by Two-Stage Least Squares (2SLS) yields the parameter coefficient of interest, σ_2, which is the effect of receiving another dollar of UI on

spousal labor supply.

Both the reduced form parameter from equation (2) (β_2) and the 2SLS parameter from equation (3) (σ_2) are of interest. The former is most directly policy relevant, as argued by Gruber (1994). Government policy-makers cannot directly control UI receipt, but they can control the level of potential benefits. So β_2 measures the relationship of direct policy interest, which is the effect of raising UI generosity on the spousal labor supply of the potentially eligible population. On the other hand, σ_2 is a measure of the structural parameter of interest for measuring how spousal labor supply responds directly to income received. In addition, σ_2 measures the effect on job losers only (since only they can receive UI), mitigating the problem from having some quitters in our sample.

An important limitation of the 2SLS estimate σ_2, however, is that it might overstate the effect of UI receipt on spousal labor supply. This is because there may be some "option value" of the program even for those couples where the husband does not take up benefits. Suppose that there is some fixed cost to taking up UI benefits; an example here would be stigma associated with receipt (see Moffitt, 1983, for a discussion in the context of cash welfare). This is consistent with there being less than full takeup among those eligible for the program. Consider now a couple where the husband is newly unemployed and would prefer not to take up stigmatizing UI, but where there is uncertainty about the prospects for the husband's reemployment. This couple may delay takeup until it appears that the husband will be jobless for a reasonably long period of time. But in the meantime they will account for the presence of insurance through UI in their choices, including the choice of labor supply by the wife; that is, the wife will work less initially than she would if this contingent option were not available, since she knows that the family can avail themselves of UI if necessary. In this case, σ_1 will overstate the effect of UI on those actually taking up benefits, since the instrument is correlated with the labor supply decisions of those not taking up, and thus negatively

correlated with the error term in the 2SLS equation.[24]

In theory, this problem could be surmounted with a second instrument which could be used to model selection into takeup of UI. We were unable to find such an instrument, however. It is worth noting that this is a general problem with the literature on UI and other social insurance programs; analysts either estimate the reduced form or 2SLS coefficients, but are unable to measure precisely the option value of these programs on those who do not take them up.

Results

The basic results of our UI reduced form regressions are presented in Table 3. We present the coefficient of interest from regressions that include all of the covariates shown in Table 2 (and discussed in the footnote to that table). Our sample consists of one observation for each of the 2560 spells of unemployment for which the husband is eligible for UI. 29% of our sample is repeat spells; correcting the standard errors for multiple observations on the same individual had little effect. In interpreting these results, it is important to keep in mind that we are measuring average hours of work per month, while potential (and actual) UI benefits are measured per week. Thus, the coefficients must be divided by 4.3, the average weeks per month in our SIPP sample, for interpretation.

As the first panel shows, there is a sizeable and significant negative effect of increases in UI

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[24]More specifically, our goal here is to measure the effect of the treatment (UI) on the treated (recipients). However, we cannot do so, because there is an effect on the non-treated (potential recipients who do not take up) as well. Our IV approach is essentially comparing the labor supply of wives in more and less generous UI systems, relative to the amount of benefits actually received. Both the true labor supply response to UI receipt and the option value among non-recipients will cause labor supply to be lower for those groups with higher UI benefits. The IV estimate, however, attributes the difference between the groups solely to receipt, thereby overestimating the direct impact of receipt.

generosity on spousal labor supply. For each $100 in benefits per week ($430 per month), wives work 23.7 fewer hours per month (using the implied effect on observed hours from the Tobit equation). The results also suggest that UI crowds out spousal labor supply both along the employment and the hours of work margins. The employment coefficient implies that $100 in UI benefits per week would lower the likelihood that wives work by 12.7%. And the Heckit coefficient indicates that conditional on work wives work 15 fewer hours per month for each $100 in benefits per week, although this estimate is not significant.

One means of assessing the total crowdout effect of the UI program is to measure our predictions for spousal labor supply in the absence of UI. Among the unemployed, the average wife works 97 hours per month; this figure differs slightly from Table 1 since our sample here excludes those ineligible for UI. Our Tobit model of total hours of work predicts that in the absence of UI she would work 127 hours per month. That is, hours of work would be roughly 30% higher during the husband's spell of unemployment if there were no UI benefits. Similarly, the non-employment rate of wives with unemployed husbands would drop by almost 45% if UI benefits were set to zero. Thus, it appears that the presence of UI is a major reason for the lack of an added worker response that was measured in Part III.

