Childcare Subsidies, Home Production, and Extended Income

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Abstract

Prior studies have suggested that home production is able to substitute market consumption. Hence, home production should be taken into account when examining whether an income shock makes people worse or better off in terms of their income and consumption possibilities. This particularly holds for people who spend relatively many hours in home production, such as mothers. Whilst studies found high responsiveness to market work and household work decisions among this group, this paper is the first to study the causal effect of changes in childcare subsidies on extended income of mothers, where we add the monetized amount of home production to the money income of mothers. For causal identification, we exploit a substantial cut in childcare subsidies in the Netherlands in January 2012, using a differences-in-differences strategy. The treatment group consists of mothers with a youngest child 0 to 12 years of age. The control group consists of mothers with an older youngest child. We find that the reform significantly increased time spent in home production (excluding child care) for the treatment group compared to the control group, but it did not significantly affect gross (and net) income. Using several methods for monetization of home production we find that the cut in childcare subsidies increased total extended income of mothers. This seems to suggest that the policy change actually made mothers better off in terms of their total consumption possibilities. It could mean that mothers overcompensate their loss in benefits by substantially increasing their home production.

1. Introduction

How can we assess the overall welfare position and consumption possibilities of an individual? Scholars tend to use an individual's cash flow for this. However, home production improves household welfare without being reflected in the household's cash flow, either in disposable household income or in gross income, like other types of private in-kind income, such as imputed rent for owner-occupied housing and fringe benefits (Smeeding and Weinberg 2001; Frick et al., 2012). Time spent on home production monotonically increases or complements the existing level of money income of an individual, since an individual cannot spend below-zero time, and as home production allows for income-independent consumption possibilities. Thus, taking into account the value of home production by monetizing this, and adding this to money income, reaching what is generally referred to as 'extended income', provides a better view on total welfare of an individual.

Home production not only plays a complementing role to an individual's money income by increasing total consumption possibilities, 'time' can also substitute for market consumption spending (Becker, 1965). Home production can play a role as a work insurance to smooth consumption when an individual is faced with a negative income shock (Becker, 1965; Aguiar et al., 2012; 2013; Guler & Taskin, 2013; Hicks, 2015). Aguiar et al. (2013) find that home production (excluding child care) absorbs around 30% of foregone market work hours during the Great Recession in the US for people aged 18-65, exploiting state-level variation of the business cycle. Moreover, their results indicate that the responsiveness of time use to changes in market work is relatively stable across genders, but differs between married and singles. For married people, around 40% of the foregone hours of market work is allocated to home production and childcare compared to 15% for singles. This makes home production a 3 to 4 times more elastic margin of substitution than leisure. Guler & Taskin (2013) provide further evidence that home production is a self-insurance mechanism against lost or reduced earnings with 2003-2008 ATUS data for the US. They find using state-level variation in unemployment insurance that unemployment insurance benefits crowd out home production – a doubling of insurance benefits reduces home production by about 22% for the unemployed.

In this paper we evaluate the dynamics in extended income, following an exogenous shock in money income resulting from a policy change that only affected a specific group of individuals. This allows us to conduct a difference-in-difference estimation (Been & Jongen, forthcoming; Bettendorf et al., 2013). We bring together the complementing role of home production, by monetizing the hours spent on home production to measure extended income, and the substituting role of home production, by tracking the evolution of extended income over time when individuals are faced with an exogenous money shock. Thus, we look at individual panel data as in for instance Guler & Taskin (2013) as opposed to a macro-level information (Aguiar et al., 2013). Compared to Guler & Taskin (2013) we exploit an exogenous shock in income.

To the best of our knowledge, the dynamics of extended income have not been studied yet in the literature. There is a large literature on labour supply decisions, for instance related to marginal tax rates (e.g., Saez et al., 2012). The

dynamics of the money income distribution (Bourguignon, 2005) and poverty (Duncan et al., 1993; Cappellari & Jenkins, 2004) are relatively well-researched. Yet, to the extent that home production indeed plays a money income-smoothing role, the dynamics of total individual welfare might be overstated.

Our identification strategy to uncover causal effects in changing net wages, home production decisions, and extended income of women comes from the cut in childcare benefits in the Netherlands at the beginning of 2012. As we will discuss in greater detail in Section 2.4, this policy change incorporated an actual cut in benefits and a status quo in the maximum hourly price leading to increases in the net cost of formal childcare of up to 20% for parents with one child to 33% for parents with two children in formal day care (Akgunduz et al., 2015). Since childcare subsidies are only given to parents with children at primary school, we assume that the policy change only affected families with at least one child younger than 12 years. This treatment group is compared to individuals with children aged 12-18. We have information on income and time use of individuals for two years before the policy change (2009, 2010) and one year after (end of 2012).

