Essays in Labor and Development Economics

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ABSTRACT

This dissertation provides program evaluation and policy analysis evidence from USA and Iran. The first chapter studies the impact of paid leave legislation on women employment. We employ California's first-in-the-nation Paid Family Leave program to draw inference using difference-in-differences and triple differences methods. The change in the employment outcomes for women before and after this program is compared to the change in similar outcomes for a set of control groups. We find that women's employment increased in the intensive margin but not extensive margin. We also find that wages increased for married prime-age and decreased for highly educated young women.

The second chapter provides evidence on the impact of a nation-wide unconditional cash transfer program in Iran on labor supply. As compensation for the removal of bread and energy subsidies in 2011, the government of Iran started monthly deposits of cash into individual family accounts amounting to 29% of the median household income. A popular outcry against the subsidy reform program has focused on the negative labor supply effects of the cash transfers on the poor. We use panel data to study the impact of these transfers on the labor supply of poor households and individuals during the first two years of the program, before inflation reduced their value.

We use the exogenous variation in the value of the cash transfers relative to household income to estimate the impact of the transfers on labor supply of individuals using fixed effects method. We also use a difference-in-differences methodology using the variation in the time households first started receiving transfers. Although everyone was eligible to receive cash transfers starting January 2011, about 20 percent of the households who for one reason or another did not submit their application in time, started receiving it three months later. Neither set of results support the hypothesis that cash transfers reduced labor supply as measured by hours of work or probability of employment.

The third chapter analyses what happens to the welfare of households and the budget of the government if it implements further price reforms in Iran. Five years into the reform, energy prices in Iran were still well below international levels. The impacts of a gradualist approach to price increase versus a one-off approach are simulated in this chapter. Under the gradualist approach government savings (reduction in foregone earnings) from selling subsidized items will increase by 20.2 trillion Rials or 0.18 percent of GDP in 2014. Half of these savings is needed as transfers to households to keep the poverty rate constant by paying each person 17,059 Rials per month. A one-off price increase would have a large effect on poverty and would require transfers equivalent to 203,775 Rials per person per month. Government savings after transfers would equal 96.4 trillion Rials or 0.87 percent of GDP.

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General Audience Abstract

This dissertation evaluates what happened to employment after the implementation of two programs; California Family Paid Leave program and Cash transfer program in Iran. It also predicts what would happen to the well-being of households if prices of energy carriers increase in Iran. The first chapter studies the impact of paid leave legislation in California on women employment. The change in the employment outcomes like hours of work per week and wages for California's women before and after this program is compared to the change in similar outcomes for other states. We find that women's employment increased after this program. We also find that wages increased for married prime-age and decreased for highly educated young women.

The second chapter provides evidence on the impact of a nation-wide cash transfer program in Iran on employment outcomes. As compensation for the removal of bread and energy subsidies in 2011, the government of Iran started a sizable monthly deposit of cash into individual family accounts. A popular outcry against the subsidy reform program has focused on the lower incentive to work especially on the poor. Neither set of results support the hypothesis that cash transfers reduced labor supply as measured by hours of work or probability of employment.

The third chapter analyses what happens to the welfare of households and the budget of the government if it implements further price reforms in Iran. Five years into the reform, energy prices in Iran were still well below international levels. The impacts of two approaches to price increase are simulated in this chapter. In the gradualist approach, prices increased 10% each year. In this approach government savings will increase by 20.2 trillion Rials in 2014. Half of these savings is needed as transfers to households to keep the poverty rate constant. A one-off price increase would have a large effect on poverty and would require transfers equivalent to 203,775 Rials per person per month. Government savings after transfers would equal 96.4 trillion Rials.

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Chapter 1

Impact of Paid Family Leave on Women Employment; Evidence from California Paid Leave Program

1.1 Introduction

Paid Family Leave (PFL) laws provide payments to parents of new born or adopted children when they take time off work to bond with their infants. Most industrialized countries guarantee the leave after the birth of a child and provide substantial amount of payments to employed mothers while they are on leave. US is one of the only four countries that does not have a universal paid leave program (Addati et al. 2014).[1] Only after the 1993 Family and Medical Leave Act (FMLA) US employees have access to an unpaid parental leave of up to 12 weeks. However, there is still no federal legislation that guarantees a payment for employees who take advantage of FMLA.

In the absence of a federal PFL, three states implemented their own paid leave programs[2] and some other states are in the verge of passing similar laws. Among them California is the first state to implement a paid leave program. Studying the impact of family leaves in general and California PFL (CA-PFL) in particular on households will be beneficial for other states that are in the process of making new policies. It may also encourage other states to start their PFLs.

Family leaves (paid and unpaid) are known to have several desirable impacts mainly on female workers and their children. Family leave laws are shown to increase leave taking by both mothers and fathers (Bartel et al. 2015 and Han et al. 2009) giving more time to parents to take care of their children specially at the early stage of their lives. Moreover, family leaves improve children's health condition by providing parents with more time to take care of their children. Tanaka (2005) and Ruhm (2000) found that paid leaves decreased probability of low birth weight and infant mortality rates in European countries. Studying the impacts of FMLA, Rossin (2011) found that the unpaid program in US have also reduced infant mortality rate and probability of low birth weight.

Women labor market consequences of family leave programs have also been studied in the literature. Ruhm (1998) found that maternity leave increased women employment in Europe. Addressing CA-PFL, Baum and Ruhm (2016) found that probability of employment, hours and weeks of work have all increased for mothers in the second year of child's life. Family leaves also increase mother's return to work especially in a short period after birth (Berger

[1] In fact, US is the only industrialized nation without a parental leave. The other nations are Swaziland, Lesotho and Papua New Guinea

[2] California, Rhode Island and New Jersey are the three states.

et al. 2005 and Berger and Waldfogel 2004).

Among labor market outcomes, impact on female wages have produced the most controversy. Theoretically, the impact on wages is ambiguous as conflicting forces will push wages up or down. On one hand, increased leave-taking by mothers can increase affected firms' costs. Moreover, leave laws increase female employment. Both of these factors tend to push estimated women's wages downward. On the other hand, family leave programs can increase wages because they preserve employee-employer relationship. Waldfogel et al. (1999) showed that PFLs can increase women retention, which in turn accelerates accumulation of experience, human capital and job tenure among women. This factor tends to increase women's wages. Whether the former factor has a larger impact or the latter is a question that needs empirical investigation. Not surprisingly, ex-post evaluations have found divergent results for different programs. Schönberg and Ludsteck (2007), for example, found that expanding maternity leave period lowered female wages in Germany. Conversely, Hashimoto et al. (2004) showed that mothers' of infants wages increased after FMLA.

CA-PFL is likely to affect leave-taking behavior and labor market outcomes for women since its compensation mechanism allows financially constrained parents to take more time off work. Moreover, almost all employees are eligible to take leave under CA-PFL whilst prior to that and under FMLA only 59% of private sector employees were covered Klerman et al. (2012).

In this paper I take the impact of PFLs on gender wage gap seriously. I use California first in the nation Paid Family Leave legislation to form a quasi-experiment to find the impacts on the wage gap for different socioeconomic groups.

This paper uses California's first-in-the-nation PFL to make a quasi-experiment for finding the impacts of paid leave on wages. Difference-in-differences (DD) and difference-in-difference-in-differences (DDD) methods are used to identify the causal effects and eliminate unobserved individual fixed effects.

I constructed a panel data set Outgoing Rotation Group (CPS-ORG) to answer this question. CPS-ORG provides hourly wage, weekly hours worked and earnings. Number of observations is much larger in this panel compared to the other designated panel data (NLSY and PSID). This fact provides the opportunity to probe into different demographic groups and find the heterogeneous effects of maternity leave on wages. This heterogeneous impact is the main contribution of this paper as it is not investigated in the numerous literature on family leave.

The paper is organized a follows. Section 1.2 provides details of the CA-PFL. Section 2.4 describes data and how the panel is constructed using CPS-ORG. Section 2.5 details the identification strategies, section 2.6 provide the estimation results and discussion of them and section 2.7 concludes.

