The impact of unconditional cash transfers on labor supply: evidence from Iran's energy subsidy reform program *

Djavad Salehi-Isfahani
Virginia Tech and ERF

Mohammad Hadi Mostafavi Dehzooei
Virginia Tech

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Abstract

We study the impact of an extensive unconditional cash transfer program in Iran on incentives to work. Starting in 2011, Iranian families started receiving about $90 (PPP) per person per month as compensation for increase in energy prices. There has been considerable criticism of the program for having reduced the incentives of the poor to work. We exploit the variation in the intensity of treatment arising from energy consumption, income, and family size and panel data to isolate the impact of the cash transfers from other economic shocks that reduced employment in the years subsequent to the start of the cash transfers. We find no evidence of a negative employment effect from cash transfers.

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1 Introduction

Cash-transfers are widely used around the world to reduce poverty and to improve health and education of the poor. There is considerable evidence that when these transfers are conditional they have the desired positive effects (references). There is less known about the impact of unconditional cash transfers (UCC). UCCs have the great advantage of being much easier to implement and doing away with the cost of monitoring of the behavior of the recipients. Their main drawback is that recipients may not use the funds in ways that planners intend or prefer, for example spend them on adult goods instead of child goods or use them to increase their leisure instead of productive activities. As unearned income, UCC can be a disincentive to work for the poor.

In this paper we examine the employment effects of a large scale cash transfer program started in 2011 in which all households were given a monthly cash transfer of about $45 per person (about $90 in Purchasing Power Parity dollars). The cash transfers were in compensation for an ambitious subsidy reform program for energy and bread in which prices were raised by factors of 2 to 9 (for a description of the program see (Guillaume, Zytek, and Farzin 2011; Salehi-Isfahani, Stucki, and Deutschmann 2013; Salehi-Isfahani 2014).

A persistent criticism of the cash transfer program in the Iranian media has been its disincentive effect on work.[1] Despite the popularity of such claims there has been no rigorous evaluation of Iran's UCT. The identification of the UCT impact is made difficult by a number of negative shocks that occurred right after the UCT program went into effect and very likely affected employment. International sanctions against Iran intensified in late 2011, reducing Iran's oil income by half and disrupted Iran's import of intermediate and capital goods causing factories to shut down or work with less than half their normal capacity (references). To identify the impact of UCT on employment, it is imperative to isolate the effect of lower labor demand from reduced incentives to work.[2]

In this paper we take advantage of a panel of household and individuals that were observed in 2010 and 2011 to estimate the impact of cash transfers. We compare the number of working members of households and the hours of work of individuals before and after the cash transfer. We first present the results of a simple regression

[1] One report claimed the loss of more than half a million agricultural jobs as a result of the program http://www.al-monitor.com/pulse/tr/originals/2013/04/iran-presidential-elections-subsidy-reforms.html.

[2] It should be emphasized that the increase in energy prices, which was part of the same package may have easily reduced demand for labor. However, our interest here is with the impact of the cash transfer part of the program.

approach that controls for the demand side effects as well as for the intensity of treatment as measured by income (poorer families received a larger proportion of their incomes in cash transfer), family size (proportional to economies of scale), and negative income shock due to higher energy prices (measured by energy use).

We also present the results of a quasi experimental approach in which we take advantage of the fact that roughly one-third of individuals in our sample did not receive a transfer for reasons during the first three months of the program. We claim that the assignment was independent of their labor supply behavior. The difficulty with the quasi experimental approach is that those who did not receive the transfer in the last quarter of 1389 were assured by the government that they would receive it later when they registered. So, the impact on the labor supply behavior of this group depends on the extent to which they were credit constrained and could not borrow to finance consumption if they needed. We use their reported interest income to control for credit constraint. Our findings from both approaches generality do not support a negative employment effect from the cash transfers.