In this paper we focus on evaluating incomes of mothers. Tests conducted for fathers showed no significant behavioural changes. Female labour supply is responsive to financial incentives in general (Eissa & Liebman, 1996), and in particular to childcare benefits (Bettendorf et al., 2015) and childcare costs (Gonzalez Chapela, 2011). Mothers are also an interesting group from the perspective of in-work poverty. Women are more likely to be income poor and time constrained (to be checked: Vickery, 1977; Douthitt, 2000; Harvey & Mukhopadhyay, 2007). That is because labour supply and home production are strong substitutes among women (Gelber & Mitchell, 2012). Women spend relatively more time in home production, and the same holds for households with children (to be checked: Zacharias et al., 2012; Merz & Rathjen, 2014a; 2014b). Even though these considerations make mothers an interesting group to focus on, it reduces the overall generalizability of our results to the rest of the population.

Our inspection of extended income over time also allows us to look at the level and dynamics of the distribution of extended income. Extended income inequality is found to be more equally distributed than money income (Frazis & Stewart, 2011; Zick et al., 2008; Frick et al., 2012; Folbre et al., 2013). Frazis and Stewart (2011) find that (1) hours of non-market work are more evenly distributed than hours of market work; (2) they vary relatively little in terms of market value; and (3) are negatively correlated with hours of market income (the substitution argument discussed earlier). The dynamics of extended income inequality has to our knowledge only been examined by Gottschalk and Mayer (2002). These scholars find comparable patterns over time for the money income and extended income inequality for the U.S. for four years.

2. Monetising procedures

In order to assess total consumption possibilities or welfare of individuals, we monetize home production and add it to money income to arrive at extended income. An alternative would be to analyse the income- and time distributions separately (as

in Merz & Rathjen, 2014a; 2014b), but this does not provide the overarching welfare picture. Since the results can be sensitive to the assumptions regarding the modification (Zacharias, 2011) we apply the three most common approaches for monetising home production (see Frick et al., 2012 for an extensive discussion). We now discuss these three approaches.

2.1 Minimum wage or housekeeper approach

The housekeeper or minimum wage approach assigns a typical wage to the time spent for each type of home production (e.g., the wage of a chef for cooking, see also Frazis & Stewart, 2011). In this aspect it could be seen as a replacement costs approach by assessing how much money it costs to let someone do the same job (Suh & Folbre, forthcoming). In the Netherlands with a relatively high minimum wage all people conducting these marketized chores will essentially earn the minimum wage, so for this approach, we multiply hours spent on home production by the minimum wage. By assigns a uniform wage to all individuals for each type of home production, the minimum wage approach ignores the quality aspect of the product. Moreover, it assumes that the individual is equally productive as the specialist (Frick et al., 2012).

2.2 Observed wage or opportunity cost approach

The observed wage or opportunity cost approach starts from a different perspective, as it focuses on the foregone earnings of individual that s/he would have obtained when s/he had spent the hours in the labour market rather than on home production. The rationale here is that individuals will weigh whether it is more profitable to produce themselves or to earn market income and purchase a product. In this way, the approach allows for individual heterogeneity by incorporating the individual's capacity to earn money income and the individual's productivity in home production. Compared to the minimum wage approach, the assumption is no longer made that "[...] a lawyer is five times more productive building a deck than a carpenter" (Frazis & Stewart, 2006: 10; 2011: 8); instead, the lawyer is assumed only to build a deck if s/he would otherwise earn less than the price of hiring a professional to repair it.

Yet, the observed wage approach also comes with a strong assumption, which is in this case that individuals have a free choice of working unlimited hours in their paid job (Frazis & Stewart, 2006; 2011; Zick et al., 2008; Frick et al., 2012). There tend to be restrictions on paid overtime hours (Anger, 2006), and it seems to imply that non-working individuals have no opportunity costs as the possibility to gain market income and purchase does not exist (Zick & Bryant, 1990). Moreover, preferences and non-monetary considerations constrain the decision-making process (Frick et al., 2012). For non-working mothers we apply the minimum wage, assuming that they make a choice between home production or earning at least the minimum wage.

2.3 Predicted wage approach

Frick et al. (2012) propose a predicted wage approach to monetize home production. This approach allows for productivity differences across individuals, as in the

observed wage approach, whilst doing a better job in accounting for individual variation in opportunities. The logic is to derive an estimate of the individual's earnings capacity or a predicted wage, which can be calculated independent of current employment status. It assumes an individual has an 'average' productivity in any type of activity, whether this is home production or paid work, derived from age, health, household constraints, skills, and qualifications.

Frick et al. (2012) argue that the predicted wage approach is preferred as it overcomes the assumption of constant productivity across individuals, as in the housekeeper wage approach. Instead the predicted wage approach accounts for individual variation in productivity as well as in opportunities. Similar to Frick et al., we derive a rather simple estimate of the individual earnings capacity based on observed and unobserved individual characteristics. From this estimate, the "predicted wage" can be calculated for every person independent of employment status, and is likely to show less variation than the observed hourly wages for those who are employed.