1.2 Program description

On July 1, 2004 California became the first state in the US to implement PFL. The new legislation provides six weeks of paid leave with up to 55% of the usual pay replaced (up to $1,129 per week in 2016) for parents of newborn or adopted children in the first year of the child's life. The program is gender neutral and both fathers and mothers are eligible to participate[3]. CA-PFL was added to the state's pre-existing Temporary Disability Insurance program and is financed through payroll tax on employees with no direct cost for employers. CA-PFL does not guarantee job protection. However, employees may take advantage of FMLA to work for the same employee after the leave. FMLA requires employer to return the employee to the same job that she/he left or one that is nearly identical.

Prior to CA-PFL, California employees were (and still are) covered under FMLA. FMLA provides up to 12 weeks of unpaid leave for workers who has are employed in businesses of 50 or more employees and have remained with that employer for at least 12 months. Although the duration of leave under CA-PFL is shorter than FMLA, it may have different impacts for two reasons. First, less than 60% of private sector employees are eligible for FMLA (Klerman et al. (2012)) while coverage is almost universal under CA-PFL. Second, the compensation offered under CA-PFL makes it easier for financially constrained parents to take leave. Rossin-Slater et al. (2013) found that CA-PFL increased maternity leave taking by mothers of infants. They also found that the growth was larger for the "less advantaged groups".

[3]In the event that both mother and father are employed with the same facility, the employer can prevent them to take leave simultaneously

1.3 Data

The data for this study are derived from CEPR Uniform Extracts of the CPS-ORG. CPS Basic Monthly Data (BMD) is a rotating panel in which households are in the sample for 4 consecutive months, dropped for 8 months and interviewed again for 4 months. CPS-ORG are extracts of CPS-BMD at their fourth and eight month-in-sample (MIS). For these observations, hourly wages are reported in addition to weekly hours worked and earnings. This is desirable since the outcome of interest in this study is wage.

Given the structure of the data, the longest period between the two interviews of a household is 12 months. A panel is constructed using 2003, 2004 and 2005 rounds of CPS-ORG. Observations are merged over two consecutive years using their household and individual identifiers. Madrian and Lefgren (2000) showed that this method may result in two different individuals linked together. They suggest that dropping observations that show a change in gender over the two years or have an age increase of more than two years or less than zero will significantly reduce the number of false merges. For this study a similar procedure is applied and consequently 2% of observations are dropped.

The constructed panel covers observations from August 2003 to June 2005, and includes individuals interviewed once before and once after the implementation of the program[4]. Observations on July 2004 are dropped since this is the program start month and it is hard to know whether individuals were affected by the legislation or had the means to access to it. Table 2.3 gives a summary statistic for this panel. The sample size for this panel is 99,464. The total number of observations in CPS-ORG from August 2003 to June 2004 are 146,269. This gives an attrition rate of 32%. This attrition is close to similar studies like Madrian and Lefgren (2000).

In this paper the sample is restricted to individuals aged 20 to 50 to include only the individuals in working age. Variable 'years of education' is derived following Jaeger (1997).

[4]These individuals are in their fourth MIS before the program start date and in their eight MIS after that.

Table 1.1: Summary statistics for first year of the panel

	Mean	Std. Dev.	Min	Max
Age	46.16276	16.56272	16	85
Citizenship			1	5
Born in US	88.32			
Foreign born, Us citizen	5.92			
Foreign born	5.76			
Marital status			1	5
Married	60.34			
Widowed	5.87			
Divorced	9.98			
Separated	1.56			
Never married	22.25			
Labor force status			1	3
Employed	65.07			
Unemployed	2.85			
Not in LF	32.08			
Weekly hours worked	39.01629	13.57162	1	160
Weekly income	729.6737	542.2164	0	2884.61
Wage rate	17.79543	14.77589	0	2307
Female	0.531171	0.499029	0	1
Years of education	13.15997	2.765987	0	18

Observations: 99,464

1.4 Identification

This paper employs two identification strategies; DD and DDD. This section describes these methods starting from DD.

1.4.1 Difference-in-differences

In the first method, the change in hourly wage of Women in childbearing age in California (treatment group) is compared to that of control groups. Women in childbearing age are the ones that are most likely to be affected by this program. The treatment group is not restricted to women with children or women with infants for two reasons. First, employers are more likely to reduce all women wages, or at least married women's wages, not just

mothers. As mentioned in introduction, one possible reason for a negative impact on wages is that employers' costs may increase if a woman asks for maternity leave. Employers will then have the incentive to offer lower wages to the potential claimants of this program. Second, data restriction. It is not possible to identify whether the reason for leave taking was family leave or another factor like health problems of the employee or a close relative of her/him.

Two distinct control groups are chosen for this study. First, California men aged 20-49. Second, Women in childbearing age in the rest of the states. The second control group is used to make sure that the above mentioned results are not driven by gender-specific trends in California.

The identifying assumption for this model is that in the absence of CA-PFL, wages would have the same time trend across treatment and control groups. To check the "parallel trend" assumption, hourly wage for treatment and control groups from 1990 to 2015 are depicted in figure 2.4. Comparing California women and men, average wage for both groups started to increase from 1998, then, a downward trend started for men on 2001 and for women on 2002 and continued for both groups till 2014. Wages of women in other states have also increased from 1997 with a slight decline starting on 2011.

Another observation in figure 2.4 is that the distance between California men and women wages decline over time. Women in 1990 earned and hourly wage of around 79% of men on average, but this ration increased to 91% in 2015.

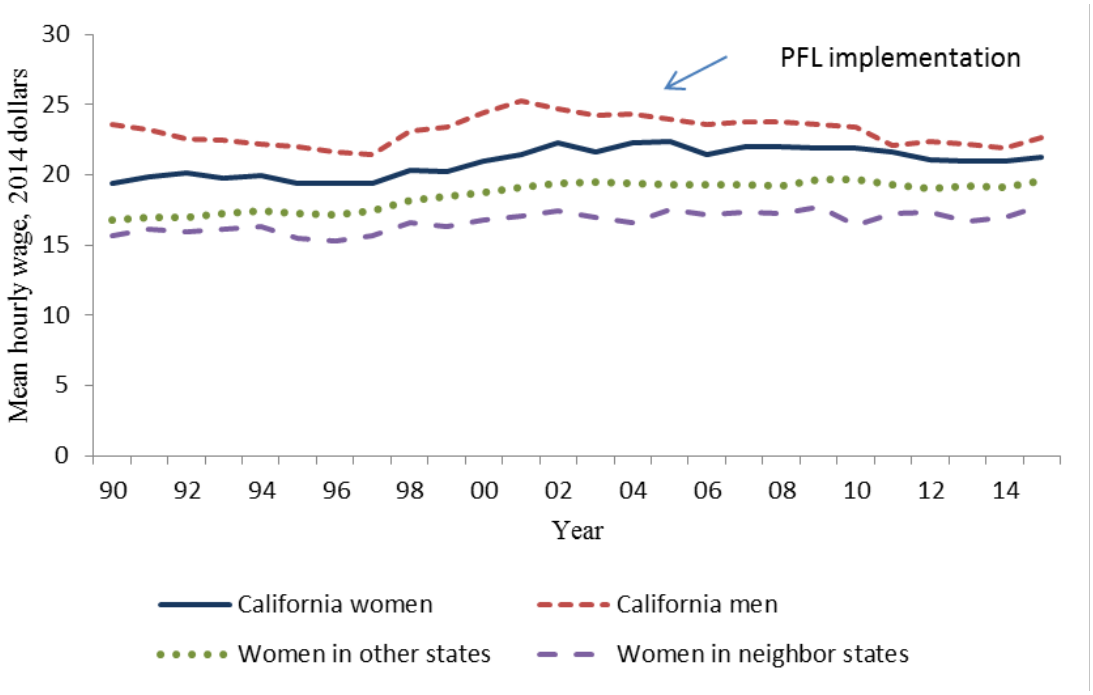
To further investigate parallel trend assumption, the moving average of the yearly change in wage is depicted in figure 1.2. This figure shows more clearly that the assumption is plausible.

