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Abstract

We study the impact of a nation-wide unconditional cash transfer program on labor supply in Iran. In 2011, the government started monthly deposits of cash into individual family accounts amounting to 29% of the median household income. We use panel data and fixed effects to study the causal effect of the cash transfers on labor supply using the exogenous variation in the intensity of treatment, which we define as the value of cash transfers relative to household income in the year before transfers. We also use a difference-in-differences methodology that relies on exogenous variation in the time households first started receiving transfers. With the exception of youth, who have weak ties to the labor market, we find no evidence that cash transfers reduced labor supply, while service sector workers appear to have increased their hours of work, perhaps because some used transfers to expand their business.

JEL Classifications: D13, I38, J22, O12

Keywords: Cash transfers, labor supply, Iran, impact evaluation

ملخص

نقوم بدراسة تأثير برنامج التحويلات النقدية غير المشروطة على نطاق البلاد على إمدادات العمالة في إيران. في عام 2011، بدأت الحكومة ودائع نقدية شهرية إلى حسابات عائلية فردية بلغت 29٪ من متوسط دخل الأسرة. نستخدم بيانات المسوحات والآثار الثابتة لدراسة التأثير السببي للتحويلات النقدية على عرض اليد العاملة باستخدام الاختلاف الخارجي في شدة العلاج، والذي نحدده كقيمة التحويلات النقدية المتعلقة بدخل الأسرة في السنة السابقة للتحويلات. كما نستخدم منهجية الاختلاف في الاختلاف التي تعتمد على التباين الخارجي في الوقت الذي بدأت فيه الأسر في تلقي التحويلات. وباستثناء الشباب الذين لديهم روابط ضعيفة بسوق العمل، لا نجد أي دليل على أن التحويلات النقدية خفضت المعروض من اليد العاملة، في حين يبدو أن العاملين في قطاع الخدمات قد زادوا ساعات عملهم، ربما لأن بعضهم استخدم التحويلات لتوسيع أعمالهم.

1. Introduction

A central question in the public debate on income assistance is the potential negative effect of transfers on the labor supply of the poor. Assuming that leisure is a normal good, economic theory predicts that an increase in unearned income reduces labor supply. In advanced market economies the potential disincentive effects of welfare programs has been widely studied and fostered key welfare reforms (Atkinson and Mogensen 1993; Mott 1992; Mott 2002). There is a much smaller literature on the negative effects of cash transfers on labor supply in developing countries (Bosch and Manacorda 2012; Abel 2014). In developing countries these programs are often used to fight poverty and promote health and education, so their potential negative impact on labor supply is of secondary concern. In contrast, income assistance programs in advanced countries are ongoing programs providing social protection to individuals unable to earn enough from market work, so it makes sense that their impact on incentives to work should be very important.

In this paper we study a large cash transfer program in a developing setting, one that has come under criticism for its potential negative labor supply effect. In December 2010, as part of an ambitious reform of bread and energy subsidies, Iran started a monthly cash transfer program to compensate households for the price increases (Guillaume et al. 2011; Salehi-Isfahani et al. 2015). In 2011, the first full year of the program, [1] transfers amounted to 6.5% of the GDP and about 29% of the median household income. After three years of inflation, the amount transferred per person is down to less than 3% of GDP per capita, but it is still one of the largest cash transfer programs in the world. In sub-Saharan Africa cash transfers per person have reached up to 40% of GDP per capita (Garcia, Moore, and Moore 2012), but because of their smaller targeted population they are much smaller in overall size. Iran's transfer program has been praised as innovative, free of leakage, and efficient as a way to distribute Iran's natural wealth among its citizens compared to subsidized energy (Guillaume et al. 2011). Although it was not specifically intended to reduce poverty, and its real value has declined since its inception due to inflation, it has contributed significantly to lowering poverty and income inequality (Salehi-Isfahani 2016). The transfer program seems popular with the poor, but not with the middle class who blame it for the high inflation rates subsequent to its implementation. The rise in inflation, from 12.4% in 2010 to 34.7% in 2013 has other causes, most importantly energy price increases ranging from 100% for bread to 9-fold for diesel fuel. The cash transfer program is also unpopular with the current Rouhani administration, which inherited it from the previous administration led by former president Mahmoud Ahmadinejad. The Rouhani government has fixed the deficit in the transfer program by raising energy prices again, and at the same time reduced inflation by two-thirds, but it remains sharply critical of the program because of its alleged potential negative effect on the labor supply of low income individuals. No evidence has been produced to support a negative effect, but press reports abound of poor workers leaving their jobs and small farmers abandoning their farms in droves after receiving cash transfers. [2] Senior Iranian politicians opposed to Ahmadinejad have criticized the program as handouts that "foster beggars", implying an adverse impact on the labor supply of the poor[3].

We use a rich panel of households observed before and after the program to examine the impact of cash transfers on labor force participation, employment, and hours of work of Iranian men and women. Identification of the causal impact of the program is important because of other shocks to the economy coinciding with the launch of the cash transfer program, mainly as a result of the tightening of international sanctions starting in July 2011. It is therefore very

[1] Unless otherwise noted, Gregorian calendar years stand for Iranian calendar years. For example, 2010 stands for Iranian year 1389 but spans the period from 21 March 2010 to 20 March 2011.

[2] See, for example, Khajehpour (2013), who wrote of "500,000 to 700,000 jobs lost in the agricultural sector due to cash handouts." Similarly, a senior economic adviser to the Rouhani government asserted that many rural workers had withdrawn from work as a result of the program (interviewed in *Tejarat Farda*, no. 67, November 2013).

[3] See https://lobelog.com/irans-presidential-election-to-put-populism-on-trial-2/

difficult to attribute changes in labor supply after the program to cash transfers. In addition, the nominal amount of transfers was the same for all individuals. In order to identify the effect of the transfers on labor supply, we take advantage of two sources of variation in the value of transfers. One is the variation in the intensity of treatment as measured by the share of net benefits (cash transfers minus higher energy bills) in total household expenditures. In order to avoid endogeneity of income to labor supply, we use total household expenditures from the year before the program. We also use the variation in the timing of registration for the program. For a variety of reasons, mostly unrelated to labor supply (e.g, loss of birth certificates, having to prove headship of the household, etc.) roughly 30% of the population had to wait three months before receiving cash transfers after the start of the program (Salehi-Isfahani et al. 2015). We employ difference-in-differences for early and late recipients to estimate the average program impact on hours worked and labor force participation.

For the most part, we focus on the labor supply of poorer workers, who are more likely to reduce their labor supply as a result of a modest increase in unearned income. Our results do not indicate a negative labor supply effect for either hours worked or the probability of participation in market work, either for all workers or those in the bottom 40% of the income distribution. We do find a negative labor supply effect for workers 20-29 years old for their hours worked. This is not surprising since the attachment of Iranian youth to the labor market is weak and many have the option to enroll in tertiary and graduate education, so it makes sense for some to reduce their labor supply as a result of cash transfers. We are not able to link the labor supply of individuals to their time use, so we cannot say if the cash transfer is financing human capital formation or mere leisure, two responses with very different implications for economic growth.

The fact that we do not find a negative impact does not contradict the strong prediction of economic theory regarding the negative labor supply effect of unearned income, nor the weight of empirical evidence from developed countries regarding the negative incentive effect of means tested programs. Iran's cash transfer scheme is not conditioned on income or wealth and is universally applied, though since 2016 the government is finding ways to drop people off the roll who it determines as high income. Furthermore, the prediction of economic theory is based on perfect markets for labor and credit, neither of which apply to Iran where both jobs and credit are rationed. Unemployment has been in double digits for decades, and the marginal utility of leisure for the poor may be too low for relatively small increases in unearned income to cause a reduction in their labor supply. Individuals, especially the poor, are severely credit constrained, which the infusion of cash can relieve and open up new opportunities for investment and consumption that were not possible before. As a result, we consider the impact of Iran's universal and unconditional cash transfers on labor supply to be an empirical question.