There is a substantial literature on the impact of cash transfers on household behavior. Danziger, Haveman, and Plotnick (1981) review the theoretical models that predict a reduction in labor supply in the presence of cash transfers. In a more recent study Haushofer and Shapiro (2013) examine the impact of an unconditional cash transfer program in the pour villages of Western Kenya between 2011 and 2012. They found that recipients of cash transfer consumed more food, healthcare, and education compared to the control group. They also found that recipients increased asset holdings in the form of home improvements and increased live stock holdings. They did not explicitly study labor supply but the recipients investment behavior suggested that the unconditional nature of the transfer did not cause it to dissipate into unproductive activities.

Our paper is organized into 5 sections. Section 2 discusses the panel data and the extent of attrition problem that is endemic to rotating panels. Section 3 summarizes the observed change in labor supply between 2010 and 2011 focusing on the number of working members of the household and their hours of work by various characteristics. This section also presents the transition probabilities for employment status. Section 4 presents the results of estimation, and Section 5 concludes.

2 Data

The data we use in this study are derived from the Household Expenditures and Income Survey (HEIS) which is collected annually by the Statistical Center of Iran

(SCI) since 1964. The survey is nationally representative and two-stage stratified, at the urban and rural level and by province. The survey is weighted and sampling weights are provided, which we use in combination with the attrition probabilities that we compute.

The survey includes information on expenditures and incomes of urban and rural Iran households. Starting in 2010, HEIS is collected as a rotating-panel. 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 to follow families and individuals, extracting a panel from HEIS is a challenging task. First, households are identified by their physical address, so when a family interviewed in year 1 moves, the new residents of that physical address replaces it and receives the same household ID. Second, to construct the panel of individuals, we have the additional problem that when a member of the household leaves his or her ID number is allocated to the next person, so individual ID's cannot be used to idnetify individuals across years. We used age and sex to do so.

Of the 38,950 households in the 2010 survey, 26,180 (67%) were designated as panel households and were supposed to be re-interviewed in 2011. We call this the unbalanced panel. Of these 17,371 households (66%) were found and reinterviewed, which comprise our balanced panel.[3] The attrition rate is therefore 34%.

There is one more step before arriving at our working sample. In order to analyse changes in family labor supply, we only keep those households that remained intact, that is their membership did not change between 2010 and 2011. This step removes another 7,032 observations from the balanced panel, leaving us with 10,339 intact households.

When analysing with house of work, we have still a smaller sample, because we exclude observations in which on one was a wage or salary worker. this is because the hours of work of self employed workers are notoriously imprecise. Furthermore, cash transfers started in the last quarter of the Iranians year 1389 (corresponding to the first quarter of 2011). For some estimations we only work with the panel of individuals for these months. HEIS sample is evenly spread over 12 months, so subsamples for each month across different years are comparable.

2.1 Attrition

Attrition is important in panel-data analysis if the observations that drop out of the sample differ systematically from those that remain in it. If attrition is not

[3] In addition to those identified by the survey as having attrited, we excluded another xx households because the age of the head has changed by more than one or two years.

Table 1: Summary statistics: the household sample

	mean	standard deviation	min	max
Urban	.36	.48	0	1
Household size	2.57	1.61	1	10
Literacy of the head	.35	.47	0	1
Age of the head	62.19	16.94	20	99
Head is female	.43	.49	0	1
Marital status of the head	1.50	.65	1	4
Number of students	.42	.79	0	4
Employment of the head	2.40	1.10	1	6
pce	2.99e+07	3.54e+07	3140128	4.21e+08
Home ownership	1.74	1.53	1	7