The estimated equation for DD method is of the form:

$$Y_{it} = \alpha + \gamma_0 Treatment_i + \gamma_1 Post_t + \delta(Treatment \times Post)_{it} + \beta \mathbf{X}_{it} + \epsilon_{it}, \tag{1.1}$$

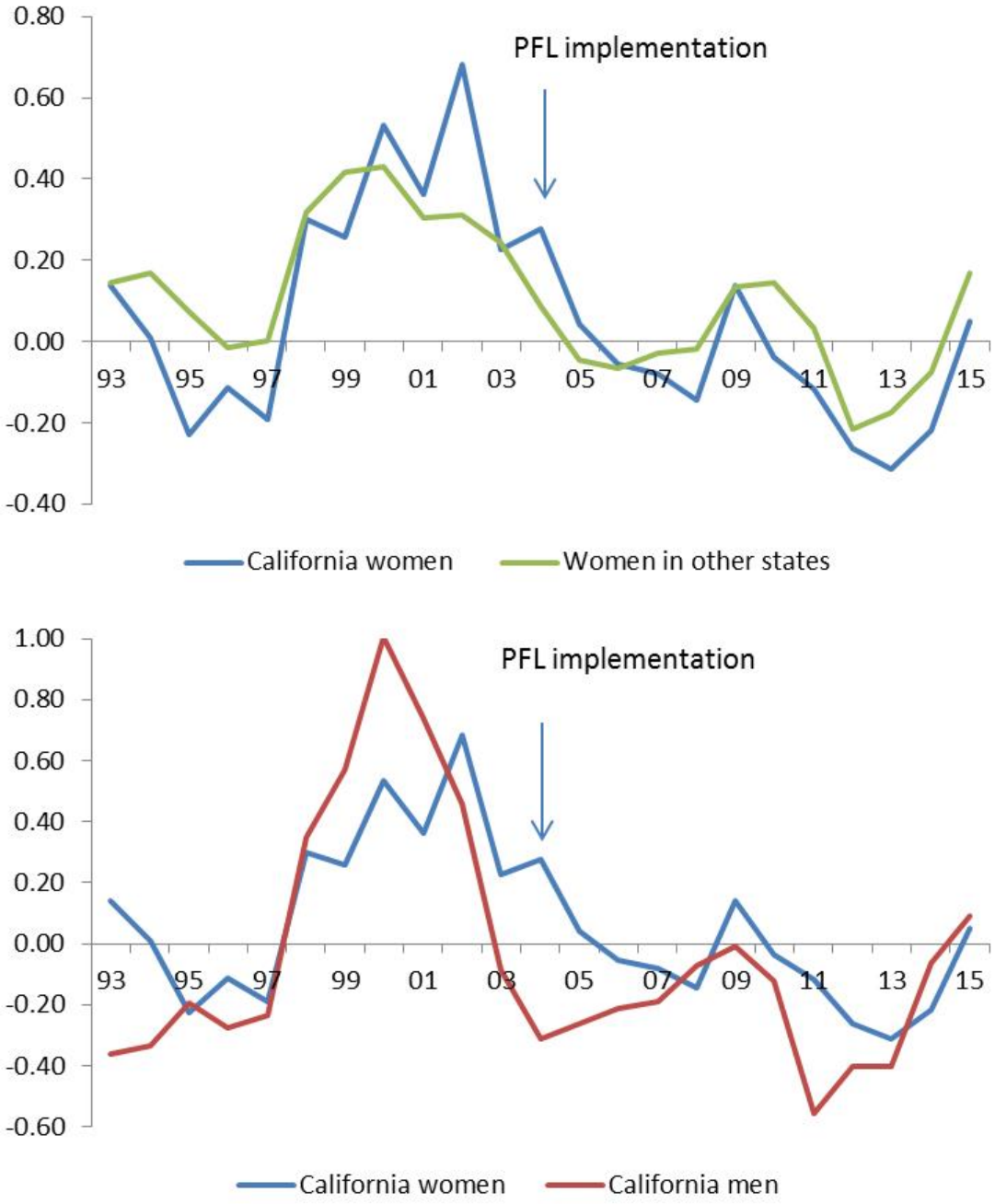
where Y_{ikt} is the logarithm of the hourly wage. $Treatment$ is an indicator that takes value 1 for individuals in the treatment group and zero for control group. $Post$ takes 1 for observations after the implementation of PFL and zero before that. $\mathbf{X}_{ikt}$ is a vector of demographic variables that includes years of education, potential experience in years, and experience squared. It also includes indicators for industry, occupation and state of residence. The

Figure 1.1: Hourly wage of California women, California men and women in other states



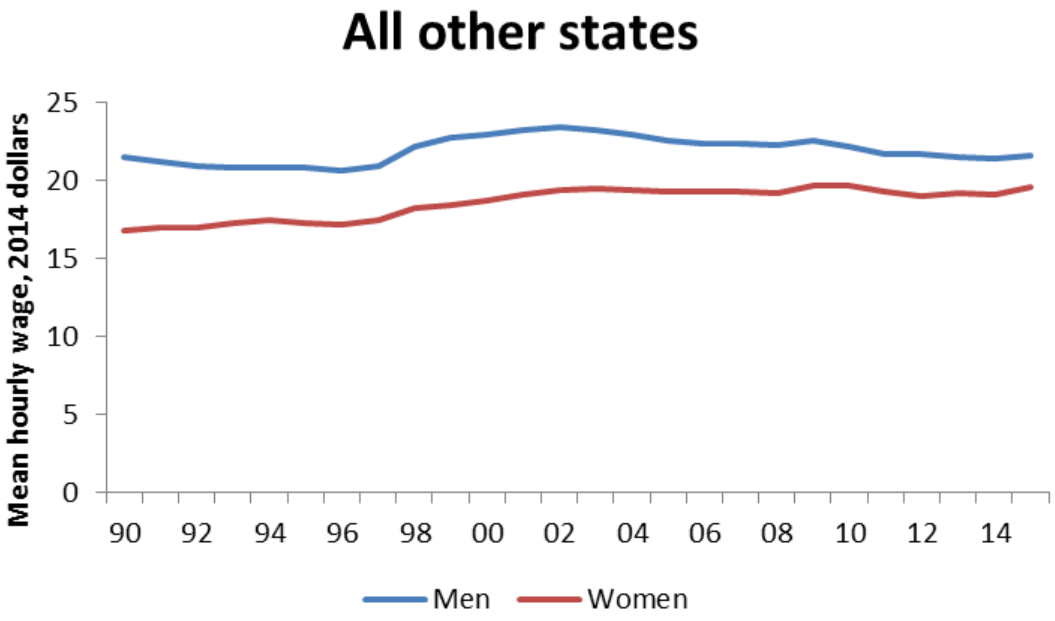
Source: CPS-ORG, 1990-2015.

Figure 1.2: Change in hourly wage of California women, men and women in other states, 3-year moving average



Source: CPS-ORG, 1990-2015.

Figure 1.3: Hourly wage of women and men in states other than California



Source: CPS-ORG, 1990-2015.

coefficient of interest is δ.

1.4.2 Difference-in-difference-in-differences

The other identification method is DDD. In this method the treatment groups is California women in childbearing age, which is the same as DD method. The two control groups are California men and women in other states.

As shown in figure 2.4 the wage gap between California men and women were declining over the 1990-2015 period. This fact casts doubt over the parallel trend assumption for the DD strategy. There is a similar trend for the wage gap in the whole country (excluding California) as shown in figure 1.3. Women in 1990 earned and hourly wage of around 82% of men on average, but this ration increased to 94% in 2015. Therefore an improvement in the DD identification will be to use the change in the gap in other states as a control for the trend in wage gap in California to remove potential bias.

The estimated equation in this case is:

$$Y_{ikt} = \alpha + \delta(CA \times Post \times Treatment)_{ikt} + \Gamma \mathbf{Z}_{ikt} + \beta \mathbf{X}_{ikt} + \epsilon_{ikt}, \tag{1.2}$$

where CA is an indicator for California residents. $Treatment$ takes one for women in child-bearing age and takes zero for men. $\mathbf{Z}_{ikt}$ is a vector of all interactions of CA, $Post$ and $Treatment$. δ is the coefficient of interest. The underlying assumption in this case is that the over time change in the wage gap (difference) between women and men in California is compared to the over time change of the wage gap in other states.

1.5 Results

1.5.1 Impact on wages

Estimation results for both identification strategies are presented in this section. The results for equation 1.1, when the control group are California Men, are given in table 1.2. Columns 1-3 report estimations for women aged 20-39. As indicated by the $Treatment \times Post$ coefficient, there is no significant change in wages when single and married women are pooled together or when the sample is restricted to single or married women in this cohort. In columns 4-6, the sample is restricted to three age groups of married women. For the 20-29 cohort, the estimated coefficient is negative but insignificant. For 30-39 cohort, the point estimate is 0.12 (indicating a 13% increase in wages) which is significant in 5% level. The 45-55 cohort is used to form a placebo test. The estimated impact on this group is not significantly different from zero (column 6). This is not surprising as women are not fertile at this age and they are not expected to get affected by the program.