We estimate the predicted wage by applying individual fixed effects regression, e.g. using within-person variation. We allow for quadratic age effects interacted with educational level to explain the individual's wage. Since we have a very particular subgroup of women with children it is likely that wage estimations would suffer from selection effects as the women with higher wages are more likely to participate, for example. To correct for selection effects in the wage, we largely follow Wooldridge (1995). Basically, we estimate year-dependent participation equations using probit regression and construct the inverse Mill's ration from this. The inverse Mill's ratio is added to the wage equation to correct for the possibility that the error terms of the participation equations and wage equation might be correlated. As exclusion restrictions in the participation equations, we use household characteristics which is common in practice. More specifically, we use the number of children in different age categories and whether a mom is single as exclusion restrictions. These characteristics are likely to affect the participation decision but are not likely to affect the wage once working.

Unlike Frick et al. (2012), we conclude that correcting for selection in the wage equation matters. The estimation results (not reported here) show that correcting for selection is important in the wage equation, e.g. the effect of the inverse Mill's ratio is significantly different from zero. Our results suggest that the predicted wage has a substantially lower variation than the observed wage similar to Frick et al. (2012).

3. Identification strategy and data

3.1 Policy change

Our identification strategy to uncover causal effects in changing net wages, home production decisions, and extended income of women comes from the 2012 cut in childcare benefits in the Netherlands. Subsidies are paid for a pre-set maximum hourly price of the day care centre. Families do not receive a subsidy for the portion of the price that is above this pre-set maximum. The 2012 cut in childcare benefit both implied an actual cut in benefits and a status quo in the maximum hourly price

leading to substantial increases in the net cost of formal childcare. Akgunduz et al. (2015) show that this increase amounted up to 20% for parents with one child to 33% for parents with two children in formal day care. These authors also show that the number of children in formal childcare dropped by about 3% while the average hours of children in formal childcare dropped by about 5%. This indicates that the cut was substantial enough to induce changes in decision making. To the best of our knowledge there were no other policy changes in the same period or before affecting the individuals along the same group lines.

Since childcare subsidies are only given to parents with children at primary school, we assume that the substantial cuts in childcare benefits only affected families with at least one child that is younger than 12 years old. Therefore, we define women with at least one child below 12 years old as our treatment group. Women with only children at the age of 12 or above are most comparable to this treatment group and are used as a control group.

3.2 Data on time use and income

For our objectives we need individual micro panel data in which we observe income, market hours, and home production, covering information before and after the cut in childcare benefits at the beginning of 2012. Therefore, we use the CentER LISS Panel, which has information with the same questions for 2009, 2010, and 2012 for Dutch individuals. This implies that we have two pre-treatment periods (2009 and 2010), and since the childcare cuts were introduced in January 2012 and our survey was set out in September, we can use the 2012 wave of LISS as the post-treatment period.

The LISS Panel is not a time diary dataset but recall data. A number of studies have shown that time diaries provide more accurate estimates for time spent on home production compared to recall data surveys (Aguiar et al., 2012; Robinson & Godbey, 1999; Frazis & Stewart, 2009). Recall estimates tend to add up to a number exceeding total time endowment. Unfortunately, there is no individual panel information available for time diaries around the policy shock we are interested in. Moreover, as we will be using individual fixed effects as part of our difference-in-difference design (as discussed later), the constant part of measurement error will disappear.

3.3 Home production definitions

Our definition of home production largely follows the definition of Aguiar et al. (2013). Home production includes household chores such as cleaning, laundry, shopping, cooking, gardening, etc. It does not include personal care or care for children, parents, and other family and non-family members or educational activities. People are asked to respond to the question: *"How much time did you spend in the last seven days on household chores?"* Responses are filled out in hours and minutes.

We exclude activities with children as home production (such as washing, dressing, playing, reading, taking child to see doctor, taking child to school/hobby activities, etc.) following Gronau (1977). According to Gronau (1977) childcare should not be considered home production as it does not respond to changes in prices

like regular market work and home production would (see also Kimmel, 2006). The main issue with childcare produced at home is that it is considered to be an imperfect substitute for childcare bought on the market due to positive utility of home-produced childcare to the parents. Such positive utility is less clear in true home production activities such as cooking and gardening.

3.4 Income definitions

The LISS dataset includes information on individual gross and net income. More specifically, people are asked to fill out their personal gross (net) monthly income in euro.

Since we expect that the labour supply and home productions changes are strongest among women when cutting childcare benefits (Gelber and Mitchell, 2012), we solely focus on women with children and not men. Moreover, childcare benefits are introduced to stimulate female labour supply specifically (see for example Eissa and Liebman, 1996). Hence, we use the individual non-equivalised income of women with children. This approach deviates from the fact that household members can share income as well as the burden of cuts in childcare benefits.

The introduced cut in childcare benefits decreased women's net income, but did not change women's gross income *ceteris paribus*. Gross income only changes because of labour supply decisions following a drop in net income. Since we are specifically interested in the consequences of childcare benefit cuts for income through different time allocations, we take gross income as our main income variable of interest. For robustness, we also present results using net instead of gross income.