Table 2: Summary statistics: DID

	treatment				comparison			
	mean	sd	min	max	mean	sd	min	max
Urban	.37	.48	0	1	.43	.49	0	1
Household size	2.57	1.62	1	10	3.05	1.65	1	13
Literacy of the head	.35	.48	0	1	.55	.49	0	1
Age of the head	62.19	16.94	20	99	56.14	16.97	23	99
Head is female	.43	.49	0	1	.24	.43	0	1
Marital status of the head	1.50	.65	1	4	1.30	.57	1	4
Number of students	.42	.79	0	4	.57	.89	0	4
Employment of the head	2.40	1.10	1	6	1.95	1.04	1	6
pce	2.99e+07	3.54e+07	3140128	4.21e+08	3.19e+07	2.90e+07	2421388	3.66e+08
Home ownership	1.72	1.61	1	7	1.49	1.34	1	7

random the sample will not be representative of the population and inference could be biased. Rotating panels, such as HEIS, present challenging problems of attrition because the surveys are not primarily designed as panels.

As noted above, about 34% of the panel households do not reappear in 2011. This a rather high rate of attrition because another survey conducted by SCI that was designed as panel had the same attiriton rate over 4 years (Salehi-Isfahani and Majbouri 2010).

Attrition is selective in our sample appears selective. Attrition is higher in urban areas, which is expected because of greater geographic mobility of the urban population. Renters had a much higher rate of attrition than home owners, also expected. We use rent status as a source of identification in estimating the probability of attrition.

Income is a determinant of attrition for urban but not rural households. The rich have a higher rate of attrition than the poor. Households who received a cash transfer in 2010 had a lower rate of attrition. This makes sense because families on the move or expecting to move may have waited before registering for the cash transfer.

Attrition appears to depend also on the working status of the head of the household, which raises more difficult questions about bias since labor supply is the outcome of interest. As number of employed members goes up probability of attrition falls. Not surprisingly, individuals who have a job are less likely to move and therefore attrit. This implies that our estimates of labor supply in 2011 are biased downward, that is we are likely underestimating the degree to which labor supply declined in 2011.

A formal test of whether attrition is random or not, is offered by Becketti, Gould, Lillard, and Welch (1988) . Their test involves regressing the dependent variable of interest on household characteristics, an attrition dummy, and interaction of the attrition dummy with other explanatory variables. An F-test of joint significance of attrition dummy and interaction terms is used to determine whether attrition is random. According to this test (F-statistic of 1.72, and p-value of 0.0046), the null hypothesis that attrition is random is rejected.

We therefore need to re-weight our observations according to the inverse probability of attrition. Following Fitzgerald, Gottschalk, and Moffitt (1998) to calculate these probabilities via a probit of attrition on a host of household characteristics.

Table 3: Attrition

	Rural(%)	Urban(%)	Total(%)
Attrited	27.8	40.0	33.6
Present in both years	72.2	60.0	66.4

Table 4: Attrition by home ownership

	2010-2011 panel	
	Rent(%)	Own(%)
Rural	55.1	25.4
Urban	63.7	31.6
Total	62.0	27.9

Table 5: Attrition by quintile of per capita expenditures

	pce quintiles				
	1	2	3	4	5
Rural(%)	28.5	26.7	26.8	27.5	29.7
Urban(%)	34.5	39.8	40.6	40	44.1
Total(%)	30.8	32.5	34.5	35.5	40

Quintile 1 is the poorest.

Table 6: Transition matrix for employment status

	Employment status in 2011						
Employment status in 2010	Employed	Unemployed	Retired	In school	Homemaker	Other	Total
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

3 Changing patterns of employment before and after cash transfers

The purpose of this section is to describe the labor market conditions before and after the UCT. The tabular presentation is helpful in seeing how employment conditions changes for households and individuals before asking about causation

We begin with an examination of the transition matrix of activity for the panel in Table 6. This table shows the proportion of individuals in each status (employed, unemployed, and inactive) in 2010 that stay or change their status in 2011. Table 6 shows that 88.5% of those employed remained employed in 2010 remained employed in 2011, 4.5% lost their jobs and became unemployed. About 2% retired, 1% enrolled in school, and 4% returned to housework. About 26.3% of those unemployed in 2010 found work in 2011. This is roughly the same (440) number as those who lost their jobs in 2011 (434). Interestingly, 3.2% of those engaged in housework (260) in 2010 found jobs in 2011, many fewer than those who left their jobs for housework (369). Overall, this table then exhibits a fair amount of stability in activity status.