Estimation results for DD method when control group is women in other states are given in the top panel of table 1.3. According to this table, there is a significant increase in wages of married women aged 20-39 with a point estimate of 0.06 (a 6.2% increase in wages). For 30-39 cohort, the point estimate is 0.09 (indicating a 9.4% increase in wages) which is larger than 20-39 cohort. Like the previous estimation, for 20-29 cohort there is no significant impact, but the point estimate is negative, which suggests that the wages may have possibly dropped.

I checked the robustness of the results using a control group consisting of women in the

Table 1.2: Impact of CA-PFL on women wages, DD

	Single and Married	Single	Married			
	(1)	(2)	(3)	(4)	(5)	(6)
Age cohort:	20-39	20-39	20-39	20-29	30-39	45-55
Treatment × Post	0.04	0.01	0.08	-0.04	0.12*	0.04
	(0.03)	(0.05)	(0.05)	(0.08)	(0.05)	(0.05)
Post	-0.02	-0.02	-0.01	-0.01	-0.01	0.00
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)
Treatment	-0.15***	-0.07	-0.23***	-0.15*	-0.23***	-0.23***
	(0.03)	(0.04)	(0.04)	(0.06)	(0.04)	(0.04)
Experience	0.04***	0.04***	0.02**	0.03***	0.02*	0.03***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Experience-squared × 100	-0.05***	-0.05**	-0.03	-0.04*	-0.02	-0.04**
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
Years of education	0.07***	0.09***	0.06***	0.06***	0.06***	0.06***
	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Adjusted R^2	0.419	0.395	0.418	0.454	0.403	0.403
Observations	3683	1508	2175	1677	2017	2248

Notes: Control group is California men. All regressions are controlled for occupation, industry and state fixed effects. Standard errors in parentheses. *:$p < 0.05$, **:$p < 0.01$, ***:$p < 0.001$.

Table 1.3: DD estimates of impact on women's wages, women in California versus all other states

	Single and Married	Single	Married			
	(1)	(2)	(3)	(4)	(5)	(6)
Age cohort:	20-39	20-39	20-39	20-29	30-39	45-55
Post × Treatment	0.03	0.00	0.06*	-0.04	0.09**	0.02
	(0.02)	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)
Post	-0.00	-0.01	0.00	-0.02	0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Treatment	0.15**	0.13	0.16*	0.15	0.15	0.12
	(0.05)	(0.07)	(0.07)	(0.13)	(0.08)	(0.06)
Experience	0.05***	0.05***	0.03***	0.02	-0.04**	-0.07***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Experience-squared	-0.00***	-0.00***	-0.00**	0.00	0.00***	0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Years of education	0.09***	0.09***	0.09***	0.10***	0.08***	0.08***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Adjusted R^2	0.40	0.42	0.37	0.41	0.35	0.34
Observations	19952	8885	11067	2979	8088	11156

Notes: Control group is women in other states. Dependent variable is log wage. All regressions are controlled for occupation, industry and state fixed effects. Standard errors in parentheses. *:$p < 0.05$, **:$p < 0.01$, ***:$p < 0.001$.

neighbor states of Oregon, Arizona and Nevada. The results are shown in table 1.4 and are generally similar to what we found earlier in this chapter.

Table 1.4: DD estimates of impact on wages, California versus neighbor states

	Single and Married	Single	Married			
	(1)	(2)	(3)	(4)	(5)	(6)
Age cohort:	20-39	20-39	20-39	20-29	30-39	45-55
Post × Treatment	0.02	-0.02	0.05	-0.03	0.08*	-0.00
	(0.03)	(0.05)	(0.03)	(0.08)	(0.03)	(0.06)
Post	0.00	0.01	-0.00	-0.05	0.02	0.06
	(0.03)	(0.04)	(0.02)	(0.05)	(0.01)	(0.03)
Treatment	0.07	0.08	0.07	0.16	0.01	0.20**
	(0.04)	(0.05)	(0.04)	(0.09)	(0.03)	(0.06)
Experience	0.03***	0.03**	0.02	0.01	-0.10*	-0.07
	(0.01)	(0.01)	(0.02)	(0.04)	(0.04)	(0.04)
Experience squared × 100	-0.04	-0.05	-0.02	0.08	0.22*	0.08
	(0.02)	(0.03)	(0.06)	(0.14)	(0.09)	(0.05)
Years of education	0.08***	0.08***	0.08***	0.09***	0.07***	0.05***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Adjusted R^2	0.380	0.394	0.361	0.416	0.336	0.327
Observations	2119	981	1138	304	834	918

Notes: Control group is women neighbor states of Oregon, Nevada and Arizona. Dependent variable is log wage. All regressions are controlled for occupation, industry and state fixed effects.
Standard errors in parentheses. *:$p < 0.05$, **:$p < 0.01$, ***:$p < 0.001$.

For the DDD strategy of section 1.4.2, the results are shown in table 1.5. The general findings of the previous two tables for married women can be seen in this table too. There is a positive impact on married women wages in cohorts 20-39 and 30-39 and the impact on the latter cohort is larger. This is possibly because the negative impact on the 20-29 cohort washes out the positive impact on 30-39 cohort and implies there is a heterogeneous impact on women wages.

1.5.2 Heterogeneity of impact

The results of section 2.6 suggest that the impact of CA-PFL is heterogeneous between demographic groups. To investigate this, the sample is re-grouped based on age and education level. The resulting four groups are: highly-educated 30-39, less-educated 30-39, highly-educated 20-29 and less-educated 20-29. Individuals with a college degree or above are classified as highly-educated and the rest are less-educated.

Table 1.5: Impact of CA-PFL on married women wages, DDD

	(1)	(2)	(3)
Age cohort:	20-39	20-29	30-39
Post × Treatment × CA	0.08*	-0.02	0.11**
	(0.03)	(0.06)	(0.04)
Post	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)
Treatment	-0.22***	-0.19***	-0.22***
	(0.01)	(0.01)	(0.01)
Post × Treatment	-0.01	-0.02	-0.00
	(0.01)	(0.02)	(0.01)
CA	0.14***	0.14***	0.14***
	(0.03)	(0.04)	(0.04)
Post × CA	-0.01	-0.01	-0.01
	(0.02)	(0.02)	(0.02)
Treatment × CA	0.01	0.02	0.00
	(0.02)	(0.04)	(0.03)
Experience	0.03***	0.03***	0.03***
	(0.00)	(0.00)	(0.00)
Experience-squared	-0.00***	-0.00***	-0.00***
	(0.00)	(0.00)	(0.00)
Years of education	0.07***	0.07***	0.07***
	(0.00)	(0.00)	(0.00)
Adjusted R^2	0.388	0.385	0.372
Observations	47423	39335	44444

Notes: Control groups are California men and women in other states. All regressions are controlled for education, experience, experience squared, occupation, industry and state fixed effects. Standard errors in parentheses. *:$p < 0.05$, **:$p < 0.01$
Source: CPS-ORG(2003-2005).

Table 1.6 presents the estimation results of the coefficient of interest for equation 1.1 when control group is women in states other than California. Columns 1 and 3 of this table show that there is a positive impact on wages for less-educated women aged 30-39 and 20-29. For highly-educated women the story is different as estimates are not significant for either group.

Table 1.7 shows the results when control group is California men. Columns 1 and 3 of this table show a positive insignificant impact on wages for less-educated women. However, the impact on highly educated youth (20-29 years old) is negative with a decrease in wages of 20%.