An alternative means of interpreting this finding is to ask how much of the loss in the husband's earnings would be made up by increased spousal labor supply if there were no UI. We can do so by assuming that wives who go to work or increase their hours earn the average hourly wage rate for current working wives of unemployed husbands. Under this assumption, our total hours estimate implies that wives would make up 14% of the husband's lost earnings. While much larger than the zero effect that is implied by our AWE regressions, this figure is still relatively small. That is, while UI is crowding out a substantial AWE, given that UI replaces only a share of the

workers income and that wives earn much less than their husbands, the associated increase in spousal labor supply from zeroing out UI benefits only replaces a small part of the lost family income.

As noted above, from a behavioral perspective it is of more interest to examine the response of wives as a function of the UI benefits received by their husbands. An upper bound on this response can be estimated by the 2SLS system (3). These results are presented in the next panel of Table 3. They indicate that for each dollar of UI received per week, wives are work 0.45 fewer hours per month, and they are 0.24% less likely to work.

A natural means of interpreting these findings is in terms of the direct financial crowdout of UI. That is, for each dollar received by the family, how much less does the wife earn? Once again making our average earnings assumption, our total hours estimates show that wives earn 68 cents less for each dollar of UI received.[25] Recall that this is an upper bound on the extent of crowdout, due to the option value argument described above; the true amount of crowdout lies between 36 cents (from our reduced form estimate) and 68 cents. In either case, the results once again suggest a critical crowd out role for UI.

This estimate also implies quite a large income effect for spouses of unemployed husbands. We find that for each dollar of income per week provided by the UI system, wives work 0.24 fewer hours per month; for each dollar of income received, they work 0.45 fewer hours. At the average non-wife monthly income during unemployment spells ($938.60), the implied income elasticity of labor supply for wives is between -0.53 (lower bound from reduced form) and -1 (upper bound from 2SLS estimate). Both of these estimates are much larger than the income effects estimated in Mroz's (1987) careful study of female labor supply, and the income effects implied by the coefficients on

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[25]One means of assessing the validity of our average earnings assumption is to use earnings per month in place of hours per month as our dependent variable. Doing so yields a implied crowdout of 65%, quite close to our implied crowdout using the average earnings assumption.

husband's income in our AWE regressions; they are at the upper end of the range of elasticities surveyed by Killingsworth and Heckman (1986). This may reflect the fact that couples are liquidity constrained during periods of unemployment, a point to which we return below.

Specification Check: Effects on Wives of Employed Husbands

Our results thus far have assumed that, conditional on our controls and sample selection criteria, we are measuring a causal effect of UI on spousal labor supply through changes in the income of the husband. This may not be true for at least two reasons. First, UI benefit differences may not be exogenous to the labor supply of wives, even after conditioning on our rich set of controls for individual characteristics and labor market conditions. For example, it is possible that UI benefits setting responds to changes in the taste for work within states over time in a manner that is not captured by our control set. Second, own potential UI benefits may have a direct effect on the labor supply of wives, beyond the indirect effect through their husbands' benefits. For example, if own UI generosity increases do induce job separations for wives, then there would be a spurious (from our perspective) negative correlation between UI benefits and spousal labor supply. An alternative view, promoted by Mortensen (1977), is that there is a UI "entitlement" effect, whereby increases in UI generosity increase the labor supply of workers who want to qualify for this generous benefit; this would bias against our findings.[26]

Both of these arguments suggest that our estimated effect may be biased by omitted variables. But one way to test for their importance is to examine the effect of UI on a control group for whom these spurious effects will operate, but our true effects will not: wives of employed husbands. Since

[26]This view, of course, presumes that compensating wage differentials do not offset any increases in potential UI benefits from increased work effort.

these husbands are not receiving UI, there should be no income effect on their wives.[27] But, if UI is correlated with spousal labor supply through these other (spurious) channels, then there will be an important effect on these wives' work decisions.