Our paper is related to three distinct areas of research. The first is the rich empirical literature on the effect of unearned income on labor supply. One particular brand examines the effect of lottery winning on employment (Imbens, Rubin, and Sacerdote 2001; Sila and Sousa 2014; Picchio, Suetens, and van Ours 2015), and finds only small negative effect from large windfall gains. The negative effect in such cases could come from an increase in the marginal tax on wages of the winners rather than from unearned income itself. In our case, because personal income taxation in Iran is limited to workers in the public sector and in large private enterprises, we do not expect such an effect on labor supply. Second, our paper contributes to the literature on the impact of cash transfers in developing countries. This literature rarely addresses labor supply issues, perhaps because most cash transfer programs are conditional, for example on school enrollment, and impact evaluations are therefore more concerned with whether they reach their stated objectives than if they reduce labor supply (see, for example, Case 2004, Bosch and Manacorda 2012, and Schultz 2004, Evans and Popova 2014, Department for International Development 2011). More recently unconditional cash transfers have become

popular, because they seem to improve the welfare of the poor without the added cost of monitoring (Haushofer and Shapiro (2013), Blattman, Fiala, and Martinez (2013) and Blattman and Niehaus (2014), Aker (2013) Baird et al. (2014)). Lack of conditionality implies greater freedom on the part of recipients to change their behavior, including working less, though this literature, too, ignores labor supply issues. There is, however, indirect evidence of labor supply effects of cash transfers, implied by the observed response of income and consumption to cash assistance, generally indicating a positive effect (Bosch and Manacorda 2012). Haushofer and Shapiro (2013) examine the impact of an unconditional cash transfer program in rural Kenya and find that recipients consumed more food, healthcare, and education compared to the control group who did not receive a transfer. They also found that recipients increased their assets, in the form of home improvements and increased livestock holdings. Blattman et al. (2013) and Blattman and Niehaus (2014) provide evidence of unconditional transfers in Uganda, where transfers did not dissipate into unproductive activities. Bosch and Manacorda (2012), who also address labor supply effects of income assistance do not find a negative effect. Where significant negative effects are found, as in the case of Brazil's rural pension program (de Carvalho Filho 2008), payments are linked to income and therefore related to labor supply directly. Iran's program differs in this respect because during the period under study it was universal and did not condition on income.

Finally, our paper is closely related to the literature on the so-called Direct Distribution Mechanisms (DDMs) and the oil-to-cash initiative. Direct distribution of income from mineral exports has been proposed as a way to reduce corruption and rent seeking in oil-rich countries by making the average citizen the first recipient of all the mineral revenues, which are subsequently taxed to finance public expenditures (Gupta et al. 2014; Moss et al. 2015; Gillies 2010; Diamond and Mosbacher 2013; Sala-i Martin and Subramanian 2008; Rodriguez et al. 2012). The proponents of this initiative argue that doing so would reduce the power of the state over its citizens, help establish the institutions of taxation as foundation for a democratic society, as well as cut down on rent seeking and corruption. The oldest such program is from Alaska (Goldsmith 2010). More recently the oil-rich countries of the Persian Gulf, such as Saudi Arabia, Kuwait, Qatar and the United Arab Emirates have adopted similar programs offering their citizens monthly cash transfers costing them some $150 billion. [4] Little is known about the labor supply effects of these programs, but the low labor force participation of youth and women in these countries suggests that the disincentives for labor supply may be significant (Ross 2012). Iran's program bears some resemblance to these programs, though it was initially designed as a replacement for subsidized energy. Nevertheless, Iran's program is a good test case for this initiative because, whatever the intention of its designers, it was set up to reach all citizens without interference by the state.

The next section describes the Iranian context, the trends in economic growth and labor market outcomes before and after the program went into effect using aggregate trends. Section 3 describes our conceptual framework, and Section 4 explains our data and how we construct our panel of households and individuals, as well as the extent of sample attrition. Section 5 provides a detailed description of the program and its specific features that inform our identification strategy. Section 6 presents our empirical results, and section 7 concludes.

2. The Setting

The most challenging part in identifying the labor supply effect of Iran's program is that only months after it started, the country was hit with tough international sanctions and the economy entered a period of turmoil. The recession that followed the tightening of sanction in July 2011

[4] See, "To Stave Off Arab Spring Revolts, Saudi Arabia and Fellow Gulf Countries Spend $150 Billion." Knowledge at Wharton, http://knowledge.wharton.upenn.edu. Diamond and Mos-bacher (2013) dismiss the cases of oil-rich Arab countries as contrary to the oil-to-cash vision because oil money first goes to the state which then hands it out in a manner that strengthens rather than weaken its rule.

makes inference of program impact from aggregate trends in GDP and employment very unreliable. It also makes the construction of the labor market counterfactual – how labor supply would have looked like in the absence of cash transfers – very difficult. In 2011, when the transfer program started, Iran had enjoyed several years of oil-induced economic growth. During 2005-2010, prior to the transfer program and the intensification of international sanctions, non-oil GDP, which reflects the level of economic activity more closely than GDP with oil, grew at about 5% per year; in subsequent years its growth reached zero and then became even negative (see Figure 1). However, during 2011, the first year of the program, thanks to the infusion of cash non-oil GDP continued to grew, led by industry and services, which grew by 6.4% and 5.8%, respectively. Only a year later, in 2012, industry, which was hardest hit by sanctions, declined by 8.1%. Agriculture, where workers suspected of leaving their jobs as a result of cash transfers, is mostly affected by the weather and is therefore the most volatile. In 2011, its value added did not change significantly, after the program was in effect, but in the following two years grew by 3.7% and 4.7%. These output fluctuations, which affect employment, suggest that changes in labor market indicators cannot be attributed solely to the supply side of the labor market.

A negative impact from cash transfers is equally hard to pinpoint from labor market trends in Figure 2, which depicts quarterly movement in labor force participation, employment rate, and hours worked for workers with less than high school, the group most likely to be affected by cash transfers. In Iran, because of the large role of the public sector, the relationship between output and employment is not strong to begin with, but the decline in employment and participation starting in the second half of 2011, shortly after cash transfers had started but while the non-oil GDP was still growing, suggests a reduction on the supply side of the labor market. But the trends lend themselves to a different reading as well. Comparing the first quarter of 2011, the start time of cash transfers, with the same quarter in 2010, both employment and participation increased (by 10.2% and 6.7%). Also, except for the second quarter of 2011, there is a downward trend that begins a year earlier, in the second quarter of 2010. This decline may be related to a different income effect, the oil price effect that Ross (2008) associates with low labor force participation of women in oil-rich countries. There may still be other causes, such as surge of imports from China that competed with Iran's labor intensive textiles industries, as well as import of cheap capital goods that reduced demand for labor while output continued to grew.

3. Conceptual Framework

Economic theory has a strong prediction for the negative effect of unearned income, but this prediction is considerably weakened by the presence of rationing in the markets for labor and credit. In Iran, both types of rationing are present, suggesting that the labor supply response to unearned income may not necessarily be negative. Rationing in the labor market is particularly prevalent in the market for formal work, both in public and private sectors, where jobs are highly sought after and employees are therefore strongly attached to their jobs. Workers in these sectors are therefore very unlikely to withdraw from employment as a result of modest amounts of cash assistance. In our sample, hours worked by wage and salary workers in the public sector, the most desirable types of jobs, did not decline between 2010 and 2011 and hours for those with less attachment actually increased (see Table 2).

Rationing in the credit market is even more severe. Loans to small enterprises and to finance consumption are very rare. Cash transfers can relieve the credit constraint of lower income individuals and thereby increase their labor supply. Transfers may enable women with small children to pay for child care and increase their labor supply (Abel 2014). Self-employed workers may be able to finance the expansion of their micro and small enterprises and increase their hours of work. The presence of credit constraints is also relevant for our estimation method. DID uses the timing of transfers to identify program impact. Without a borrowing

constraint, the timing would not matter and those who received cash early or later would experience the same change in total wealth and therefore have the same labor supply behavior.

A final conceptual note is about the level at which labor supply decisions are made. These decisions can be made at the individual or the household level with different implications for response to unearned income (Blundell and MaCurdy 1999; Chiappori 1988). Households often redistribute hours among members in response to shocks (Blundell et al. 2007). We do not know much about how such intrahousehold allocations are made in Iran, but in this paper we focus on the behavior of individuals rather than households and assume that households do not constrain their members' labor supply decisions. We treat an increase in household unearned income as affecting the labor supply of all household members. Individuals living in households with a larger ratio of transfers to income would behave differently than the same individuals living in a household with a lower ratio irrespective of the composition of the two households.