We next examine labor supply at the level of households and individuals. At the household level we measure labor supply by the number of working members in each year. Table 7 groups households by the decile of per capita expenditures and shows that all groups lost working members, though the differences are not significant.

At the level of the individual we notice that hours of work decreased only for the poorest and richest deciles and increased for the rest, though, again, none are significant. There is no evidence here then for either an increase or decrease in

Table 7: Number of household members working

	Per capita expenditure deciles									
	1	2	3	4	5	6	7	8	9	10
Year										
2010	1.14	1.22	1.21	1.19	1.20	1.16	1.14	1.12	1.05	0.98
2011	1.06	1.16	1.15	1.17	1.14	1.13	1.10	1.09	1.03	0.94
Change	-0.08	-0.06	-0.06	-0.02	-0.06	-0.03	-0.03	-0.03	-0.03	-0.03

Note: Decile 1 is the poorest.

Table 8: Hours of work per week of individuals by decile of per capita expenditures

		Per capita expenditure deciles in 2010									
		1	2	3	4	5	6	7	8	9	10
Year											
2010		34.11	38.48	40.20	41.03	42.13	42.59	45.71	44.30	42.89	44.67
2011	All year	35.44	39.52	40.78	42.14	44.90	44.17	45.83	45.49	43.06	45.64
Change		1.33	1.04	0.57	1.10	2.77	1.58	0.12	1.18	0.17	0.97
2010		33.96	34.01	38.22	38.74	44.44	40.71	44.98	43.75	43.89	46.26
2011	Last season	30.50	38.19	39.12	40.64	46.54	44.01	45.64	45.12	42.67	44.44
Change		-3.45	4.18	0.89	1.90	2.10	3.31	0.66	1.37	-1.22	-1.82
2010		34.15	39.77	40.86	41.78	41.16	43.33	46.08	44.53	42.45	43.98
2011	First three seasons	36.67	39.88	41.29	42.66	44.21	44.22	45.93	45.65	43.23	46.14
Change		2.53	0.11	0.43	0.88	3.05	0.90	-0.15	1.12	0.78	2.16

the employment during 2010-2011 that one might attribute to cash transfers. The fact that on average households lost working members, albeit imprecisely estimated, while those who kept their jobs worked more suggests that a slight negative labor demand shock occurred while those with jobs did not reduce their supply of effort, at least for those in deciles 2-8.

How did hours of work change by employment status, sector of work and occupation? First, note that wage ans salary workers (who reported it as their sole or main job) do not seem to have cut back on their hours of work (Table 9).

Workers in industry, which was hit hardest by sanctions, had slightly fewer hours of work,but those in agriculture and services increased their hours (Table 9). This finding is also consistent with the demand shock as a driver of reduced employment.

The breakdown of change in hours worked in Table 9 by occupations shows that workers in lower occupations (services and agriculture) did not reduce their hours worked, whereas those in the professional and technical category did, perhaps because of differences in the demand shocks. Again, we find no evidence here that

Table 9: Hours of work by employment status (wage and salary workers), main production sector, and occupation

Employment status	wage and salary workers only	wage and salary main source of income
Year		
2010	46.03	39.89
2011	46.20	40.58
Change	0.17	0.69

Main production sector	Agriculture	Industry	Services
Year			
2010	32.98	47.55	40.51
2011	33.39	47.00	42.01
Change	0.41	-0.55	1.50

Occupation	Professional & tech	Managerial	Clerical	Sales	Services	Agriculture	Production& transport
Year							
2010	40.04	43.56	47.38	48.68	35.91	32.94	45.29
2011	39.45	43.91	46.74	49.62	38.38	33.50	46.07
Change	-0.59	0.35	-0.65	0.94	2.47	0.57	0.77

Note: Only two groups of employment status is considered. Individuals who are only wage and salary workers, and individuals whose main source of income (more than 50%) is wage and salary.

poorer workers had less incentive to work as a result of the cash transfer.