Table 4 reestimates our equations for spells of employment of the husband. As in Table 3 (and for Table 5 as well), we only report the coefficients of interest from regressions that include all of the other regressors in Table 2. The results indicate that there is only a small effect of UI on the wives of husbands who remain employed: each $100 in UI benefits per week raises hours of work by 4.6 per month (the implied effect on observed hours from the Tobit estimate, which is in square brackets). The effect on propensity to work is insignificant. These effects are much smaller than the comparable effects on the unemployed, implying that our results for the unemployed do not arise simply through some source of spurious identification.[28]

[27]Except, of course, through life cycle effects of UI outside of the unemployment spell. We return to this point below.

[28]The estimates for total hours and employment are statistically significantly different across the employed and unemployed spells samples.

Part V: Extensions and Implications

The results thus far have demonstrated that UI is crowding out spousal labor supply responses to the unemployment of their husbands, and that this crowdout is substantial. But the natural variation in income during unemployment spells that is provided by the UI system offers an opportunity for learning much more about family labor supply decision-making. In this section, we explore a number of additional questions about how spousal labor supply decisions are made.

Life Cycle Effects of UI

Even though the effect of UI on wives of employed husbands is much smaller than the effect on wives of unemployed husbands, the fact that there is even a small effect may reflect an additional phenomena: life cycle effects of UI. In a life cycle model, the possibility of receiving UI during unemployment spells should influence the wife's labor supply over all periods, not just periods of unemployment. This is because, as highlighted by the model of Heckman and MaCurdy (1980), UI is only transitory income, and thereby should not effect the intertemporal allocation of labor supply. This would suggest a small effect on average during spells of employment, which is exactly what is found in the first panel of Table 4. But this finding cannot distinguish life cycle effects of UI from some effect through the alternative (spurious) channels described above.

The presence of a life cycle response to UI can be assessed, however, by using information on the <u>likelihood</u> that husbands become unemployed. If individuals have a higher likelihood of collecting UI, then there will be a larger income effect for that couple from increases in UI generosity. As such, in a life cycle model, more generous UI will induce less work at all times for wives of husbands with the greatest risk of unemployment. On the other hand, for couples where the husband has only a low risk of unemployment, more generous UI will have little effect on their

life cycle labor supply decisionmaking.

In the next panel of Table 4, we therefore split the sample of employed months based on the predicted likelihood that the husband has a spell of unemployment. We first estimate a regression model of the likelihood that a husband has a spell of unemployment over the duration of the SIPP panel as a function of his initial job characteristics.[29] We then apply these coefficients to our sample of employed spells to form a predicted risk measure. We then split this employed sample into those who are above the average predicted risk and below the average predicted risk, and run separate regressions of the form of equation (2) within each subsample.

The results are reported in the next panel of Table 4. In all three columns, the point estimates are consistent with there being a life cycle response to UI. The effect of UI on those below the average predicted risk level is small and insignificant, while the effect on those above this level is larger; the high risk coefficient is significant for total hours and the hours Heckit, although in no case is the difference between the high and low risk coefficients significant. Thus, there is evidence that couples are responding to UI generosity not only during the unemployment of husbands, but over spells of employment as well.

How Well Do Couples Smooth Spousal Labor Supply?

The results thus far suggest that there are effects of the UI system both during and outside of spells of unemployment. But the relative magnitudes of the coefficients suggest that at least some couples are unable to perfectly smooth spousal labor supply over the life cycle, perhaps due to liquidity constraints. That is, in a life cycle model with perfect capital markets, there would be an

[29]The regression model includes the usual set of covariates from our AWE regressions, along with interactions of our industry and occupation dummies to increase the precision of our prediction. Our second stage regression then includes these interactions as well.

equal effect of UI during spells of employment and unemployment. There may still be an AWE due to substitutability of leisure, but the transitory income provided by UI should have no impact on the intertemporal allocation of labor supply.[30] If families are otherwise unable to smooth consumption, however, UI benefits will have a differentially large impact during unemployment spells. This is what we appear to find, comparing the results for unemployed spells (Table 3) to our results for employed spells (Table 4).