The households in our sample have on average 1.2 working members and about 20% of households have 2 or more working members. In 2011, 98% of cash transfer recipients were head of households, suggesting that at least in registering for the transfers the household acted as a unitary decision maker since the program allowed for adult members to apply for transfers directly. Furthermore, 82% of working men 20-59 year old in our sample are household heads, so the correspondence between transfer recipients and decision makers is close. Only about 3% of workers who resided in the same household decided to get the transfer directly. Among them, the largest group was married sons by far. There are legitimate questions of intrahousehold allocation of labor supply that arise in the context of Iran's cash transfer program. For example, the transfer may make it possible for a household member to enroll in school while another increases his or her labor supply to compensate. In this situation, a regression of individual labor supply might reveal a positive or negative supply response when at the household level it is zero. We ignore such interdependence in the labor supply of household members.

4. Data

Our data are derived from two rounds of the Household Expenditures and Income Survey (HEIS), 2010 and 2011, [5] which has been collected annually by the Statistical Center of Iran (SCI) since the 1960s. HEIS is a nationally representative, two-stage stratified (urban-rural and by province) data. The households in the sample are randomly divided into 12 groups of roughly equal size, and interviewed in different months of the year. Starting in 2010, HEIS is collected as a rotating-panel and households were interviewed the same month each year, so in the panel estimation we can ignore the month of interview. However, since the program began on the tenth month of the Iranian year 1389 (December 2010), we restrict the sample to specific months of the year, the first 9 months in the fixed effects estimation and the last three months for the DID. Rotating panels are used primarily to reduce year to year fluctuations and to make consecutive year samples more similar. Because their primary aim is not collecting panel data, households are not followed if they relocate. Of the 152,291 individuals (38,285 households) in 2010, 104,703 or 68% of the sample (26,180 households) were randomly selected and designated as panel to be re-interviewed in 2011, and the rest were designated to rotate out after one year. Of the non-rotating group, 68,925 individuals were actually found and re-interviewed the second year.[6] These form our balanced panel with an attrition rate of 34%. Besides attrition, the panel suffers from weak identification of household members between

[5] In this paper, we use Gregorian years while the actual survey period is in Iranian years from March 21 to March 20. For example, year 2010 refers to the survey period between 21 March 2010 to 20 March 2011, and the last quarter of 2010 corresponds to the first quarter of 2011, and so on.

[6] In addition to those identified by the survey as having attired, we excluded another 11,290 individuals because their age had changed by more than two years or their gender changed.

rounds. If an individual leaves the household, he or she is dropped from the sample and his or her ID is given to the next member. To guard against mismatch of individuals across years, we drop an additional 2,823 households (comprising 22,408 individuals) whose membership had changed from one year to the next, leaving us with 46,517 individuals in intact households (or 11,631 intact households) in the panel, or 67% of the original panel. In addition, we checked for consistency of our matching method using age, sex, and relation to head of household. Our intact sample consists of 84% men and 16% women.

Table 3 presents the summary characteristics of the intact panel and compares them to the 2010 base sample. These statistics do not show a large difference between the two samples suggesting that the constructed panel is representative for the whole population. Specifically, the mean and standard deviation of hours of work per week, a variable of interest in this study, is very similar between the two samples. The mean value of participation, the other variable of interest, is slightly different between the two samples, however the difference is not significant and the difference is reduced once we control for observables.

Attrition in panel-data is important if the observations that drop out of the sample differ systematically from those that remain. In our case, attrition is high (45%) and appears selective. It is higher in urban areas, among renters, and higher income families (see Table 4). The employment status of the head of the household and the number of employed household members are also correlated with attrition (those with more working members are less likely to attrit). A test of whether attrition is random or not, offered by Becketti et al. (1988), rejected the randomness of attrition, so following Fitzgerald et al. (1998) we re-weight our observations according to the inverse probability of attrition calculated from a probit of attrition status on relevant household characteristics. We use these weights along with the probability weights provided by HEIS in the summary tables as well as in regressions. Our regression results are not changed by much if we do not use the attrition weights.

Another view of the changing labor market conditions before and after the cash transfer program is from the transition matrix for employment status of individuals using the 2010-2011 panel (Table 5). This matrix shows the proportion of individuals in each employment status (employed, unemployed, and inactive) in 2010 and 2011, and indicates a fair amount of stability in activity status. Of the individuals employed in 2010, 88.5% remained employed, 4.5% lost or quit their jobs (became unemployed), and the rest became inactive (2% who retired, 1% enrolled in school, and 4% returned to housework) in 2011. Of the unemployed, 26.3% (440 individuals) found work in 2011, about the same number (434) who lost their jobs in 2011. Of those engaged in housework in 2010, 260 or 3.2% found jobs in 2011, many fewer than those who left their jobs for housework (369).

5. Program Description and Identification of Impact

The cash transfer program was introduced in 2010 as compensation for the removal of bread and energy subsidies, estimated at $50-$60 billion, about 15 percent of the GDP (Guillaume et al. 2011). The legislation supporting the program was passed by the Iranian parliament in January 2010, but the law was not implemented until December of that year, when the government raised prices of bread and energy products by factors ranging from 2 to 9 and simultaneously released the cash it had deposited in dedicated household bank accounts.[7] Transfers were universal and made directly to individual bank accounts. Initially, the plan was to compensate only the households in the bottom one-third of the income distribution, but because identifying them proved administratively impractical, the government decided to pay everyone. Given the size of the price hikes, the transfers were critical in preventing a large negative income shock to households, and may have forestalled potential social unrest that

[7] For a more detailed description of the program and its implementation, see Guillaume et al. (2011), Tabatabai (2011), Salehi-Isfahani (2016), and Salehi-Isfahani et al. (2015).

often follows much less severe energy price adjustments (Harris 2010; Bacon and Kojima 2006; Beaton and Lontoh 2010).

We take advantage of two features of Iran's cash transfer program to identify its impact, its differential impact on households with different incomes and the fact that registration for the program was closed before everyone could register and re-opened three months later. Cash transfers are exogenous shocks to household resources, but do not affect all households equally. Although all individuals received the same amount of transfer, household budget constraints shifted at different rates. We define the *intensity of treatment* as the ratio of transfers (net of the increase in energy expenditures) to household expenditures before the program started. We use expenditures of the last year, because the same year's expenditures can be endogenous as households adjust their hours of work, and therefore their incomes, to transfers received. The variation in this measure is substantial and is potentially a good source of identification of the program impact. Transfers amounted to 4.9% of per capita expenditures for individuals in the top quintile of the per capita expenditure distribution (net of transfers), compared to 49.3% for those in the bottom quintile (see Table 6).

The intensity of treatment thus defined is likely to be correlated with unobserved individual characteristics that affect labor supply and thus create a correlation between treatment intensity and the error term. We use fixed effects estimation, which reduces the bias from this source because it compares changes in individual labor supply with different intensities of treatment. We complement the fixed effect results with a difference-in-differences method using the variation in the time of program registration. To get the transfer, heads of households had to open a bank account and provide birth certificates for all their household members. Women who claimed to be household heads had to provide proof of divorce or their husband's death. For various reasons, about 30% of the population who did not register in time had to wait three months for the registration system to re-open. This variation in timing of program participation helps define two groups of transfer recipients based on the timing of registration. One group consists of early participants, who started receiving cash transfers in winter 2011 and continued to receive them in 2012. The second group consists of late participants who registered after March 2011 and therefore received cash transfers in winter 2012 but not in the same quarter in 2011. The former group was in the same position before and after transfers whereas the latter group experienced an increase in transfers in the second relative to first period.[8] This variation offers the opportunity to estimate the program impact using difference-in-differences methodology.

For this strategy to identify the impact of cash transfers, a few assumptions are required. If the government's promise to continue the program for some time were taken seriously, and if credit markets functioned well, all else being equal the two groups would experience the same change in their permanent incomes and have identical reduction in their labor supply. We do not believe that either condition holds in the case we study. First, there was little reason to believe that the rules governing the distribution of money saved from removal of subsidies would not change. The Ahmadinejad government had already shown itself particularly inept in foreseeing problems when it suddenly abandoned its original plan to pay compensation only to the poor. Millions of people had filled questionnaire about their income and wealth only to be told they were not of any use. In another instant, it abandoned raising the value added tax when merchants went on strike and shut down the Tehran bazaar. There was no assurance that protests against price increases would not force the government to abandon the subsidy reform program and with it the cash transfers. Second, as in all developing countries, the poor have little access to credit (Gersovitz 1988). When they borrow, they either do so at exorbitant interest rates, or with collateral of equal value (Deaton 1997). Under these conditions, it would

[8] Data limitations do not allow us to define a comparison group that did not receive transfers in both years.

not have been feasible for the poor who did not receive cash in the first quarter of 2011 to reduce their labor supply and borrow for consumption based on the promise that they would receive the same amount in the future. On these grounds we believe that it is reasonable to assume that if there were any negative impact on labor supply as a result of the cash transfers we should be able to detect it in the change in the labor supply of late receivers relative to early receivers. Below we estiamte this effect using a difference-in-differences (DID) methodology.