4 Empirical results

We begin with a simple regression approach in which labor supply (hours worked) of individuals is regressed on a series of exogenous determinants of labor supply. We include both demand and suppply side determinants of labor supply. This method has some appeal because cash transfers were exogenous and by 2011 more than 95% of Iranians were receiving it. We ask if we can observe any effect of the unearned income from cash transfers on labor supply.

The results are presented in Table 10. Column 1 includes the least number of controls, but includes the main determinants of change in hours. The change in log wages is significant in all regressions, as is the ratio of the change in cash transfers to income (dctration).[4] What is interesting here is that controlling for a host of variables does not change the magnitude of the CT effect, which is always significant and positive, rejecting the hypothesis that those with large income shocks were more likely to reduce their supply of hours worked. The negative coefficient of log per capita expenditures (lpce) suggests that poorer workers had larger increases in hours worked, not less.

The last column in this table includes a labor demand variable (change in the rate of unemployment), which appears with a negative (but insignificant) coefficient, suggesting that individuals residing in districts with large increases in unemployment had fewer hours worked in 2011 compared to 2010. This is consistent with the reduction in labor demand after the cash transfers.

Next we use a quasi experimental approach in which we compare the labor supply of those who for whatever reason did not receive a transfer during the first three months of the program. Registration was open for a certain period before the program started and for the first month or so after it started but then was closed until after the Iranian New Year on March 20 2011. We identify roughly a third of the individuals in our sample as not having received transfers when the program was active in the last quarter of 1389 (2010/2011). We ask if their labor supply behavior differed from those who did receive transfers. We approach this question from the difference-in-differences perspective, assuming that selection into the program was independent of labor supply conditional on observed characteristics. We also perform matching estimation using the same framework of treatment and control.

[4]For most individuals this is the same as the transfer they received in 2011; for those who received transfer in 2010 as well, it is the difference between the two amounts.

Table 10: Regression of change in hours worked by individuals

	(1) dhours	(2) dhours	(3) dhours	(4) dhours	(5) dhours
Log change in wages	4.28***	4.37***	4.47***	4.57***	4.65***
	(0.25)	(0.26)	(0.26)	(0.26)	(0.33)
Change in cash transfers to income	1.48***	1.62***	1.55***	1.51***	1.32***
	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)
Energy share of total consumption	8.05	7.38	6.63	8.08	7.92
	(5.75)	(5.77)	(5.75)	(5.97)	(7.39)
Household size	-0.23	-0.11	-0.02	-0.024	0.03
	(0.18)	(0.18)	(0.18)	(0.19)	(0.23)
lpce †	-2.99***	-3.23***	-2.01***	-2.14***	-2.36***
	(0.39)	(0.44)	(0.49)	(0.51)	(0.65)
Female		0.87	1.46	2.19**	1.72
		(0.86)	(0.87)	(0.92)	(1.17)
Urban		-0.86	-0.32	-0.47	-0.08
		(0.56)	(0.56)	(0.58)	(0.73)
Years of education			-0.34***	-0.33***	-0.27***
			(0.06)	(0.07)	(0.08)
Age			-0.07***	-0.06**	-0.07**
			(0.03)	(0.03)	(0.03)
Change in district unemployment rate				-6.61	-7.04
				(4.67)	(6.39)
Private sector to total employment					8.75***
					(2.17)
Constant	-4.22	-2.49	-19.19**	-19.22**	-21.79
	(7.49)	(8.25)	(8.83)	(9.15)	(12.03)
N	4093	4093	4093	3823	2335

Standard errors in parentheses

** $p < 0.05$, *** $p < 0.01$

Dependent variable: change in hours worked per week.