A further prediction of the life cycle model of labor supply with liquidity constraints is that the differential between the effect of UI during employed and unemployed spells will fall as the underlying risk of unemployment rises. Consider two couples, one of which (couple A) has a 100% chance of having an unemployment spell over a two year period, and one of which (couple B) has a 10% chance. Under this scenario, there will be a much larger spousal labor supply response to UI for couple A than for couple B, since UI increases represent a larger expected income flow. Moreover, if there are perfect capital markets, then for both couples the response will be equal during months of employment and unemployment, since UI is just a source of transitory income so that there are no intertemporal effects. That is, if couple B should happen to become unemployed in a low UI regime, they will just borrow to smooth consumption during their spell, rather than having the spouse suddenly work more.

Now suppose that there are imperfect capital markets, so that couples cannot borrow during spells of unemployment. Even in this world, couple A will smooth the impact of UI over the life cycle through savings, since UI represents a certain income stream. But, for couple B, savings is an imperfect smoothing device, and if they are unable to borrow or otherwise purchase insurance,

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[30]There could be some small differential effect of UI during unemployment spells, to the extent that the unemployment spells convey information about future unemployment prospects; but, since UI is time limited and therefore small relative to lifetime income, this effect should be second order.

there will be a differential spousal labor supply response to UI during the spell of unemployment itself. That is, UI will have a larger impact during unemployment spells than during employment spells for this low risk group. Therefore, there is an additional testable implication of the imperfect capital markets view: the differential response of spouses to UI during spells of unemployment should be larger for low risk than for high risk couples.

We test this supposition in the final panel of Table 4, by presenting additional results for our sample of unemployed spells, split by predicted risk in parallel fashion to the second panel (using the same cutoffs for high and low risk). Indeed, comparing the coefficients in the second and third panels of Table 4, for those in the high risk group there is a relatively small additional effect of UI during unemployment spells: one hundred dollars of UI per week lowers hours per month by 7.3 while the husband is employed, and by 16 while he is unemployed. But for the low risk group, there is a striking difference between employment and unemployment: each $100 increment to UI results in only a 0.1 hours/month reduction in work during employment spells, but a 34.2 hours/month reduction during unemployment spells. Thus, families in this group do not appear to be smoothing the wife's labor supply over the life cycle; instead, there is a large differential response to UI benefits during the unemployment spell itself. This suggests that these families are liquidity constrained during unemployment spells.

Presence of Young Children

A critical element in labor supply decision-making for couples is family structure. In particular, the presence of young children may have an important effect on the responses of wives to the income flows from UI. As discussed in Killingsworth and Heckman (1986), it is likely that wives will respond more to the state-contingent stream of UI income if there are small children

present. In the context of a simple static model of the allocation of time, the elasticity of labor supply of wives rises with their value in home production and falls with their net wage.[31] So long as women are in charge of child care, their value in home production will be highest when there are small children in the household. And the presence of children means that the wife's work will imply associated child care costs, lowering her net wage. Thus, a prediction of this model is that the elasticity of response to income flows during unemployment will be highest when there are small children present. Moreover, having children in the household may increase the responsiveness of labor supply to family income because family consumption is less flexible with respect to variations in income (due to the fixed consumption needs that are tied to children), so that leisure must be more flexible (Mincer, 1962).

In Table 5, we assess how the effect of UI varies with the presence of children, by freeing up our UI coefficient by whether there is a child less than 6 in the household. In fact, there is a much stronger crowdout effect of UI in households with a small child. According to the total hours equations, households without a child, each $100 in UI income per week results in 18.8 fewer hours of work per month; but in those with a small child, each $100 of UI per week results in 31.5 fewer hours, an effect that is over 50% larger. In fact, the main effect of having a young child present is zero for this sample, in contrast to the large negative effect for the full sample in the AWE

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[31]Consider a family consisting of two persons, m and f, with utility $U = Z$, where Z is a consumption good produced according to the production function $Z=L_f^a L_m^b C^{1-a}$, L_i is the hours of leisure of family member i, and C is a consumer good. The family maximizes their utility subject to $C = R + w_m H_m + w_f H_f$, where R is exogenous income, w_i is the net hourly wage for person i, and $H_i = T - L_i$ is hours of work (T is total time available). Differentiating the first order condition for family member f with respect to non-wage income R, one obtains: $\delta L_f/\delta R = a/w_f$. So the response of the wife's leisure (and thus her labor supply) to income changes will rise with a, her relative value in home production, and fall with w_f, her net hourly wage. It is important to emphasize that the level of labor supply falls with a, which is consistent with the large and significant negative coefficients on the presence of small children in Table 2. But, conditional on a given level of labor supply, the elasticity of labor supply rises with a.