Inference based on the DID estimates rely heavily on two assumptions. The first is that recipients in the winter quarter of 2011 (4th quarter of Iranian year1389) are correctly identified (see section 4), and second that, conditional on observable characteristics, the allocation of households to comparison and program groups is random. For the first assumption we rely on the evidence presented in Salehi-Isfahani et al. (2015), who used detailed information on unearned income as recorded in the 2010 survey to identify the early participants. Their estimate of the rate of non-participation based on survey data is within 5% of the rate announced by the government based on administrative data. Roughly a third of the individuals in our sample are late participants. The validity of the second assumption can be gauged from the summary statistics for the two groups presented in Table 7. The groups are similar in their main characteristics, though the program group is slightly older, poorer, and less educated. These differences are accounted for in the DID estimation by conditioning on individual and household characteristics.

6. Econometric Results

We report our results by labor market outcome (participation and hours worked) and by methodology (fixed effects vs. DID). We begin with the impact of cash transfers on hours worked per week.

6.1 Supply of hours worked

6.1.1 Fixed effects

We use a linear formulation of individual hours supply for our empirical estimation:

$$Y_{it} = \alpha_0 + \alpha T_{it} + \mathbf{X}_{it}\beta + \lambda_i + \theta_t + u_{it}, \qquad (1)$$

where Y_{it} is labor supply of individual i at time t, T is treatment intensity, $\mathbf{X}$ is a matrix that includes individual or family characteristics, λ_i is the unobserved individual effect, θ_t is the time effect, and u_{it} is the idiosyncratic error. Because treatment intensity is measured as the ratio of cash transfers (minus increased energy expenditures) to per capita expenditures, it is correlated with λ_i causing OLS estimates to be inconsistent. We therefore eliminate λ_i by first differencing the above equation to obtain the familiar fixed effects form:

$$\Delta Y_{it} = \alpha \Delta T_{it} + \mathbf{\Delta X}_{it}\beta + \Delta\theta_t + \Delta u_{it}, \qquad (2)$$

We make the standard assumption that the time trend $\Delta\theta_t$ is common and its addition to the error term is not a source of bias. This assumption underlies both fixed effects and DID estimation, where it is known as the parallel trends assumption. It is conceivable that changes in labor demand between 2010 and 2011 depend on income, in which case this assumption would be violated. We can check the validity of the parallel trends assumption in our case by examining the correlation of changes in labor market outcomes for different groups of workers before treatment started. In figures 3 and 4 we see the trends in the weekly hours of work and labor force participation rates during 2006-2010. These are quite similar for men in different income groups and for female participation, but not for hours of work for women. In general, we advise caution in interpreting the impact evaluation results for women because female labor

markets are highly segmented by income and education and it is quite likely that demand shocks are not uniform across groups of female workers.

The fixed-effects method also depends on the parallel trends assumption because in effect it uses households with small intensity of treatment (mostly the rich) as controls of those with higher intensities of treatment (mostly the poor), and therefore needs to assume that $\Delta\theta_t$ is the same for individuals with different treatment intensities. We obtain (but do not report) very similar results when we use a difference-in-differences regression on two groups of individuals with low and high intensity of treatment (the former acting as the control group). Fixed effect results on hours of work are presented in Table 8. [9] The estimate of program impact on weekly hours worked for men is positive and significant (column 1), but for women the estimate is not significant. The results for men seems surprising in view of the expectation of a negative supply response to unearned income. The coefficient of log unearned income itself (excluding cash transfers) is negative (and significant), as we would expect. For women, neither coefficient is significant and both are very close to zero (column 3). In columns 2 and 4 we added the initial value of the controls, many of which are time-invariant and therefore eliminated from the regression equation 2. The results do not change. If these results are valid, they reject the hypothesis that cash transfers had a negative supply effect.

6.1.2 DID

As noted above, the DID estimates are based on the comparison of change in hours worked for those who received cash transfers in two periods (winter quarters of 2011 and 2012) and those who received it starting in the second period. The standard formulation of the DID equation is:

$$Y_{it} = \alpha_0 + \alpha CT_i + \beta Year_t + \delta CT_i \times Year_t + \mathbf{X}_{it}\beta + \varepsilon_{it}, \qquad (3)$$

where CT is a dummy variable equal to one if the household received the transfer in the second period only. The difference in labor supply between the two groups is captured by α_0, β is the change in labor supply for the group that received transfer in both years, and δ is the program impact. X_{it} are individual and household characteristics that Salehi-Isfahani et al. (2015) find to be correlated with the probability of being a late recipient (treatment group) – mainly rural-urban residence, education, gender of the household head, age, log of unearned income, marital status and wage worker indicators. Our formulation of the DID equation is slightly different from the usual case because our comparison group is treated in both periods while the program group is treated only in the second period. It is easy to see that the standard DID regression identifies the impact of cash transfers with the same parameter δ of the interaction term in equation 3 (see table 8).

	Comparison	**Program**	
	(CT=0)	(CT=1)	Difference groups
Year=0 (2010)	α_0	$\alpha_0 + \alpha$	α
Year=1 (2011)	$\alpha_0 + \beta$	$\alpha_0 + \alpha + \beta + \delta$	$\alpha + \delta$
Difference years	β	$\beta + \delta$	δ

The DID results are presented in Table 9. These results are consistent with the fixed effects results, though the estimate of program impact (coefficient of Year × Treatment) is no longer significant. The year effect indicates a drop in the average hours worked for men but not women. The coefficient of the treatment dummy indicates that the male treatment group worked about 3 fewer hours in 2010 than the corresponding comparison group and the female group worked 1.4 fewer hours less, though these differences are not significant. The other noteworthy coefficients are the large and positive effects of education on hours worked for

[9] Throughout this section, we report Huber-White robust estimates of standard errors that adjust for failure to meet assumptions concerning normality and homogeneity of variance of the residuals.

women but not men. As in the fixed effect results, these results do not provide any evidence of a negative supply response.

6.2 Heterogeneity in impact

6.2.1 Impact by type of worker and age

We repeat the regressions for fixed effects and DID for subgroups of prime age wage and salary workers and youth (aged 20-29). The fixed effect estimates are insignificant for both groups, and the DID estimates for wage and salary workers are not much different from those for all workers reported earlier – positive and insignificant. But, in the case of youth we notice a significant negative effect in the DID regressions (Table 10), which is interesting but somewhat predictable since youth are less attached to their jobs, and may want to enroll in school when they receive cash. On average the youth who received cash only in the winter quarter of 2012 worked 9 fewer hours compared to those who received cash in the winter quarters of 2011 and 2012. The estimated impact for youth who reported to work and study (not reported) is even larger – 23.5 fewer hours – though the number of observations is very small and the impact is not significant.

6.2.2 Impact by sector of employment

We also look for program impact among subgroup of workers employed in agriculture, industry, and services. As noted earlier, agricultural workers are suspected of leaving their jobs upon receiving cash transfers more than workers in other sectors, perhaps because their jobs are physically demanding and seasonal. As the results in table 11 indicate, there is no evidence of a negative impact for workers in agriculture, nor for that matter for workers in industry. Where we have a significant estimate, for service sector workers and using fixed effects, the impact is positive – an increase of 36 minutes weekly hours as a result of a 10% increase in the intensity of treatment. The DID estimate for the service sector is also positive but it is not significant.