The DDD strategy also provides similar evidence. Columns 1 and 3 of table 1.8 show a

Table 1.6: Heterogeneity of impact on women wages, DD

Age cohort	30-39		20-29	
	(1)	(2)	(3)	(4)
Education level:	Low	Hi	Low	Hi
Post × Treatment	0.06*	-0.00	0.07*	-0.11
	(0.03)	(0.05)	(0.03)	(0.06)
Post	0.00	0.02	-0.03**	0.01
	(0.01)	(0.02)	(0.01)	(0.02)
Treatment	0.14	0.22	0.05	0.21
	(0.07)	(0.12)	(0.09)	(0.16)
Experience	-0.02	-0.04	0.06***	0.08*
	(0.02)	(0.03)	(0.01)	(0.04)
Experience-squared ×100	0.06	0.14	-0.10**	-0.23
	(0.04)	(0.08)	(0.03)	(0.17)
Years of education	0.05***	0.09***	0.07***	0.09***
	(0.00)	(0.01)	(0.00)	(0.01)
Adjusted R^2	0.221	0.166	0.282	0.226
Observations	7586	4488	5702	2176

Notes: Control group is Women in states other than California. Regression controlled for education, experience, experience squared, occupation, industry and state fixed effects. Standard errors in parentheses. *:$p < 0.05$, **:$p < 0.01$
Source: CPS-ORG(2003-2005).

positive impact on wages of less-educated women. A 7% increase for 30-39 cohort and 8% ifor 20-29. For highly educated youth, the wage decline is 17%.

Summarizing the effects, two out of three strategies show a positive impact on wages of low-educated women which is similar to what we saw in sections 1.4.1 and 1.4.2. For highly educated youth, two of the identifications show a negative impact. This outcome is not surprising. Highly educated employees are those who are hardest for employers to replace if parental leave is obtained and finding a replacement for them while on leave must be more costly. For this group the increased firm costs can affect wages in higher magnitude compared to the increase in retention and thus the overall impact becomes negative.

The results mentioned above could be affected by selection bias. (Baum and Ruhm 2016) showed that CA-PFL increased employment among women. Women with low wage rate are more likely to join labor force as a result of CA-PFL. This can bias the estimates of impact downward. Taking this bias into account, the results of this paper are a lower bound for the impact. Selection would not affect the sign of the estimated coefficient for married prime-age or less-educated women since the estimate is positive for these groups. However,

Table 1.7: Heterogeneity of impact on women wages, DD

Age cohort	30-39		20-29	
	(1)	(2)	(3)	(4)
Education level:	Low	Hi	Low	Hi
Post × Treatment	0.09	0.01	0.07	-0.22*
	(0.05)	(0.07)	(0.05)	(0.11)
Post	-0.03	0.02	-0.03	0.01
	(0.02)	(0.04)	(0.02)	(0.04)
Treatment	-0.20***	-0.15**	-0.08	0.10
	(0.04)	(0.05)	(0.04)	(0.09)
Experience	0.05***	0.07***	0.05***	0.08***
	(0.01)	(0.02)	(0.01)	(0.02)
Experience-squared ×100	-0.08***	-0.14***	-0.08***	-0.16***
	(0.01)	(0.04)	(0.01)	(0.04)
Years of education	0.05***	0.07***	0.05***	0.07***
	(0.00)	(0.02)	(0.00)	(0.02)
Adjusted R^2	0.284	0.211	0.327	0.280
Observations	2139	1075	2018	759

Notes: Control group is California men. Regression controlled for education, experience, experience squared, occupation and industry fixed effects. Standard errors in parentheses. *:$p < 0.05$, **:$p < 0.01$
Source: CPS-ORG(2003-2005).

the negative impact on highly-educated youth should be considered with caution.

1.5.3 Extensive margin of employment

To further explore the labor market outcomes I find the impact on extensive and intensive margins of employment. Table 1.9 shows the impact on the extensive margin using a specification similar to equation 1.1. In this specification the dependent variable is a dummy taking one if the individual is employed and 0 otherwise. Probit regression is used to find the probability of employment. As can be seen in column 1 there is no impact on employment. I used the same specification with the pooled cross section for column 2 and found no significant impact in this case too.

Table 1.8: Heterogeneity of impact on women wages, DDD

Age cohort:	30-39		20-29	
	(1)	(2)	(3)	(4)
Education level:	Low	Hi	Low	Hi
Post × Treatment × CA	0.07*	0.00	0.08*	-0.19*
	(0.04)	(0.05)	(0.04)	(0.09)
Post	-0.02**	0.02*	-0.02**	0.02*
	(0.01)	(0.01)	(0.01)	(0.01)
Treatment	-0.23***	-0.16***	-0.11***	-0.04*
	(0.01)	(0.01)	(0.01)	(0.02)
Post × Treatment	0.01	-0.01	-0.02	-0.01
	(0.01)	(0.02)	(0.01)	(0.03)
CA	0.07*	0.15	0.06	0.13
	(0.04)	(0.07)	(0.04)	(0.09)
Post × CA	-0.01	0.00	-0.01	-0.02
	(0.02)	(0.03)	(0.02)	(0.03)
Treatment × CA	0.03	0.05	0.01	0.17**
	(0.03)	(0.04)	(0.03)	(0.06)
Experience	0.06***	0.07***	0.06***	0.08***
	(0.00)	(0.00)	(0.00)	(0.00)
Experience-squared ×100	-0.09***	-0.13***	-0.09***	-0.14***
	(0.00)	(0.01)	(0.00)	(0.01)
Years of education	0.06***	0.08***	0.06***	0.08***
	(0.00)	(0.00)	(0.00)	(0.00)
Adjusted R^2	0.298	0.249	0.355	0.310
Observations	32797	15745	30913	12190

Notes: Control groups are California men and women in other states. Regression controlled for education, experience, experience squared, occupation, industry and state fixed effects. Standard errors in parentheses. *:$p < 0.05$, **:$p < 0.01$

Source: CPS-ORG(2003-2005).

1.5.4 Intensive margin

This section explores the impact on usual weekly hours of work. I explore two samples in this case using equation 1.1. The first is married women aged 20-40. Columns 1 and 3 of table 1.10 shows no impact on this group. The second, are women who took leave. for this groups I include women who took leave for maternity, vacation, or illness reasons. Column 4 of the table shows a significant increase in hours of work for the pooled cross section sample. This result is robust when the control group is changed to the far-west states. However, using the balanced panel which eliminates personal fixed effects shows no significant impact.

Table 1.9: Probbaility of employment of married women, DD

	Balanced panel	Pooled cross-section
	(1)	(2)
Post × Treatment	-0.01	0.00
	(0.03)	(0.01)
Post	-0.01	-0.00
	(0.01)	(0.00)
Treatment	-0.14**	-0.13**
	(0.04)	(0.02)
Age	-0.02**	0.01**
	(0.00)	(0.00)
Age squared	0.07**	-0.01*
	(0.01)	(0.00)
Pseudo R^2	0.0451	0.0407
Observations	17884	83844

Table 1.10: Impact on usual weekly hours worked, DD

	Balanced panel		Pooled cross-section	
	Married	Leave takers	Married	Leave takers
	(1)	(2)	(3)	(4)
Post × Treatment	1.32	0.03	0.09	2.43*
	(0.90)	(6.09)	(0.29)	(1.21)
Post	-0.11	0.68	0.11	-0.28
	(0.30)	(1.53)	(0.09)	(0.39)
Treatment	-0.08	11.71	1.08	-1.02
	(2.09)	(8.40)	(0.64)	(2.71)
Experience	-0.50**	-1.45	0.59***	0.25*
	(0.16)	(0.91)	(0.03)	(0.11)
Experience squared × 100	1.00*	2.80	0.72**	-0.32**
	(0.43)	(2.43)	(0.03)	(0.12)
Adjusted R^2	0.112	0.328	0.061	0.094
Observations	5122	210	91355	4534

1.6 Conclusions

This paper studies the causal effects of paid family leave on women wages. It uses California first in the nation paid leave program to form a quasi-experiment for studying the impact. Estimations using DD and DDD methods show favorable outcomes for married prime-age women as wages increased for this group. This is possibly because of the positive impact of the program on less-educated women.

Another finding of the paper is the decline of wages for highly-educated young women. This group is more vulnerable under a paid leave policy for the following reasons. First, being a high-skilled worker it is harder for firms to replace them while on leave. Potential employers may therefore offer lower wage rates or less benefits to this group. They may also be forced to work at positions with lower payments. Second, these people are at their early stage of their career and it is easier for them to be victims of discrimination since they have not established their relationship with their employers prior to the start of the program; a relationship that older women could have done in prior years. Third, they are more likely to have worked for less than 12 months for their employers and therefore their leaves are not job-protected since CA-PFL does not guarantee protection. It is under FMLA that employees can go back to their pre-leave jobs and the federal law have more binding restriction on eligible workers.