regressions; that is, on average, families with small children where the head is unemployed appear no longer able to substitute the home production of the wife for lower consumption by the family. Thus, our findings are consistent with the notion that income variations are a more important determinant of women's labor supply when there are small children present.[32]

Part VI: Conclusions

In the absence of private financial markets for insuring unemployment, a natural presumption is that other forms of insurance will arise to smooth the family's consumption over this adverse shock. One source of such insurance is spousal labor supply. But the strong theoretical presumptions in favor of an Added Worker Effect have been surprisingly refuted by much of the previous literature on this topic, as well as by our own estimates using more recent data. In this paper, we suggest that a major reason for the inability of previous research to measure an AWE has been the presence of public insurance for unemployment spells through the UI program. We provide evidence to support this contention; our estimates suggest that UI is significantly crowding out the labor supply of the wives of unemployed husbands.

While we find a sizeable crowdout, our estimates also suggest that even in the absence of UI there would still be a large reduction in family income from the unemployment of the husband. Thus, spousal labor supply only provides at best partial insurance against the income risk from unemployment. This partial insurance may be due to the other forces counteracting the AWE, such as the DWE. Alternatively, it may simply reflect the fact that given the relatively low earnings of

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[32]These findings are not simply the result of an omitted interaction with having small children; when we estimate separate equations for those with and without small children (paralleling the second and third panels of Table 4), the results are somewhat stronger, although the standard errors rise as well.

wives compared to their husbands, it is difficult to use this mechanism to substantially replace the reduction in family income from the husband's unemployment.

Our estimated response to UI is quite large; the income effects implied by our results are much larger than those estimated in much of the previous literature on female labor supply. This large estimated income sensitivity may arise from the fact that households are liquidity constrained and have fixed consumption needs during unemployment spells. This would be consistent with our finding that the effect of UI is larger when young children are present. It is also consistent with our result that families appear unable to smooth the spousal labor supply response to UI over the life cycle; this is a natural outcome in a model where liquidity constraints prevent couples from intertemporally optimizing their labor supply patterns. And it is consistent with the evidence in Gruber (1994), who finds a significant consumption smoothing effect of UI.[33] An interesting direction in which to extend this research would therefore be to consider the interaction of spousal labor supply with indicators of the ability of couples to smooth their consumption, such as accumulated assets.[34]

It is also interesting to consider our findings in the context of recent studies of the impact of female labor supply on income inequality. Juhn, Murphy and Topel (1991) and Juhn and Murphy (1996) find that, despite a secular fall in the employment and earnings of those men at the bottom of the income distribution since the early 1970s, the labor supply of their wives has increased much less than the labor supply of wives of husbands higher up the income distribution. In this context,

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[33]The results of that analysis are not inconsistent with the crowdout of spousal labor supply that we find here, in that the measured effect in Gruber (1994) is relatively small (with each $1 in increased UI benefits leading approximately to a 27 cent consumption increase).

[34]This type of analysis will be difficult, of course, since the choice of asset levels and labor supply should be modelled simultaneously.

our findings suggest that spousal labor supply may be playing quite different roles as short run insurance against unemployment versus long run insurance against changes in permanent earnings prospects. On the other hand, the findings of these other papers may reflect the fact that the opportunities for the wives of low skilled men (who are often themselves low skilled) have deteriorated as well; also, changes in income taxation over this period have given the largest labor supply incentives to wives of high earners (Eissa, 1995). Considering further the use of spousal labor supply as insurance against different types of long run and short run income risk is an important priority for future work on this topic.

Finally, we have not considered the welfare implications of our findings. Our results suggest that public insurance through UI is substantially crowding out a private insurance mechanism. But this mechanism is not a costless one. In particular, we have not measured the loss in family utility from increases in spousal labor supply to compensate for reductions in UI benefits. That is, when UI generosity is reduced the family is "buying" more insurance by reducing the leisure of the spouse, and this is not reflected in our estimates. A welfare computation of the effects of UI generosity changes must account for the family utility reduction from this increase in spousal labor supply.