Following Merz and Rathjen (2014) we construct extended income by adding the money value of home production to gross income. We use different approaches to monetize home production, e.g. minimum wage approach, observed wage approach, and predicted wage approach. All monetary variables are expressed in 2006 euro using the CPI from Statistics Netherlands.

For the minimum wage approach, we apply the minimum wage to all persons subject to the analysis. The observed wage is approximated by rescaling gross earnings to the number of hours worked. For persons without gross earnings or a wage that is lower than the minimum wage, we replace the wage by the minimum wage. The predicted wage approach estimates a wage regression corrected for selection effects (using Wooldridge, 1995) taking the rescaled gross earnings as a dependent variable.

4. Empirical analysis

4.1 Empirical model

Given that the policy change only affected women with a child below 12, we use a difference-in-difference strategy. We estimate the following model with index i the individual, and index t is the time (2009, 2010, 2012).

$$y_{it} = \beta_0 + \beta_1 P_t + \beta_2 G_{it} + \beta_3 P_t \cdot G_{it} + \beta_4 X_{it} + \alpha_i + \pi_t + \epsilon_{it}$$

Where y can consist of gross money income, home production, or gross extended income (all in real euro) in our main tests. As sensitivity tests we also look at net income and net extended income. P is a dummy variable indicating the pre-treatment periods (2009 and 2010) and the post-treatment period (2012). G is a dummy variable equal to 1 for the treatment group, and equal to 0 for the control group. β_3 captures the treatment effect of the cut in childcare benefits on the outcome variable of interest, measured by the coefficient for the interaction of the treatment group dummy and the treatment period dummy. X is a vector of control variables including personal (age and age squared) and household characteristics (whether an individual is single or not). α_i captures the individual fixed effects. A fake treatment, or placebo, for 2010 shows that we can assume that the parallel trends assumption holds, e.g. the fake treatment is not significantly different from zero (not reported here). ε represents the error term.

4.2 Descriptives

We start by showing simple descriptive statistics on pre- and post reform for control and treatment group in Table 1 (see also Frick et al., 2012; Frazis & Stewart, 2011).

Table 1: Descriptives for treatment and control group

	Treatment			Control			Diff-in-diff
	2009-2010	2012	Difference	2009-2010	2012	Difference	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age	38	40	1	43	46	3	-1
	(5)	(5)		(5)	(4)		
Gross money income (€/m)	1167	1119	-48	1232	1284	52	-100
	(937)	(949)		(907)	(904)		
Net money income (€/m)	834	798	-36	902	941	39	-75
	(610)	(614)		(601)	(586)		
Home production (h/m)	82	76	-6	91	76	-15	9
	(65)	(52)		(57)	(50)		
Extended income (minimum)	1793	1688	-105	1919	1849	-70	-35
	(903)	(885)		(906)	(888)		
Extended income (observed)	2039	1841	-198	2138	2101	-37	-161
	(1255)	(1122)		(1204)	(1349)		
Extended income (predicted)	1814	1593	-221	2295	2341	46	-267
	(1168)	(907)		(1105)	(1235)		

Note: the extended income figures shown are gross

First we look at the levels of all variables. This shows that the control group scores higher on all dimensions we examine, both before the policy change (2009-2010) and after (2012) – except for hours in home production in 2012. The fact that money and extended income levels as well as hours of home production before the policy change are all higher for the control group could be a consequence of age. Because of this, in our regressions we control for age and age squared. We can see that by construction, average extended income is (monotonically) higher than gross money income. For the treatment group, extended income was 54% (minimum wage), 55% (predicted wage), or 74% (observed wage approach) higher before the treatment. This shrunk for all, to in between 42% (predicted wage approach) and observed wage approach (62%), although it did not go down much for the minimum wage approach (from 54 to 51%). For the control group before the treatment extended income was generally

higher as a percentage of gross money income for every monetizing procedure (56% for minimum wage, 74% for observed wage, 86% for predicted wage). This dropped as well, and interestingly, for this group much more for the minimum wage approach (to 44%), and less so for the predicted wage approach (to 79%) and the observed wage approach (to 64%). The fact that the predicted wage approach adds relatively more to gross money income for the control group than for the treatment group might come from the fact that the control group is older, on average.

Moving to the differences over time per group, we can see that for the treatment group all variables except age went down over time (Column 3). Gross and net money income went down by 48 (4.1%) and 36 euro (4.3%) respectively. For the control group we see the opposite – gross and net money income went up by 52 (4.2%) and 39 euro (4.3%) respectively. The fact that gross and net money income went down for the treatment group but not for the control group might be a result of the policy change we exploit. Home production went down for both groups, but more so for the control group (15 hours compared to 6 hours per month on average). This fits in a wider trend of decreasing home production time noted more broadly in the literature (Robinson and Godbey 1999; Aguiar and Hurst 2007; Ramey 2009; Ramey and Francis 2009). Aguiar et al. (2013: 1672) find that women in the U.S. reduced their nonmarket work time by nearly 2 hours per week or 8 hours per month between 2003-2008. Moving to extended income, we can see that as a result of the decrease in both money income and hours spent on home production levels of extended income went down substantially – depending on the monetizing procedure by 105 (5.9%), 198 (9.7%), or 221 euro (15.2%). For the control group levels of extended income went down less so for the minimum (3.6%) and observed wage (1.7%) approach, and increased slightly for the predicted wage approach (1%).