The other contribution of this paper is showing the heterogeneous impact of the program. It shows different impacts on different demographic groups. This might be one of the reasons that the literature has found divergent results for evaluating the impact of paid leaves on wages. The fact that the impact is not desirable for young women is important from policy making point of view since PFLs are designed to help women, not to hurt them. The drop in the wages for young women may be due to higher discrimination. More strict monitoring of discrimination against this group can alleviate the problem. Firms should also be instructed that there is no direct cost to them since all benefits are paid through the state's disability insurance fund.

Chapter 2

Cash Transfers and labor supply; Evidence from a Large-scale Program in Iran

2.1 Introduction

A central question in the debate on income assistance is the potential negative effect of transfers on the labor supply of the poor. Economic theory predicts that, if leisure is a normal good, an increase in unearned income reduces labor supply. In developed countries the potential disincentive effects of welfare programs has been widely studied and fostered key welfare reforms (Atkinson and Mogensen 1993; Moffitt 1992; Moffitt 2002). In developing countries, where cash assistance has been widely used to fight poverty, there has been little concern over the impact of cash transfers on labor supply of the poor, with greater focus on whether they actually achieve their goals of improving health and education. This divergence between the research and policy concerns of poor and rich countries is largely due to differences in the purpose for which these programs are intended. In developed countries income assistance programs are usually ongoing programs to provide social protection to individuals unable to earn enough from supplying labor, so it makes sense that their impact on incentives to work should be very important. In developing countries cash transfers are instruments for fighting poverty and promoting economic development, which are not expected to continue once the program has achieved its objectives. In this context learning about their impact on poverty alleviation and use it to design more effective programs (in kind vs. cash, conditional vs unconditional) takes precedence over their potential disincentives for labor supply.

We study a large cash transfer program in a developing setting, but one that has raised serious questions about labor supply. In 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, transfers amounted to 7% of the GDP (7.6% of the GDP per capita) and about 28% of the median household income. After three years of inflation the amount of transfer is down to less than 3% of GDP per capita, however, because of its national coverage it is still one of the largest in the world. In sub-Saharan Africa cash transfers have reached up to 40% of GDP per capita (Garcia, Moore, and Moore 2012), but these are smaller in size because they were given to smaller shares of the population.

The transfer program has been praised as innovative, free of leakage, and a more even and efficient way to distribute Iran's natural wealth compared to cheap energy (Guillaume, Zytek, and Farzin 2011). Although it was not specifically intended to reduce poverty and its real value has declined due to inflation, it remains popular with the poor and evidence shows

that it has contributed to lowering poverty and income inequality (Salehi-Isfahani 2016). The program is much less popular with commentators and policy analysts in Iran because of its alleged negative effect on labor supply of the poor. Anecdotal stories of poor workers leaving their jobs and small farmers abandoning their farms after receiving cash transfers abound.[1] Many Iranian politicians who opposed the program's founder, former president Mahmoud Ahmadinejad, have criticized it for "fostering beggars", implying an adverse impact on the labor supply of the poor.[2]

Economic theory has a strong prediction of a negative labor supply effect when cash transfers affect the tradeoff between work and leisure, such as means tested welfare or cash transfers that target the poor.[3] But the labor supply effect of universal and unconditional cash transfers in Iran is an empirical question because of imperfections in the markets for labor and credit. Unemployment has been in double digits for decades, and the marginal utility of leisure may be already too low for relatively small increases in unearned income to raise its consumption. Individuals may also be rationed in the credit market, which an infusion of cash relieves, opening up new opportunities for investment and consumption that were not possible before. This environment holds for many other developing countries and makes the study of the impact of cash transfers on labor supply more appealing in this context.

In this paper 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. The launch of the cash transfer program coincided with major shocks to the Iranian economy such as the tightening of international sanctions starting in 2011 and continuing in 2012 and devaluation of rial to one third of its value in slightly more than a year. It is very difficult to attribute changes in labor supply after the program to any one cause, in particular the cash transfer. In order to identify the effect of the transfers on labor supply, we take advantage of two sources of variation in treatment to identify impact. One is the variation in timing of registration for the program. For a variety of reasons, mostly unrelated to labor supply (e.g, loss of birth certificates, proving headship of household, etc.), roughly 20% of the eligible population started receiving cash transfer three

[1] 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).

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

[3] The Iranian cash transfer scheme was for several years not conditioned on income or wealth, and was universally applied, but in 2016 the law was changed to exclude high income families.

months after the start of the program (Salehi-Isfahani, Stucki, and Deutschmann 2015). We employ difference-in-differences for early and late registrants to identify the impact of cash transfers. The second source of variation is the difference in the intensity of treatment as measured by the share of net benefits (cash transfers minus higher energy bills) in total household income. We use this variation in treatment in a fixed effects scheme to draw causal inference for program impact. Both of these methods help us to get rid of the confounding influence.

Our paper contributes to three distinct areas of research. The first is the rich empirical literature on cash transfers in developing countries. Most cash transfer programs are conditional, for example on child school enrollment, presuming that the poor may not spend unconditional transfers productively. Conditional cash transfers (CCT) have been intensively studied and the overwhelming evidence is that they are generally effective in reaching their objectives (Case 2004, Bosch and Manacorda 2012, Schultz 2004, and Evans and Popova 2014). Recent evidence suggests that unconditional cash transfers (UCT) can also be effective in improving the welfare of the poor, and without the added cost of monitoring (Haushofer and Shapiro (2013), Blattman et al. (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 to work less. The evidence on the labor supply effect of these programs is mainly indirect, as implied by the observed response of income and consumption to the 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 of cash transfer consumed more food, healthcare, and education compared to the control group who did not receive a transfer. They also found that recipients increased asset holdings in the form of home improvements and increased live stock holdings. Blattman et al. (2013) and Blattman and Niehaus (2014) provide evidence of UCT programs in Uganda, where the unconditional nature of transfers did not result in the dissipation of the money into unproductive activities. Aker (2013) compares cash vs. in-kind transfers and finds evidence in favor of the former.(Bosch and Manacorda 2012), which specifically address labor supply, find no evidence of a negative labor supply effect of income assistance.

Iran's program differs from most programs of this kind because it is national and did not attempt to separate the population into recipients and non-recipients. Smaller programs can generate variation in treatment – if a control group is followed up – that can

greatly improve identification of impact. The large literature around programs such as Progressa/Opportunadis that designated control groups is testimony to the power of this design. Iran's program was offered to everyone from the start, so we have to use generated variation in participation from inside the national program.

Our paper is also closely related to the literature on 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 then taxed by the state to finance public expenditures (Diamond and Mosbacher 2013; Sala-i Martin and Subramanian 2008; Rodríguez et al. 2012 and Gupta et al. 2014). 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 ranging from \$600-\$4000 per month. [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. 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 Iranians without any interference by the state.

Finally, our study is related to the literature on the effect of unearned income on labor supply. Several papers examine the effect of lottery winning on employment (Imbens et al. 2001; Sila and Sousa 2014; Picchio et al. 2015). The evidence from these studies suggests that windfalls have a small negative effect on labor supply, mainly at high levels of windfall income. The negative effect could come from an increase in the marginal tax on wages of the winners rather than from unearned income. In our case, because income taxation in Iran is undeveloped, we do not expect any effect from the tax side.

The findings of this paper do not indicate a negative labor supply effect for hours of work

[4] Diamond and Mosbacher (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.

or probability of employment. There is a noticeable decline in participation and hours of work for some groups in 2011 after the program was implemented, which can be attributed to the general worsening of economic conditions as a result of the tightening of international sanctions against Iran in 2011. However, there is no significant difference between the change in the labor supply of our comparison and program groups.

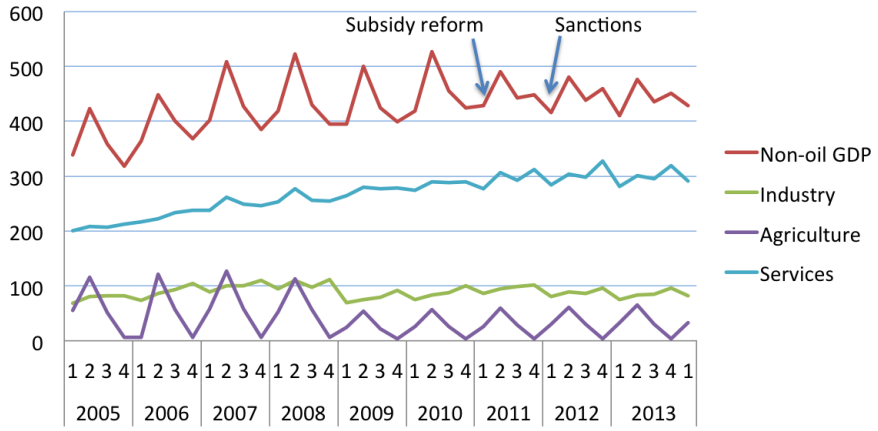
The paper is organized as follows. The next section describes the program and the Iranian context in more detail. Section 2.4 describes the source of our data and how we construct our panel of households and individuals, as well as the extent of sample attrition. Section 2.2 describes changes in labor supply before and after the program went into effect. It shows that a declining trend in labor supply preceded the implementation of the program, questioning the derivation of impact from regular time series on employment. Section 2.6 presents our empirical results, and section 2.7 concludes.