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Table 1: Means

	All Months			First 3 Months	
	(1) Everyone	(2) Husband Employed	(3) Husband Unempl.	(4) Husband No Spell	(5) Husband Has Spell
Wife Employed	.634	.634	.624	.633	.619
	(.437)	(.436)	(.467)	(.475)	(.479)
Wife's Monthly Hours (including 0's)	98.0	98.0	98.9	98.1	97.6
	(77.3)	(77.1)	(82.7)	(84.8)	(84.8)
Wife's Monthly Hours (if >0)	131.4	130.8	149.5	151.5	154.2
	(60.2)	(60.3)	(52.6)	(52.4)	(51.3)
Wife's Hourly Earnings (>0)	7.24	7.25	6.78	7.49	6.73
	(4.02)	(4.01)	(4.21)	(4.61)	(4.26)
Wife's Age	36.5	36.4	36.8	36.3	35.9
	(7.1)	(7.0)	(7.0)	(7.0)	(7.2)
Husband's Age	38.5	38.5	39.0	38.3	38.1
	(7.3)	(7.3)	(7.4)	(7.2)	(7.5)
Wife's Years of Education	13.1	13.1	12.4	13.2	12.3
	(2.6)	(2.6)	(2.8)	(2.6)	(2.8)
Husband's Years of Education	13.5	13.5	12.3	13.7	12.3
	(2.9)	(2.9)	(3.1)	(2.9)	(3.1)
Wife White	.897	.898	.850	.903	.864
Kids Under 2 Yrs	.150	.151	.129	.165	.147
	(.378)	(.379)	(.352)	(.396)	(.372)
Kids 2-5 Yrs	.326	.326	.322	.329	.333
	(.583)	(.583)	(.580)	(.584)	(.590)
Kid 6-18 Yrs	.963	.960	1.04	.947	.999
	(1.06)	(1.06)	(1.10)	(1.05)	(1.10)
Husband's Wkly Wage Last Qtr	474	479	336	497	377
	(252)	(251)	(242)	(278)	(236)
UI Weekly Benefits	152	153	120	154	141
	(41)	(40)	(49)	(41)	(48)
Avg Female Wg by St/Ed/Yr	7.19	7.19	7.10	7.26	6.96
	(1.72)	(1.73)	(1.61)	(1.71)	(1.64)
Avg Female Unem by St/Ed/Yr	.049	.049	.055	.049	.055
	(.030)	(.030)	(.034)	(.030)	(.035)
Observations	33169	30294	2875	21018	4176

Notes: Based on authors' tabulations of SIPP data described in text. Standard deviations in parentheses.

Table 2: Basic AWE Regressions

	(1) Hours Tobit	(2) Employed	(3) Hours Heckit	(4) Hours OLS	(5) Employed
Husband Unemployed	-3.02 (3.15) [-2.42]	-.012 (.014)	8.22 (2.43)	-1.79 (1.15)	-.013 (.007)
Wife's Age	5.57 (1.08)	.025 (.005)	3.00 (.81)	5.13 (2.72)	.061 (.016)
Wife's Age^2	-.082 (.014)	-.0003 (.0001)	-.037 (.011)	-.073 (.036)	-.001 (.000)
Husband's Age	.150 (1.05)	.001 (.005)	1.31 (.75)	3.68 (2.65)	-.017 (.016)
Husband's Age^2	-.006 (.013)	-.0000 (.0001)	-.015 (.009)	-.039 (.034)	.0003 (.0002)
Wife White	-18.5 (1.8)	-.061 (.008)	-8.95 (1.55)	-11.8 (5.5)	-.043 (.032)
Wife's Education: 12 Yrs	32.3 (2.0)	.139 (.009)	10.4 (2.6)	13.3 (4.3)	.091 (.025)
13-15 Yrs	44.0 (2.5)	.196 (.011)	13.3 (3.4)	5.76 (5.20)	.069 (.031)
16+ Yrs	68.0 (2.7)	.277 (.012)	28.7 (4.3)	12.1 (6.1)	.053 (.036)
Husband's Education: 12 Yrs	-1.02 (1.88)	.002 (.008)	-1.16 (1.35)	-3.12 (4.32)	.022 (.025)
13-15 Yrs	.308 (2.16)	.007 (.009)	.390 (1.54)	-4.80 (5.23)	-.015 (.031)
16+ Yrs	-8.74 (2.42)	-.026 (.011)	-5.45 (1.77)	1.04 (5.84)	.031 (.034)
# of Kids: 0-1 Yrs	-46.7 (1.5)	-.177 (.006)	-18.1 (3.1)	-19.4 (2.14)	-.123 (.013)
2-5 Yrs	-35.3 (1.0)	-.128 (.004)	-18.1 (2.0)	-18.1 (1.88)	-.107 (.011)
6-18 Yrs	-9.51 (.56)	-.029 (.002)	-7.61 (.50)	-10.1 (1.38)	-.044 (.008)