6.3 Labor force participation

The estimation of program impact for labor force participation is complicated by the fact that it is binary, which does not lend itself to first differencing (Greene 2004). We therefore limit our estimation of program impact on participation to DID using nonlinear probit estimation suggested by Eissa and Liebman (1996). The DID equation with probit is as follows:

$$\Pr[Y_{it}] = \Phi(\alpha_0 + \alpha CT_i + \beta Year_t + \delta CT_i \times Year_t + \mathbf{X}_{it}\beta) \qquad (4)$$

Before looking at the estimation results for equation 4, it is useful to examine the simple transition matrix for participation in Table 12, which forms the basis of the DID. For both men and women, roughly equal numbers entered and exited the labor force, so the same percentage of men (88%) and women (18%) were in the labor force in each of the years. About 85% of men and 13% of women were in the labor force in both periods. Most men and women did not change their labor force status, but women were much more mobile than men: about 4% of men and 25% of women who were participating left the labor force in 2011, and 3% of men in the labor force in 2011 were new entrants compared to 24% for women.

We report two sets of DID results for participation. The first compares early and late participants, as before (Table 13), and the second compares individuals living in households with low and high intensity of treatment (see Table 14). The latter DID results mimic our fixed effects estimates, but instead of comparison across a range of intensities of treatment, it compares two groups at the extremes of treatment intensity. High intensity are those with intensity of 25% or higher and low intensity are those with 10% and lower. Neither results in these tables indicate a reduction in labor supply by men or women in the extensive margin.

6.4 The role of expectations

The government promise of a steady monthly transfer of cash, if credible, affects the permanent income of households, which in the presence of a well functioning credit market can make the timing of the start of the transfer irrelevant as long as missed payments are made up later, which was the case in Iran. But, as noted earlier, the market for credit market does not exist in Iran, especially for the poor who may decide not to pay back their debts even if they continue to receive cash transfers later. It is quite a stretch to think that lenders to the poor can rely on the Iranian judiciary to recover small claims against poor individuals. Nevertheless, it is important to know if shocks to permanent income can affect behavior before any cash is transferred.

Three provinces were selected as test runs for the cash transfer program in summer of 2010, 6-9 months before the program started in other provinces.[10] We define treatment here as having enrolled in the program and received a bank account into which the transfers were deposited. Withdrawal from these accounts became possible at the same time as the regular transfer accounts, right after the program implemented. About 850,000 individuals registered in these provinces (without any cash being transferred). Arguably, households in these provinces had formed their expectation of the change in their permanent incomes several months before households elsewhere. If there were no credit constraint as they and the government promise of future payments were credible, expectations of future cash transfers would be as good as cash itself, and we would expect a shift in the labor supply behavior of households in the test provinces compared to the rest of the country (if cash transfers would have labor supply effects).

To test this hypothesis, we define a treatment variable such that our control (or comparison) group are households in the three test provinces, who presumably experienced the positive shock to their expected permanent income in both summers of 2010 and 2011. We define the treatment (or program) group as those living in the rest of the country who did not experience this increase in the summer of 2010 but did so in summer of 2011. The DID equation is therefore the same as equation 3. We present the DID results for hours worked by wage and salary workers in Table 15 separately by age group and rural-urban residence and for the bottom 40%. The program effect is negative throughout, but is not significant in any set. The negative estimates of impact suggest that households in the non-test provinces experienced a larger decline (or slower increase) in their labor supply, indicating that earlier participation in the program, without any actual cash transfer, may have reduced the labor supply of some workers. The workers in the bottom 40% had larger negative impacts, -5.54 compared to -1.95 hours per week. Despite the consistent negative estimates of impact, because they are not significant, we do not consider them as evidence that expectation of increase in permanent income resulted in any significant reduction in labor supply. As a result, we believe that these result do not reject the assumption of a binding credit constraint that we have made throughout this paper.

7. Conclusions

Universal cash transfers started in Iran as replacement for energy subsidies, on over 4 million equivalent barrels per day of oil and gas products. The logic of replacing a policy that delivers the country's natural wealth as in-kind transfers to households and firms based on their energy consumption, while distorting production costs and polluting the environment, with one that gives the citizens the cash and offers them a choice in what they want to spend it on, is very compelling. Yet, since they started in 2011, the cash transfer program has come under sharp criticism, in part for its alleged negative effect on the labor supply of the poor. While standard economic theory predicts that an increase in unearned income should increase leisure and

[10] The choice of the three provinces – Ardebil, Gorgan, Mazandaran – is not clear, except that Ahmadinejad had earlier served as the provincial governor in Ardebil.

reduce labor supply, imperfections in the markets for labor and credit suggest that a different response is possible. Hence the need for an empirical approach.

In this paper we use panel data constructed from Iran's rotating expenditure and incomes surveys to examine the causal impact of the program on the labor supply of the poor. We employ fixed effects and difference-in-differences estimation to identify the causal impact of the transfers on labor force participation and individual hours of work. We find little evidence to support the claim of a negative labor supply effect in the fixed effects results, where the estimates of impact are either positive or not significant. The DID results allow estimation of impact for subgroups of workers. While there is no evidence of a negative supply response for the average worker, male or female, there is one for youth in their twenties. If one were to expect a strong negative impact it would be for youth, who have weak job attachment, can stay in school longer, or enjoy more leisure, though we do know which of these options they choose. Counterintuitively, we find a positive impact among service sector workers. This result can be explained by the fact that the service sector is populated by credit-constrained small firms that cash transfers can help expand. Our overall conclusion is that the program did not affect labor supply in any appreciable way.

We point to several caveats in our empirical tests that may have hide a negative causal impact from view. Our fixed effects estimates assume workers with different level of incomes experienced the same level of shock to their labor demand during 2010-2011. We look for confirmation of this assumption by looking backwards, to previous years, when labor market outcomes for different groups appear to move together. However, the shock due to international sanctions that took effect in 2011-2012 may have affected different workers differently than previous shocks, so we should interpret these results cautiously. Another caveat is related to our difference-in-differences result which assume credit constraint. If there is no credit constraint, the promise of payment in the future is as good as payment now (minus interest income). In this case the timing of the start of cash transfers may matter less than their expected lifetime amount. Since the latter is the same for those who joined the program later but were paid retroactively, we lose our source of exogenous variation in the value of transfers. This is not a large concern in our view since credit constraint, especially consumption loans for poor workers, is a rare thing in Iran and we do not expect workers to quit their jobs and start borrowing to finance their consumption while waiting for cash transfers three months later. Nevertheless, we test the importance of the credit constraint assumption by comparing the behavior of households in three provinces that were included in an early treatment 6-9 months prior to the program with those in the rest of the country. We found no difference in the labor supply behavior of the two groups.

Our own understanding of the lives of the poor in Iran is that getting $1.50 per day, with dubious real value in future years, is little reason for poor workers to quit their jobs, though some in more physically demanding jobs might. But does the reduction in labor supply, if it occurs, represent a real loss of value to the economy? We doubt that many would consider an agricultural worker forced to work with hazardous pesticides without proper equipment quitting his or her job after receiving a cash transfer a bad outcome.

As more oil exporting countries decide to remove energy subsidies, or for political economy reasons decide to transfer a part of their oil wealth unconditionally to their citizens, the question of how such transfers affect the incentive of their citizens in working and acquiring skills become more important. The findings in this paper do not settle this question. What we have accomplished is at the very least to shift the burden of proof on this issue to those who claim cash transfer make poor people lazy, and to show the need for better data and more research.