All in all, comparing the evolution for the treatment group to that of the control group, we see that on average the money income position of the treatment group worsened, whereas its hours spent on home production went up (decreased less) (Column 7). The treatment group also on average lost ground in relative terms regarding their extended income position. However, it is important as we will see later that these calculations do not correct for control variables or individual fixed effects. Moreover, for all variables, the standard deviations are large as well, implying ample variation in the cross-sectional means.

4.3 Main results for the difference-in-difference estimations

For our main results, we focus on gross money and extended income, and we restrict our sample to individuals observed in all periods (balanced). Our results are shown in Table 2.

In Column 1 we first show the results for using gross money income. Our treatment variable is not significant here, indicating no significant difference in the evolution of gross money income between treatment and control group once individual fixed effects are included. If anything, the coefficient is hinting at a positive effect on the level of gross money income of mothers due to the policy change. The dummy variable indicating the treatment group ("Mothers child $<$ 13") is negative and significant for gross money income, however. This indicates that there

is a negative average permanent gross money income difference between the treatment and the control group. Further results not shown here (see Been & Jongen, forthcoming) indicate that the policy change does not have an effect on (market) labour supply of women, both in the intensive and extensive margin. Leaving out the other control variables does not affect these results (results not shown here). The individual fixed effects, however, are important. Thus, we find that the results are driven by using within-person variation over time. Leaving out individual fixed effects only compares the cross-sectional means and should be avoided according to the analysis that includes fixed effects and, hence, analyses the mean within-person variation.

In Columns 2-7 we move to extended income, our more encompassing income concept than gross money income from Column 1. We show the results for our three ways of monetizing home production, namely the minimum wage or housekeeper approach (Columns 2-3), the observed wage approach (Columns 4-5), and the predicted wage approach (Columns 6-7). Now we see effects of the policy change on the income position of women. Interestingly, however, the sign is positive rather than negative for all monetizing procedures. Thus, the policy change made mothers richer rather than poorer when their home production is monetized and taken into account. This both holds when we only look at monetized home production, ignoring gross money income (the even columns) and when we examine extended income or the sum of monetized home production and gross money income (uneven columns). It corresponds to our descriptives where we found that the amount of home production of our treatment group increased after the policy change relative to the control group.

The results of the effects on extended income do not seem to depend strongly on the monetizing procedure. For the minimum wage approach, the effect is significant at the 5% level; for the observed and predicted wage approach the effect is only significant at the 10% level. The coefficients are quite comparable across the different monetizing approaches. Looking at extended income, the effects vary between 214 euro per month (minimum wage approach) and 307 euro (predicted wage approach). These treatment effects are not significantly different.

For the extended income definitions the dummy variable indicating the treatment group ("Mothers child $<$ 13") is generally insignificant, apart from extended income, predicted wage approach at the 10% significance level. This seems to imply that the negative average permanent income difference between the treatment and the control group that we found previously for money income disappears once we look at extended income. The dummy variable indicating the 2012 period is only significant for the home production variable when monetized using the observed wage approach. Thus, only for this specification there is an indication of a significant time trend common to both the treatment and control group.

All in all, the different estimation techniques paint quite a comparable picture of the effects on total consumption possibilities of mothers. Mothers seem to substitute so strongly following an income shock that they seem to become richer once home production is accounted for.

Table 2: Main results for monetized income

		Minimum wage approach		Observed wage approach		Predicted wage approach	
	Gross money income (€/m)	Home production (€/m)	Extended income (€/m)	Home production (€/m)	Extended income (€/m)	Home production (€/m)	Extended income (€/m)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Period = 2012	-244.9	386.9	141.9	841.2**	596.3	-208.2	-453.1
	(293.9)	(510.9)	(455.5)	(367.0)	(473.6)	(329.2)	(371.3)
Mothers child < 13	-123.9**	-0.440	-124.3	-51.83	-175.7	-108.4	-232.3*
	(57.68)	(57.76)	(91.07)	(124.7)	(139.5)	(94.30)	(124.8)
Treatment	46.57	167.7**	214.3**	205.3*	251.9*	260.0*	306.6*
	(67.75)	(71.44)	(102.7)	(110.0)	(134.6)	(136.4)	(158.5)
Age	26.18	-417.7*	-391.5	-707.7***	-681.5**	-979.8**	-953.6**
	(130.6)	(235.7)	(246.2)	(228.5)	(271.2)	(448.8)	(467.8)
Age squared	532.2	2,749	3,281	4,159*	4,692*	11,880**	12,412**
	(1,038)	(1,791)	(2,140)	(2,197)	(2,493)	(4,751)	(4,926)
Single	338.2***	-214.3*	124.0	-294.3	43.95	-167.1	171.1
	(129.8)	(117.9)	(191.4)	(199.1)	(264.8)	(222.1)	(297.8)
Period = 2010	-48.79	164.7	115.9	350.1***	301.3*	5.072	-43.72
	(96.86)	(170.8)	(153.3)	(131.6)	(165.3)	(111.7)	(125.9)
Constant	-637.8	12,777*	12,139*	22,237***	21,599***	20,674*	20,036*
	(4,251)	(7,603)	(7,344)	(6,363)	(7,841)	(10,567)	(11,126)
Individual FE	yes	yes	yes	yes	yes	yes	yes
Time FE	yes	yes	yes	yes	yes	yes	yes
Observations	1,011	1,011	1,011	1,011	1,011	1,011	1,011
R-squared	0.025	0.042	0.025	0.020	0.018	0.079	0.071
Persons	337	337	337	337	337	337	337