2.2 The setting

The most challenging part of determining the labor supply effect of the cash transfers is that only months after they started, Iran's economy and its labor market entered a period of decline and uncertainty. The primary reason was Western sanction against Iran, which tightened considerably during 2011, weakening Iran's oil exports and its currency. This makes the construction of the counterfactual for cash transfers very difficult. The tightening of the sanctions was anticipated, but the extent of their impact on the economy was in dispute. One of the reasons for removing the subsidies was to achieve self sufficiency in gasoline in the face of the coming sanctions.

Figure 2.1 shows the quarterly data on productive sectors and the non-oil GDP, which is more closely linked to employment than GDP including oil. Before 2011, the year in which subsidy reform and cash transfers were introduced, the economy was growing at about 5% per year; after this date growth approached zero. Figure 2.2 uses data from Iran's Labor Force Survey (LFS) to track quarterly movement in labor force participation, employment rate (extensive margins), and hours worked per person (intensive margin). Similar to the GDP, there is a flat trend in the extensive margin of employment in the years that follow the program's implementation. Before the program started, participation had a falling trend and employment was approximately stable. The average hours of work, the intensive margin,

Figure 2.1: The timing of various shocks to GDP, quarterly data by sector of production



Note: GDP is in constant 2004 rials $\times 10^{12}$.
Source: Central Bank of Iran, Economic Trends, various years.

exhibits a slight negative trend before the program among urban workers, which reverses itself in the subsequent two quarters. Rural working hours are highly seasonal but also show a slight rising trend after cash transfers, which is at odds with the anecdotal impressions noted in introduction.

The fluctuations in employment before and after the cash transfer program attest to the difficulty of gauging the program's impact on employment. The participation rate of urban workers in their 30s dropped from about 60% to 56% before the implementation of cash transfers. It is therefore difficult to ascertain from these trends to what degree the cash transfer program reduced labor force participation.

For individuals, table 2.1 shows hours of work using another data source, Household Expenditures and Income Survey which is conducted by Statistical Center of Iran. The table shows that hours of work increased for all quintiles of per capita expenditures. None of the changes in the table are significant, however. Despite the downward trend in labor supply noted in in the beginning of section 2.2, there is no evidence in the 2010-2011 panel for either an increase or decrease in labor supply that one might attribute to cash transfers.

Figure 2.2: Labor force participation, employment rates, and average weekly hours worked per worker

Note: All workers aged 15-64.
Source: Statistical Center of Iran, quarterly reports of Labor Force Surveys.

Table 2.1: Comparison of the 2010 base sample and the balanced panel

	Expenditure quintile in 2010				
	1	2	3	4	5
Year					
2010	36.2	40.6	42.4	45.0	43.7
2011	37.4	41.4	44.5	45.7	44.2
Change	1.2	0.8	2.2	0.7	0.6

Note: Note:
Source: Ctatistical Center of Iran, Household Expenditure and Income Survey 2010-2011

2.3 Conceptual framework

Estimating the impact of cash transfers on labor supply shares certain features with estimating labor supply functions, except that the focus on program impact and hence the change in labor supply before and after the program helps avoid many of the complications in the standard labor supply estimations. Most importantly, we can eliminate unobserved individual characteristics that do not change from one year to the next by using fixed effects. The complication of selection into labor market, which is critical in the case of Iranian women whose labor force participation is less than 20%, is a case in point. Therefore, our main focus is on the impact on prime-age men, although we briefly report the impact on women and the youth.

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. The scarcity of formal sector jobs means that employees in these sectors, both public

and private, are strongly attached to their jobs and may not withdraw their supply with modest levels of cash assistance. Workers in the informal sector, especially those working in harsh or unpleasant environments, are more likely to leave their jobs when they can afford to do so. Those with more flexible hours, such as the self employed are more likely to reduce their hours but not necessarily withdraw from market work altogether. In the case the self-employed, the direction of change in labor supply is not certain if they are credit constrained as the extra cash may help them expand their business and lead to more work. We allow for heterogenity of the impact of cash transfers by gender, income level, and type of work. When relevant, we focus on the labor supply of workers at the lower end of the income distribution.

Decisions about labor supply can be made at the individual or the household level with different implications for response to unearned income (Blundell and MaCurdy 1999; Donni and Chiappori 2011). We do not have detailed information about household decision making in Iran, but how the program distributed cash suggests that household heads play an important role. In our 2011 sample, 97% of those who received cash transfers were heads of household, suggesting that at least in registering for the transfers the household acted as a unitary decision maker. Of the remaining 3%, who resided in the same household but decided to get the transfer directly, by far the largest group was married sons. 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.

2.4 Data

Our data are derived from three rounds (2010, 2011, and 2012)[5] of the Household Expenditures and Income Survey (HEIS). This survey has been collected annually by the Statistical Center of Iran (SCI) since the 1960s. It is a nationally representative, two-stage stratified

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

(urban-rural and by province). 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 this survey 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. Table **??** in the Appendix shows the distribution of the monthly sample sizes.

Rotating panels are used primarily to reduce year to year fluctuations and to make consecutive year samples more similar. Households are not followed if they move to a new location unlike the designated panel data. Because their primary aim is not collecting panel data, attrition is a problem. Households are identified by their physical address, and when a family interviewed moves, next year its ID number is given to the new residents of that physical address. In addition, if a an individual leaves the household, his or her ID is given to the next member, so we had to construct our panel of individual based on age and sex of the members. Of the 38,285 households in 2010, 26,180 (68%) were designated as panel households to be re-interviewed in 2011, and the rest were designated to rotate out after one year. Of the non-rotating group, 17,234 households were actually found and reinterviewed in the second year.[6] These form our balanced panel. We drop an additional 5,603 households whose membership had changed from one year to the next, leaving us with 11,631 intact households in the panel, or 67% of the original panel. Table 2.2 compares the base sample with the constructed balanced panel and table 2.3 presents the summary characteristics of the intact panel. Note that if we restrict the sample to those who participate in labor market, 84% of the sample are men and 16% are women.

To give a better picture of the data we present the transition matrix for employment status of individuals in the 2010-2011 panel. Table 2.4 shows the proportion of individuals in each employment status (employed, unemployed, and inactive) in 2010 and 2011. Overall, this transition matrix exhibits 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. This

[6] In addition to those identified by the survey as having attrited, we excluded another 2,823 households because the age of the head and spouse had changed by more than two years or the gender of the head was changed.

Table 2.2: Comparison of the 2010 base sample and the balanced panel

	Balanced panel	Base sample (2010)
% urban	0.69	0.73
	(0.46)	(0.45)
Household size	3.66	3.76
	(1.54)	(1.63)
Number in labor force	1.23	1.28
	(0.95)	(1.01)
Number working	1.05	1.08
	(0.85)	(0.90)
Number of students	0.89	0.90
	(1.01)	(1.02)
Per capita expenditures (million rials)	35.00	36.93
	(40.15)	(37.66)
Head characteristics		
% literate	0.75	0.75
	(0.43)	(0.42)
Age	51.33	50.19
	(15.39)	(15.08)
% female	0.14	0.13
	(0.35)	(0.34)
Years of education	6.14	6.67
	(5.29)	(5.44)
Observations	11631	38285

Note: Summary statistics: household level, full sample and balanced panel.
Sd in parentheses.

is about the same number who lost their jobs in 2011 (434 versus 426). 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).