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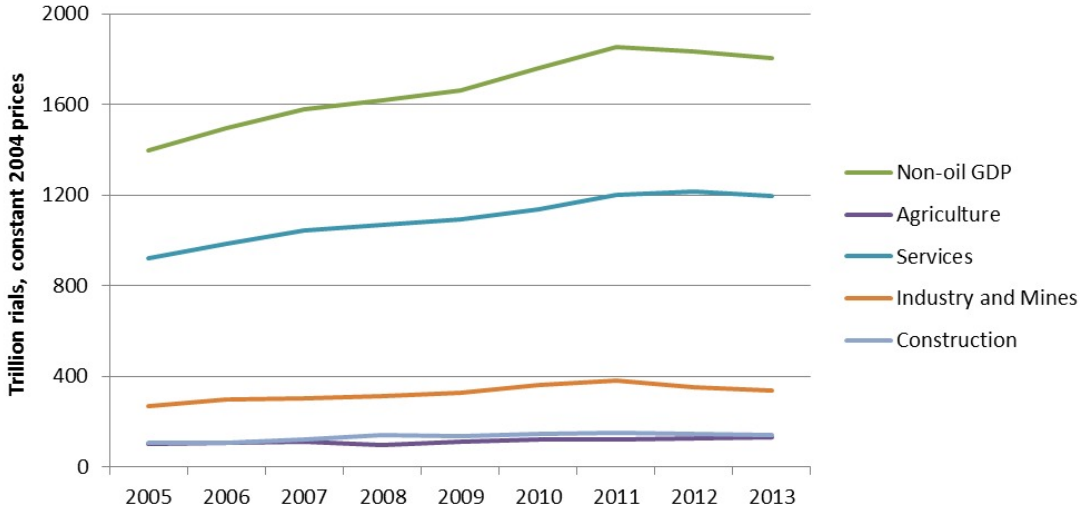
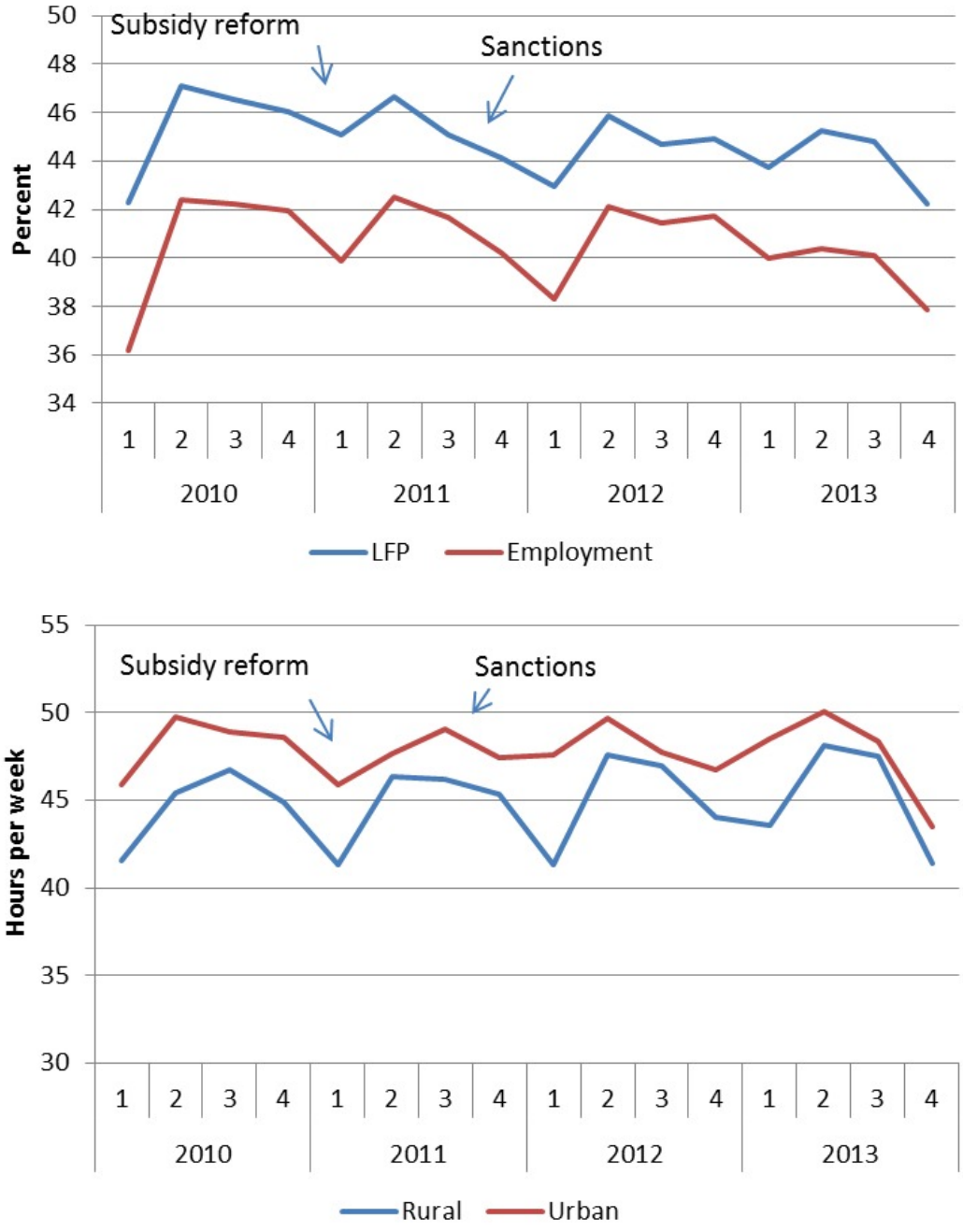
Figure 1: The Timing of Various Shocks to GDP, Quarterly Data by Sector of Production



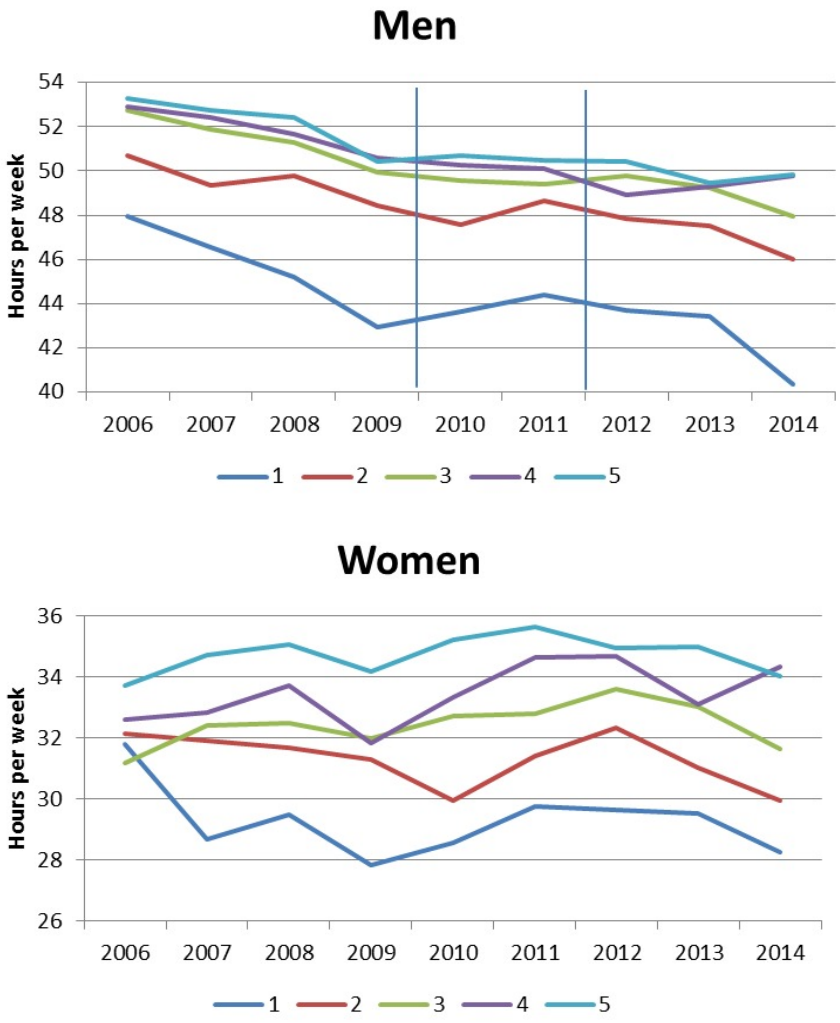
Note: GDP is in constant 2004 rials $x10^{12}$.
Source: Central Bank of Iran, Economic Trends, various years.