Table 2: Basic AWE Regressions, Continued

	(1) Hours Tobit	(2) Employed	(3) Hours Heckit	(4) Hours OLS	(5) Employed
Female Unemp.	-32.4	-.176	-4.88	-35.4	-.063
	(23.5)	(.102)	(17.3)	(21.8)	(.128)
Avg. Female Wage	1.20	.003	1.02	1.79	.009
	(.51)	(.002)	(.36)	(.70)	(.004)
Fixed Effects?	No	No	No	Yes	Yes
Number of Obs	33169	33169	24197	33169	33169

Notes: Standard errors in parentheses. Regressions also include spline in husband's quarterly earnings, dummies for husband's industry/occupation, and state and year dummies.

Table 3: Does UI Crowd Out the AWE?

	(1) Hours Tobit	(2) Employed	(3) Hours Heckit
	Reduced Form		
Potential UI Benefits	-0.325	-0.127	-0.149
	(0.131)	(0.051)	(0.095)
Effect on Observed Hours	-0.237		
Mean	97.3	.618	148.6
Implied Effect at UI = 0	126.9	.789	168.6
	2SLS		
UI Benefits Received	-.623	-0.238	-0.353
	(.251)	(0.096)	(0.225)
Effect on Observed Hours	-.454		
Number of Obs	2560	2560	1667

Notes: Standard errors in parentheses. Coefficients and standard errors on employment dummy are multiplied by 100. Regressions include all of covariates listed in Table 2, as well as spline in husband's quarterly earnings, dummies for husband's industry/occupation, and state and year dummies.

Table 4: Extension - Employed Sample and Predicted Risk

	(1) Hours Tobit	(2) Employed	(3) Hours Heckit
Employed Sample			
Potential UI Benefits	-0.057 (0.033) [-0.046]	-0.011 (0.015)	-0.046 (0.024)
Number of Obs	30056	30056	22153
Employed Sample - Cut by Predicted Risk			
Potential UI Benefits- Low Risk	-.002 (.057) [-.001]	.009 (.021)	-.009 (.034)
Potential Benefits- High Risk	-.091 (.045) [-.073]	-.037 (.023)	-.093 (.039)
Number of Obs (Low Risk, High Risk)	17633, 12423	17633, 12423	13729, 8846
Unemployed Sample- Cut by Predicted Risk			
Potential UI Benefits- Low Risk	-.506 (.223) [-.342]	-.184 (.094)	-.065 (.179)
Potential Benefits- High Risk	-.221 (.176) [-.160]	-.080 (.070)	-.151 (.115)
Number of Obs (Low Risk, High Risk)	899, 1661	899, 1661	547, 1036

Notes: Standard errors in parentheses. Coefficients and standard errors on employment dummy are multiplied by 100. Regressions include all of covariates listed in Table 2, as well as spline in husband's quarterly earnings, dummies for husband's industry*occupation, and state and year dummies.

Table 5: Extension - Effects of Having Young Child Present

	(1) Hours Tobit	(2) Employed	(3) Hours Heckit
UI Benefits	-0.258 (0.137) [-0.188]	-0.105 (0.053)	-0.141 (0.091)
Child < 2	-4.46 (14.9) [-2.23]	-0.041 (0.058)	1.68 (8.72)
UI Benefits* Child < 2	-0.255 (0.101) [-0.127]	-0.078 (0.039)	-0.129 (0.077)
Number of Obs	2560	2560	1667

Notes: Standard errors in parentheses. Coefficients and standard errors on employment dummy are multiplied by 100. Regressions include all of covariates listed in Table 2, as well as spline in husband's quarterly earnings, dummies for husband's industry/occupation, and state and year dummies.