† Log per capita expenditures.

DID estimates are run on the last quarter of 1389 (2010/2011), which corresponds to the first three months of the program. This reduces the number of observations by 3/4. The results are in Table 11.

Finally, we run the DID on the number of working members of the household (see Table 12). As before, the program impact is estimated by the coefficient of Year×Treatment, which is very close to zero (but insignificant), to providing any evidence of a decline in the number of members working between before and after cash transfers. The overall change in the number of workers per household is also very small and imprecisely estimated (though it is negative).

The rest of the coefficients show plausible values. Richer households had more working members, as did those living in rural areas, but more educated families had fewer working members.

5 Conclusions

The results in this paper dispute the popular claim that cash transfers have had a disincentive effect on the labor supply of individuals, in particular the poor. Economic theory suggests that unearned income reduces labor supply. However, this effect is likely to be small if the income elasticity of leisure is small or even positive if individuals have credit constraints so that the increased cash helps them invest in productive opportunities which also induces them to work more.

Table 11: DID estimates of program impact on house worked by individuals

	(1)	(2)	(3)
Urban	3.17***	4.00	3.36
	(0.77)	(0.76)	(0.76)
Treatment	0.77	1.34	-0.40
	(1.25)	(1.12)	(1.14)
Year	0.38	0.081	-0.02
	(0.91)	(0.89)	(0.87)
Treatment X Year	-1.25	-1.16	-1.19
	(1.61)	(1.46)	(1.44)
Household size	0.05	0.21	0.35
	(0.27)	(0.28)	(0.27)
lpce †	3.46***	5.85***	5.75***
	(0.62)	(0.77)	(0.79)
Female		-11.00***	-10.97***
		(1.69)	(1.62)
Age		-0.11***	-0.09**
		(0.04)	(0.04)
Years of education		-0.31***	-0.27***
		(0.10)	(0.10)
Interest income			-0.000000150
			(0.000000100)
Constant	-15.86	-49.68***	-46.96***
	(10.68)	(11.43)	(13.13)
N	3663	3663	3663

Standard errors in parentheses

** $p < 0.05$, *** $p < 0.01$

Dependent variable: hours worked per week by individuals.

† Log per capita expenditures.

Table 12: Change in the number of household members who work: DID estimation

	(1)	(2)	(3)	(4)	(5)
Year X Treatment	0.006	0.006	0.009	-0.0007	0.024
	(0.027)	(0.027)	(0.027)	(0.035)	(0.044)
Urban	-0.12***	-0.11***	-0.12***		
	(0.014)	(0.014)	(0.015)		
Treatment	-0.044**	-0.044**	-0.016	-0.015	-0.027
	(0.019)	(0.019)	(0.021)	(0.028)	(0.034)
Year	-0.021	-0.024	-0.021	-0.012	-0.038
	(0.016)	(0.015)	(0.015)	(0.021)	(0.024)
Household size	0.087***	0.087***	0.097***	0.095***	0.104***
	(0.0048)	(0.0048)	(0.0049)	(0.0071)	(0.0068)
Lpce	0.073***	0.078***	0.075***	0.060***	0.106***
	(0.011)	(0.012)	(0.012)	(0.017)	(0.019)
Years of education		-0.0068***	-0.0059***	-0.0021	-0.0146***
		(0.0016)	(0.0016)	(0.0021)	(0.0027)
Age		0.0048***	0.0049***	0.0041***	0.0059***
		(0.0005)	(0.0005)	(0.0008)	(0.0008)
Observations	4946	4946	4946	2092	2854

Note: Standard errors in parentheses. ** p¡0.05, *** p¡0.01. In the model in the first column, only size and expenditures of the household are controlled for. In second column, head age and years of education are added. Province dummies are added in column 3. Column 4 is for urban households only. And Column 5 for rural only.

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