Figure 2: Labor Force Participation, Employment Rates, and Average Weekly Hours Worked of Less Educated Private Sector Workers



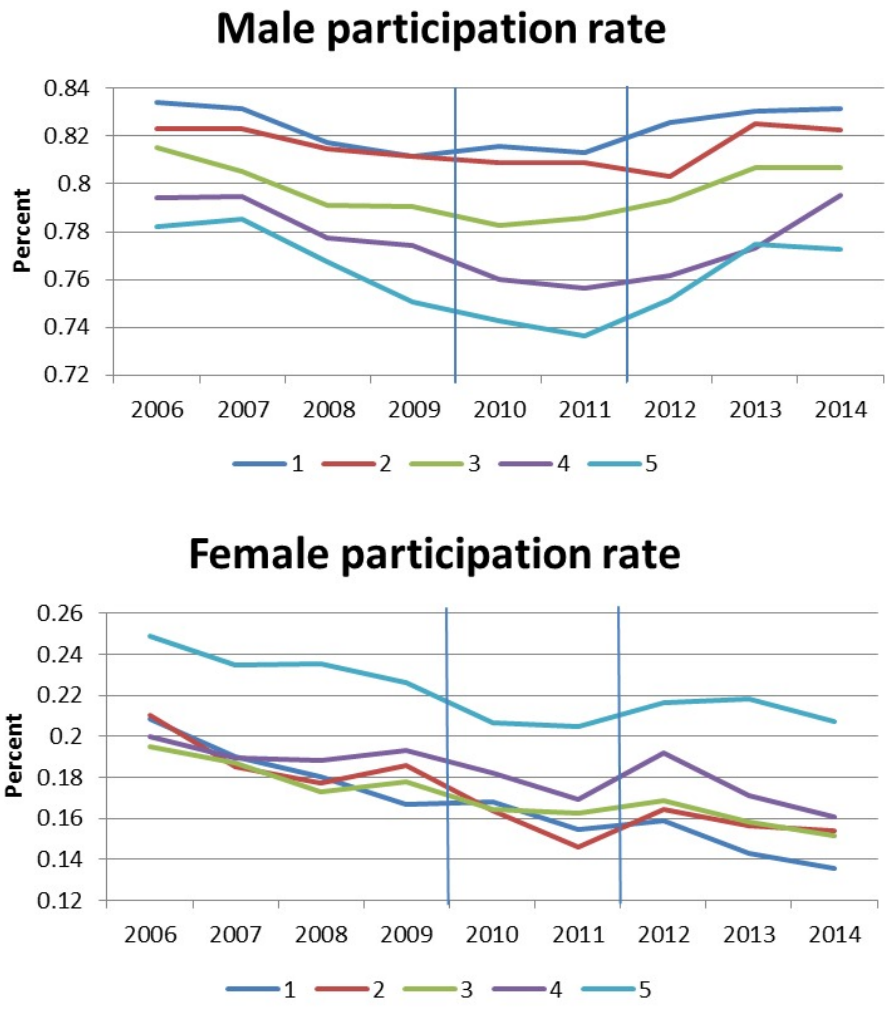
Note: Workers with less than high school, private sector wage and salary workers, aged 15-64. .
Source: Statistical Center of Iran, quarterly reports of Labor Force Surveys.

Figure 3: Parallel Trend in Hours Worked



Note: Workers aged 15-64.
Source: Authors' calculations from HEIS data.

Figure 4: Parallel trends in participation



Note: Workers aged 15-64.
Source: Authors' calculations from HEIS data.

Table 1: Hours of Work Per Week by Expenditure Quintile

	Expenditure quintile in 2010				
	1	2	3	4	5
Year					
2010	43.14	47.06	47.18	48.92	47.84
	(18.69)	(17.72)	(17.69)	(17.97)	(17.61)
2011	43.31	46.82	47.62	48.72	47.90
	(17.91)	(17.80)	(17.37)	(17.09)	(16.99)
Change	0.17	-0.24	0.44	-0.19	0.06
	(0.55)	(0.55)	(0.58)	(0.62)	(0.66)

Note: Quintiles of expenditure per capita in 2010.
Source: Statistical Center of Iran, Household Expenditure and Income Survey 2010-2011. The evidence in this section does not offer a strong prior regarding the direction of the impact of the cash transfer program on labor supply. We look to the causal empirics discussed below for more insight.

Table 2: Hours of Work by Groups of Workers

	Wage worker			
	Public	Full-time	Part-time	self employed
2010	45.3	51.1	14.7	43.9
	(16.0)	(12.7)	(11.5)	(22.0)
2011	45.1	51.3	15.5	44.8
	(15.2)	(12.1)	(11.5)	(21.5)
Change	-0.1	0.1	0.8	0.9
	(0.3)	(0.1)	(0.2)	(0.2)

Source: Statistical Center of Iran, Household Expenditure and Income Survey 2010-11 and Authors' calculation.

Table 3: Comparison of the 2010 Base Sample and the Balanced Panel

	Balanced panel	Base sample
Urban (%)	67.93	71.43
	(46.67)	(45.17)
Expenditures†	33.46	31.53
	(34.61)	(33.27)
Net income†	26.33	28.96
	(26.67)	(30.62)
Cash transfers†	0.32	0.28
	(0.66)	(0.58)
Household size	4.28	4.53
	(1.59)	(1.86)
Employment rate (%)	27.89	28.51
	(44.84)	(45.15)
Participation rate (%)	32.03	33.81
	(46.66)	(47.31)
Hours of work per week	47.34	47.47
	(19.53)	(18.44)
% female	50.39	49.40
	(49.99)	(50.00)
Age	30.96	31.21
	(20.29)	(19.69)
% literate	83.86	83.60
	(36.79)	(37.03)
Years of education	6.51	6.81
	(5.29)	(5.38)
Marital status:		
Married (%)	58.13	56.75
Widow (%)	4.72	4.59
Divorced (%)	0.69	0.92
Never-married (%)	36.45	37.75
Observations	46,517	152.291

Notes: Summary statistics: individual level, full sample and balanced panel. Standard errors in parentheses. † Income, expenditures and cash transfers are per person in million rials per year.
Source: HEIS 2010-11

Table 4: Attrition is Not Entirely Random: More Urban Households and Renters Attrited

	Rural(%)	Urban(%)	Total(%)
Attrited			
Yes	27.8	40.0	34.2
No	72.2	60.0	65.8
Attrition by home ownership			
Rent	55.1	63.7	62.0
Own	25.4	31.6	27.9
Attrition by pce quintiles			
1	28.5	34.5	30.8
2	26.7	39.8	32.5
3	26.8	40.6	34.5
4	27.5	40.0	35.5
5	29.7	44.1	40.0

Source: HEIS 2010-2011 and authors' calculations.

Table 5: Transition Matrix for Employment Status

	Employment status in 2011						
	Employed	Unemployed	Retired	In school	Homemaker	Other	Total
Status in 2010							
Employed	88.49	4.45	1.93	0.96	3.73	0.43	100
Unemployed	27.28	57.83	1.32	6.73	5.28	1.57	100
Retired	10.82	1.43	80.34	0.27	5.65	1.50	100
In school	4.41	8.22	0.20	78.05	5.17	3.95	100
Homemaker	3.20	0.86	0.80	0.65	94.06	0.42	100
Other	11.38	12.80	4.88	14.84	8.33	47.76	100
Total	39.24	7.19	5.95	12.08	33.56	1.98	100

Notes: Individuals aged 20-59
Source: HEIS 2010-2011 and authors' calculations.

Table 6: Intensity of Treatment by Expenditures Quintiles

Quintiles of per capita expenditures	Ratio of net transfers to expenditures (%)
1	49.3
2	24.7
3	15.0
4	10.5
5	4.9
Total	19.5

Notes: Intensity of treatment is the ratio of transfers (net of the increase in energy expenditures) to household expenditures before the program started.
Source: HEIS 2010-11 and authors' calculations.

Table 7: Summary statistics for comparison and program groups

	Program	Comparison
% urban	50.65	46.86
	(50.01)	(49.91)
Household size	4.41	4.36
	(1.71)	(1.55)
Labor force participation rate (%)	49.33	51.72
	(50.01)	(49.98)
Employment rate (%)	41.77	45.87
	(49.34)	(49.84)
Per capita expenditures (million rials)	28.42	28.13
	(25.34)	(23.14)
% literate	81.29	83.68
	(39.02)	(36.96)
Age	35.55	36.45
	(10.92)	(10.47)
% Female	52.54	50.96
	(49.95)	(49.98)
Years of education	7.46	7.33
	(5.18)	(4.99)
Marital status:		
Married (%)	69.26	75.68
Widow (%)	2.18	1.84
Divorced (%)	1.59	0.71
Never-Married (%)	26.98	21.77
Observations	1,336	3,811

January-March 2011. Sd in parentheses.

Notes: Individuals aged 20-59.