4.4 Sensitivity tests and additional specifications

We now conduct a number of sensitivity tests to assess the robustness of our results. The regression output is shown in Table 3.

For our first test, we examine whether there is endogenous panel attrition – that is, if our results change when we allow individuals not to be observed in all waves. As can be seen from the table below in Columns 1-3, this does not affect the sign or size of the coefficient. Extended income based on the predicted wage approach becomes significant at the 5% rather than at the 10%. Also the size of the coefficients is not much affected, though now it seems that the observed wage rather than the predicted wage approach yields the strongest effects on extended income.

Our second set of sensitivity tests looks at net income instead of gross income. The monetary value of home production is added to the net income reported in the LISS data. Again all results correspond to our earlier results. As with gross money income, we do not find evidence for an effect of the policy change on the net money income position of mothers as such (Column 4). This is surprising given the substantial spending cut of the policy change. One possibility is that the affected mothers were compensated for this by their employers through a higher hourly wage. As we cannot directly observe at the margin, we cannot really test for this. We still find extended income increasing effects of the policy change when looking at net rather than gross extended income (Columns 5-7).

Table 3: Sensitivity tests for monetized income

	Gross extended income, unbalanced panel (€/m)			Net money income, balanced (€/m)	Net extended income, balanced (€/m)		
	Minimum wage approach	Observed wage approach	Predicted wage approach		Minimum wage approach	Observed wage approach	Predicted wage approach
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Period = 2012	161.8	763.1	-413.7	-199.2	187.7	642.1*	-407.4
	(400.9)	(522.9)	(329.6)	(195.2)	(418.1)	(367.3)	(297.6)
Mother child $<$ 13 y.o.	-123.6*	-137.5	-221.7***	-81.32**	-81.76	-133.1	-189.7*
	(63.14)	(91.42)	(82.52)	(36.38)	(73.06)	(131.1)	(109.3)
Treatment	167.2**	318.6*	265.2**	15.11	182.8**	220.4*	275.1*
	(80.65)	(172.5)	(116.5)	(40.87)	(84.30)	(120.0)	(145.9)
Age	-346.7*	-856.0***	-885.9***	52.38	-365.3	-655.3***	-927.4**
	(194.5)	(286.0)	(333.3)	(84.63)	(224.4)	(238.5)	(453.6)
Age squared	2,826*	6,243**	11,739***	130.4	2,879	4,290*	12,011**
	(1,582)	(3,068)	(3,514)	(639.1)	(1,928)	(2,317)	(4,829)
Single	165.2	130.5	170.3	235.6**	21.32	-58.68	68.47
	(152.7)	(202.4)	(219.1)	(93.85)	(164.4)	(234.3)	(272.7)
Period = 2010	117.0	351.2**	-42.37	-44.99	119.7	305.1**	-39.92
	(134.8)	(171.8)	(111.7)	(64.44)	(140.9)	(132.2)	(102.0)
Constant	11,143*	25,999***	18,412**	-1,386	11,391*	20,851***	19,288*
	(6,106)	(7,936)	(8,114)	(2,806)	(6,720)	(6,548)	(10,563)
Individual FE	yes	yes	yes	yes	yes	yes	yes
Time FE	yes	yes	yes	yes	yes	yes	yes
Observations	1,567	1,567	1,567	1,011	1,011	1,011	1,011
R-squared	0.018	0.016	0.080	0.029	0.026	0.017	0.075
Persons	615	615	615	337	337	337	337

Earlier we discussed the criticism on overreporting of hours spent on home production in recall data surveys (Aguiar et al., 2012). This would only affect our results when the treatment group would overreport more than the control group after the policy change. This seems unlikely. Regardless, we are able to generate all our results when we top code the hours spent on home production using the 99th percentile (66 hours of home production per week; results not shown here). Leaving out mothers with a 2011-born baby does not change the results.

A related question is where the time comes from that mothers use to increase their home production when facing an income shock. In Table 4 we show that we do not find that our treatment group reduced their hours spent on market work as discussed already (Column 1). The same holds for childcare at home (Column 2), for leisure (Column 3), and for sleeping. One explanation for these results is that mothers increase their home production whilst doing other things such as childcare. Unfortunately the LISS dataset does not contain information on whether time spent on home production is combined with time spent on something else. Also, as we saw in our descriptives, the amount spent on home production went down on average, but less so for our treatment group. This might ease the time crunch.