Table 8: Estimates of program impact on weekly hours worked: fixed effects

	Men		Women	
	(1)	(2)	(3)	(4)
Intensity of treatment	0.043*	0.049*	-0.008	0.001
	(0.020)	(0.022)	(0.010)	(0.010)
Change in unearned income	-0.071**	-0.061**	0.008	0.008
	(0.022)	(0.020)	(0.013)	(0.013)
Age		0.460		-0.351
		(0.639)		(0.286)
Age squared		-0.005		0.004
		(0.007)		(0.003)
Log unearned income		-0.207**		-0.007
		(0.069)		(0.032)
Education level:				
Less than primary		-0.013		0.602
		(1.282)		(0.623)
Primary completed		0.050		1.274
		(1.432)		(1.010)
Lower secondary		0.563		-1.052
		(1.622)		(1.042)
Upper secondary		0.833		1.092
		(1.418)		(1.422)
Tertiary		0.502		-3.329
		(2.414)		(5.912)
Controlled for:				
Urban		Yes		Yes
Marital status:		Yes		Yes
Observations	4435	4435	4763	4763

Notes: Intensity of treatment is the ratio of cash transfers to last year's per capita expenditures. Columns 2 and 4 include controls of first period characteristics. Standard errors are in parentheses. * $(p < 0.05)$, ** $(p < 0.01)$.

Table 9: Estimates of Program Impact on Weekly Hours Worked: DID

	Men (1)	Women (2)
Year×Treatment	1.30	2.54
	(2.18)	(1.63)
Year	-4.26**	-0.39
	(1.30)	(1.01)
Treatment	-2.92	-1.39
	(1.84)	(1.12)
Age	1.72*	0.96*
	(0.78)	(0.48)
Age squared	-0.03**	-0.01
	(0.01)	(0.01)
Log unearned income	-1.03**	-0.07*
	(0.08)	(0.03)
Wage and salary worker	8.12**	34.33 **
	(1.15)	(3.09)
Education level:		
Less than primary	3.41	-0.17
	(1.79)	(0.69)
Primary completed	2.67	-0.84
	(2.11)	(0.76)
Lower secondary	2.50	0.56
	(1.63)	(0.87)
Upper secondary	2.02	8.98**
	(2.04)	(1.19)
Tertiary	3.50	18.48**
	(2.95)	(3.85)
Controlled for:		
Urban	Yes	Yes
Province	Yes	Yes
Marital status	Yes	Yes
Observations	3424	3656

Notes: The comparison group received transfers in both periods (winter quarters 2011 and 2012) and program group in the second period only. Standard errors in parentheses ** $(p < 0.05)$, *** $(p < 0.0)$.

Table 10: Effect of Cash Transfers on Hours of Work Per Week for Wage and Salary Workers and Youth

	Prime-age		Youth	
	(1) Fixed effects	(2) DID	(3) Fixed effects	(4) DID
Intensity of Treatment	0.03		0.08	
	(0.02)		(0.05)	
Year × Treatment		2.31		-8.97*
		(1.77)		(4.18)
Change in unearned income	-0.05		0.02	
	(0.03)		(0.03)	
Age	0.18	-0.50	-1.08	12.02
	(0.85)	(0.65)	(8.82)	(6.73)
Age squared	-0.00	0.01	0.00	-0.23
	(0.01)	(0.01)	(0.17)	(0.13)
Log unearned income	-0.15	-0.26**	0.32*	-0.00
	(0.09)	(0.06)	(0.14)	(0.14)
Year		-1.77		2.51
		(0.97)		(2.25)
Treatment		-2.42		5.23
		(1.49)		(3.53)
Observations	3215	2204	1251	945

Notes: Youth are ages 20-29. Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$.
Source: HEIS panel, 2010-2011.

Table 11: Effect of Cash Transfers on Hours of Worked By Sector of Employment, DID and Fixed Effects

	Agriculture		Industry		Services	
	(1) Fixed effects	(2) DID	(3) Fixed effects	(4) DID	(5) Fixed effects	(6) DID
Intensity of treatment	0.05		0.02		0.06*	
	(0.04)		(0.06)		(0.03)	
Year × Treatment		-6.84		0.62		6.02
		(6.82)		(6.55)		(3.26)
Change in unearned income	-0.02		-0.15		-0.09	
	(0.03)		(0.09)		(0.05)	
Age	1.10	-2.42	1.92	-8.12	-0.64	0.89
	(1.39)	(2.22)	(1.80)	(8.59)	(0.78)	(1.61)
Age squared	-0.01	0.02	-0.02	0.10	0.01	-0.01
	(0.02)	(0.03)	(0.02)	(0.10)	(0.01)	(0.02)
Log unearned income	-0.13	-0.38	0.25	0.09	-0.12	-0.50**
	(0.16)	(0.23)	(0.20)	(0.27)	(0.08)	(0.18)
Year		0.67		-0.61		-3.76
		(3.22)		(3.93)		(2.47)
Treatment		-5.92		-3.91		-3.96
		(5.33)		(6.46)		(2.82)
Observations	955	417	524	150	2672	795

Notes: Regressions restricted to male workers only. Includes controls for education level, marital status, province, and urban. Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$.
Source: HEIS panel, 2010-2011.

Table 12: Transition Matrix of Labor Force Participation Status of Men and Women, 2010-2011

	Labor force status in 2011			
	Out	In	Out	In
	Women		Men	
Labor force status in 2010				
Out	5,899	323	678	203
In	354	1,012	222	6,018

Notes: Men and women 20-59 years old, 21 March to 20 December, 2010 and 2011.
Source: Authors' calculations using data from the (2010-2011) panel.

Table 13: Impact on Probability Of Participation: DID Results for Early Vs. Late Participants

	Men (1)	Women (2)
Year×Treatment	-0.011	0.074
	(0.016)	(0.048)
Year	-0.004	-0.012
	(0.009)	(0.022)
Treatment	-0.012	-0.005
	(0.015)	(0.028)
Age	0.007	0.036**
	(0.007)	(0.011)
Age squared	-0.000*	-0.000**
	(0.000)	(0.000)
Log unearned income	-0.008**	-0.000
	(0.001)	(0.001)
Child 0-1 year old		-0.031
		(0.040)
Controlled for:		
Urban	Yes	Yes
Province	Yes	Yes
Marital status	Yes	Yes
Education level	Yes	Yes
Observations	3370	3474

Notes: Men and women 20-59 years old, 21 March to 20 December, 2010 and 2011.
Source: Authors' calculations using data from the (2010-2011) panel.

Table 14: Impact on Probability of Participation: DID Results for Rich vs. Poor

	Men (1)	Women (2)
Year× Treatment	0.007	-0.017
	(0.010)	(0.026)
Year	-0.007	-0.019
	(0.009)	(0.024)
Treatment	-0.003	-0.024
	(0.009)	(0.018)
Age	0.004	0.026**
	(0.004)	(0.008)
Age squared	-0.000*	-0.000**
	(0.000)	(0.000)
Log unearned income	-0.007**	-0.001
	(0.001)	(0.001)
Controlled for:		
Urban	Yes	Yes
Province	Yes	Yes
Marital status	Yes	Yes
Education level	Yes	Yes
Observations	5762	6206

Notes: Men and women 20-59 years old, 21 March to 20 December, 2010 and 2011.
Source: Authors' calculations using data from the (2010-2011) panel.

Table 15: Testing the Effect of Possible Increase in Permanent Income: DID Regression of Change in Hours Worked

		Bottom 40%				
	(1) All	(2) Total	(3) Urban	(4) Rural	(5) Youth	(6) Prime age
Treatment × year	-1.95	-5.54	-2.70	-5.61	-1.21	-6.14
	(2.71)	(3.87)	(9.88)	(4.00)	(8.34)	(4.47)
Treatment	2.97	5.24	0.13	6.07*	1.54	5.86
	(1.93)	(2.77)	(7.83)	(2.75)	(6.26)	(3.19)
Year	1.89	4.15	1.42	4.49	1.85	3.16
	(2.62)	(3.74)	(9.74)	(3.81)	(8.16)	(4.29)
Log ueinc†	-0.27**	-0.44**	-0.55**	-0.34**	-0.77*	-0.36**
	(0.06)	(0.10)	(0.18)	(0.12)	(0.32)	(0.11)
Age	0.02	0.07	0.17*	-0.03	0.20	-0.13
	(0.03)	(0.05)	(0.08)	(0.06)	(0.23)	(0.09)
Years of education	0.27**	0.58**	0.49*	0.65**	0.65**	0.50**
	(0.07)	(0.13)	(0.22)	(0.17)	(0.22)	(0.17)
Urban	5.09**	3.45**			1.26	4.60**
	(0.76)	(0.99)			(1.72)	(1.25)
N	3472	1824	576	1248	584	1166

Notes: The control group consists of the households from three provinces that participated in the cash transfer program earlier; all others are assigned to the treatment group. Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, † Unearned income. Individual hour worked, wage and salary workers.