Table 4: Time spent on other categories

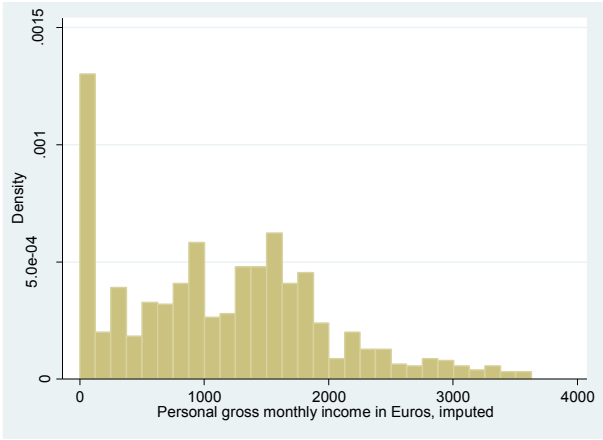
	(1) Market work (h/m)	(2) Childcare at home (h/m)	(3) Leisure (h/m)
Treatment	-1.82	2.14	-4.1
	(7.47)	(8.28)	(11.29)
Observations	1,011	1,006	1,009
R-squared	0.018	0.180	0.027
Persons	337	337	337

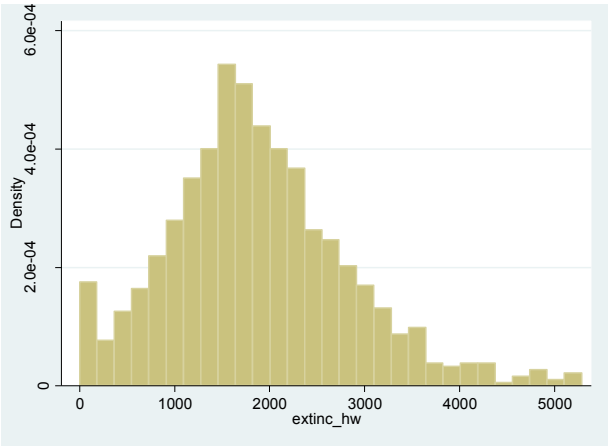
Last, we reflect on the level and development of inequality for our income concepts. This allows us to gain insight into the heterogeneity of responses of mothers when facing an income shock. For now we restrict our analysis to the predicted wage monetizing procedure as advocated by Frick et al. (2012).

A first result is that the level of inequality in gross money income is higher than for extended income, both before and after the policy change, for both the control and treatment group. This finding corresponds to prior literature showing that extended income is more equally distributed than money income (Frazis & Stewart, 2011; Zick et al., 2008; Frick et al., 2012; Folbre et al., 2013). Figure 1 (note the different scales on the y-axis) shows this equalising effect of home production. First of all, there is hardly any mother with a zero extended income level. This implies that mothers not supplying any labour keep up their consumption using home production. More generally, the distribution is more normal-shaped or less flat, which suggests a negative correlation between market income and home production.[1]

[1] The correlation coefficient for our subgroup is -0.31.

Figure 1: Distribution of gross money income and extended income (predicted wage approach)





In Table 5 we quantify the distribution using the Gini index as our measure of inequality for the treatment and control group before and after the policy change. We do not formally quantify the differences in inequality scalar levels we find here, but use qualitative terms for now. The results again confirm that extended income is much more equally distributed than gross money income. The difference is very large – between 10 to 15 Gini points. That is comparable to the decrease in inequality in market to disposable income by taxes and transfers for the U.S. in the mid-2000s, which is 15 Gini points (see LIS figures) – in the Netherlands the state is more redistributive (24 Gini points). Frazis & Stewart (2011) find that (1) hours of non-market work are more evenly distributed than hours of market work; (2) they vary relatively little in terms of market value; and (3) are negatively correlated with hours of market income. Our results indicate that inequality in home production indeed is lower than inequality in gross money income. However, we find that inequality in monetized home production as such is substantially higher than inequality than inequality in gross money income, shedding doubt on their second claim. Their third claim seems the most important, as we find that summing monetized home production to gross money income, reaching extended income, leads to a much more equal distribution. This is also reflected in a negative correlation between hours of market income and home production discussed earlier.

With our figures we can also tell something about the dynamics of extended income inequality; a topic rarely covered in the literature (as far as we know except for Gottschalk & Mayer, 2002). We ignore inequality in monetized home production (the third row), as the dynamics are the same as for inequality in hours on home production (the second row). We find that for the treatment group, inequality in gross money income went up, whilst inequality in home production stayed the same. This led to a marginal increase in extended income inequality. For our control group we find quite different patterns. Gross money income inequality went down rather than up, whilst inequality in hours spent on home production went up rather than remained stable. The outcome was the same, however, a marginal increase in extended income inequality, this time not a consequence of an increase in gross income inequality but of an increase in hours on home production inequality.[2] All in all, for our specific group of mothers with children we find that the distribution of extended income and the distribution of gross money income can follow disjoint trajectories, which is opposite to the findings of Gottschalk and Mayer (2002) who find that both extended income and money income inequality went up relatively comparably in the US for the four years they cover.

What can be inferred from this at the micro level? We have to be careful here, since we now move to a cross-sectional comparison, whereas we saw in our regressions that we really want to focus on the within-person shifts over time. Bearing this in mind, from our cross-sectional perspective it seems as if there are mothers that increase their gross money income via changes in market hours or hourly wage when faced with an income shock. The increased heterogeneity within the treatment group is particularly striking when compared to the control group where gross income inequality went down. This increased heterogeneity within the treatment group might explain why we do not find an average effect for gross money income.

Table 5: Gini coefficients for gross income, home production, and extended income

	Treatment			Control			Diff-in-diff
	2009-2010 (1)	2012 (2)	Difference (3)	2009-2010 (4)	2012 (5)	Difference (6)	(7)
Gross money income (€/m)	43.8	45.4	Increase	39.7	37.4	Decrease	Increase
Home production (h/m)	36.7	36.8	Same	34.2	36.3	Increase	Decrease
Home production (predicted) (€/m)	56.5	56.7	Same	41.6	47.8	Increase	Decrease
Extended income (predicted)	29.6	30.4	Increase	26.0	27.0	Increase	Same

For the control group we find that home production inequality is going up. Combined with our previous finding that hours spent on home production went down substantially on average (see Table 1), this seems to suggest that those mothers with a child above 12 that were spending below-average numbers of home production decreased their hours of home production the most. For the treatment group,

[2] Or a change in correlation over time, but we do not have theoretical reasons to expect this.

however, we find a decline in home production across the entire distribution, and a less steep average decline.

5. Conclusions

A key question in social sciences is how to assess the welfare position of individuals. This holds both for gaining insight into the evolution of living standards, for inequality matters, as well as how individuals cope with volatility and shocks. By and large, the literature has been preoccupied with money income, generally ignoring the role that home production can play at different stages of this. There have been studies looking at extended income to see how this affects the distribution, or how home production can play a substitutive role in volatile times for individuals, but these approaches have not been married. This is what we do in this paper, and in addition to this, we use a difference-in-difference approach to make causal claims. The immediate consequence, however, of this approach, is also that the population that we cover in our study is limited: it only speaks to mothers aged 21-50 with a child aged below 18. It is important to keep in mind that we cannot generalise much from our specific group (see also Chapela, 2011).

We first show that home production adds to a large extent to the consumption possibilities of our group of individuals. Extended income is in between 42 and 86% higher than gross money income, depending on the group, period, and monetizing procedure. Moreover, as has been documented in the literature previously, extended income is substantially more equally distributed than gross money income with a Gini index of in between 26 and 30.4, compared to 37.4 to 45.4 for gross money income. This is at least partly due to a negative correlation between level of market income and hours spent on home production.

With our individual panel dataset we are able to move beyond cross-sectional averages and look at within-individual change over time when faced with an income shock. For this, we exploit a policy shock that only affected women with a child aged below 12. Comparing their extended income to the extended income of women with a child aged above 12, who were not affected, we find causal evidence that home production plays a money income smoothing role. Our income shock did not affect net and gross money income in our data; however, it did increase home production – or more exact, led affected mothers to reduce their home production less so than our control group. In fact, the increase (lower reduction) in home production seems to be so pronounced that we find positive effects on the extended income position of affected mothers regardless of which monetizing method we use.

Our finding that mothers negatively affected by a policy change alter their behaviour in such a way that they become richer in terms of their extended income might indicate overcompensation. This is a non-trivial finding, as we show further that market production or time spent on childcare are unaffected. A second explanation would be that the monetizing procedures are not accurate enough. However, we find comparable results using three monetizing procedures, even in terms of size of coefficients. Moreover, with our difference-in-difference design, this would only matter if all monetizing procedures for some reason make the treatment group richer relative to the control group, which is not obvious.

We have a number of future plans lined up. We plan to include an appendix on fathers – first tests indicate that this group does not change its behaviour in home nor market production. Second, we can move to the household level, assuming individuals pool their income in their household. Then for this group we can apply equivalence scales etc (see Folbre et al; 2013; Frazis & Stewart, 2011). Essentially we then combine the being single or not and the substitution part to examine the total effect on consumption possibilities for people. It would be interesting to obtain a better understanding of which people change their behaviour, in particular as our cross-sectional inequality results indicate that there is heterogeneity in our sample. One way would be to look at individuals who are time crunched, for instance by including levels of market work or home production and see whether there is convergence. However, this introduces endogeneity and is not easily done within our difference-in-difference estimation. We might also enquire into the monetizing procedures, for instance, by exploiting a wellbeing approach given that we have questions on satisfaction whilst doing home production, market work, and overall life satisfaction.

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