Attrition in panel-data is important if the households that drop out of the sample differ systematically from those that remain. In our case, attrition is high (33%) and appears selective. It is higher in urban areas, among renters, and higher income families (see Table 2.5). The employment status of the head of the household and the number of employed household members are also correlated with attrition (more working members 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 all the empirical analysis in this paper, in summary

Table 2.3: Summary statistics of working sample, 2010

	Mean	S.d.	Min	Max
Urban	71.06	45.35	0	1
Gross expenditures	34.82	31.63	2.92	652.12
Net income	30.72	25.21	-108.04	491.41
Cash transfers	0.55	0.37	0	5.02
Unearned income	6.16	13.10	0	218.09
Household size	4.24	1.49	1	14
Employment rate (%)	27.68	44.69	0	1
Lfp rate (%)	32.10	46.69	0	1
Hours of work per week	21.41	26.98	0	112
% female	51.58	49.98	0	1
Age	31.44	20.30	0	99
% literate	87.83	32.69	0	1
Years of education	6.63	5.08	0	24
Marital status:			1	4
Married (%)	74.40	42.61		
Widow (%)	2.00	13.97		
Divorced (%)	1.09	10.38		
Never-married (%)	22.02	41.44		
Observations	38,523			

Notes: Incomes and cash transfers are per person in million rials per year Source: HEIS

tables as well as in regressions. In general, the attrition weights we calculate do not affect the main results.

2.5 Program description and identification of impact

We take advantage of two features of Iran's cash transfer program to identify its impact, its universality and the fact that registration for the program was closed before everyone could register and re-opened three months after the start date. The 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 passed the Iranian parliament in January 2010 but the law was not implemented until December of that year when the government raised prices on bread and energy products

Table 2.4: 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 16+ Source: HEIS

Table 2.5: Attrition rates by place of residence, income group and home ownership

	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

by factors ranging from 2 to 9 and simultaneously released the cash it had deposited in dedicated household bank accounts.[7]

The transfers were critical in preventing a large negative income shock to households and forestalled potential social unrest that has often followed much less severe energy price ad-

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

justments (Harris 2010; Bacon and Kojima 2006; Beaton and Lontoh 2010). 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. This feature of the program allows us to treat cash transfers as external shocks to household and individual resources.

Although the size of cash transfers were uniform, they shifted household budget constraints at different rates. This variation can be captured by a measure of the *intensity of treatment*, which we define as the ratio of transfers (net of the increase in energy expenditures) to last year's household expenditures, or before transfers started (the same year's expenditures are affected by the transfer and will be endogenous to the model). For individuals in the top quintile of the expenditure distribution net transfers were only 4.9% of per capita expenditures whereas for the bottom quintile they amounted to 49.3% (see Table 2.6).

Table 2.6: Subsidy to expenditures ratio by expenditures quintiles

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

Note: Net transfers is transfers net of the change in energy expenditures. The ratio is net transfers to last year's expenditures.
Source: HEIS 2010-2011

The intensity of treatment thus defined is likely to be correlated with unobserved individual characteristics that affect labor supply and create a correlation between treatment and the error term. To break this correlation we used fixed effects, which in essence compares the change in labor supply before and after treatment for the same individuals subject to different intensities of treatment.

We complement the fixed effect results with a difference-in-differences method using another

feature of the program. 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 one-third of the population did not register in time to receive the stipend and had to wait three months to register.

This variation in timing of participation helps us define two groups of transfer recipients. One group are early participants who completed their registration on time and started receiving cash transfers in winter 2011. Clearly, this group also received transfers in winter 2012. The second group consists of late participants who registered after March 2011 and therefore received cash transfer in winter 2012 but not in the same quarter the year before. The former group was in the same position before and after March 2011 whereas the latter group experienced an increase in transfers in the second relative to first period. This variation offers the opportunity to estimate 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; Besley 1995). When they borrow, they either do so at exorbitant interest rates, or with collateral of equal value (Deaton 1997). Under these conditions, it would 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 in the change in the labor supply of the later receivers relative to the early receivers. This suggests a straightforward difference-in-difference identification methodology.

Inference based on the DID rely heavily on two assumptions. The first is that recipients in the winter quarter of 2011 (1389) are correctly identified (see section 2.4), 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 use detailed information on unearned income as recorded in the 1389 (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 2.7. The groups are similar in their main characteristics, though the program group is slightly older, poorer, and less educated. In the DID results, the difference between the two groups is captured by the estimated value of α, the coefficient of the treatment dummy, which captures the initial difference in labor supply between the comparison and program groups. Some difference is still captures after controlling for age and education of the individual. This difference is less than or very close to one hour per week for hours of work. These differences suggest that the two groups are fairly similar to begin with and with the conditioning on household characteristics they should provide a plausible basis for DID estimation. Table 2.7 shows the distribution of characteristics by program status for the two groups.

2.6 Econometric results

We divide the discussion of the estimation results into intensive and extensive margins. The application of fixed effects and DID to hours of work is straightforward, but in the case of participation, which is binary, it is more complicated. 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.

Table 2.7: 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

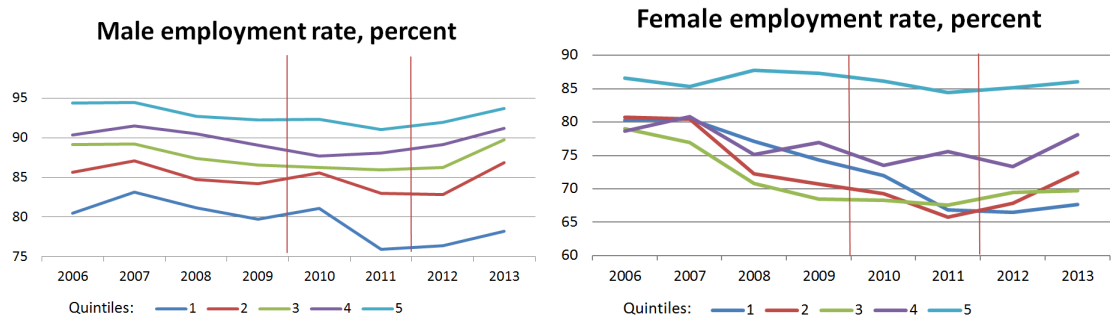
January-March 2011. Sd in parentheses.

2.6.1 Supply of hours worked

This section presents the estimation results of both identification strategies for the impact on hours of work. We first present the results for fixed effects and then for DID. It is more likely to find an impact in the hours of work if there is an impact on supply of labor to begin with. Given the situations of the labor market in Iran with high unemployment it is more sensible for workers to adjust their working hours rather than leaving the job market. So in this section we first look at the supply of hours in the market.

In this section we restrict our analysis to prime age (30-64) individuals. The results for wage and salaried workers and youth are presented in section 2.6.2 and for sectors of employment in section 2.6.3.

Figure 2.3: Checking parallel trend assumption for employment



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

Fixed effects

We begin with a simple, linear formulation of individual hours supply:

$$y_{it} = \alpha_0 + \alpha T_{it} + \mathbf{X}_{it}\beta + \lambda_i + \theta_t + u_{it}, \tag{2.1}$$

where y_{it} is labor supply of individual i at time t, T is treatment intensity, $\mathbf{X}$ are 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 can be correlated with λ_i and OLS estimates would be inconsistent. We can eliminate λ_i by first differencing, which yields:

$$\Delta y_{it} = \alpha \Delta T_{it} + \Delta \mathbf{X}_{it}\beta + \Delta\theta_t + \Delta u_{it}. \tag{2.2}$$

The standard assumption in fixed effects is that the time trend ($\Delta\theta_t$) is common. If this is not the case then our estimate of the program impact - as measured by the change in treatment intensity - could be biased. We check whether the time trend before program implementation is the similar for different quintiles of per capita expenditure. Figure 2.3 show the employment rate by quintiles for men and women. It can be seen from the figure that the pre-program trends are similar for men but differ for women across quintiles.

for different individuals from d for all individuals and is therefore not a source of bias. The fixed effects method uses the households with small intensity of treatment (mostly the rich) as controls of those with higher intensities of treatment (mostly the poor). We tried a difference-in-differences regression for high intensity individuals using those with low intensity as controls and found very similar results.

The results are presented in Table 2.8. The estimate of program impact on weekly hours worked for men is positive and significant, which is surprising in view of economic theory. However, the coefficient of log unearned income (excluding cash transfers) is, as expected, negative (and significant). For women neither coefficient is significant and both are very close to zero. 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 in equation 2.2. These columns present similar results to columns 1 and 3. If these results are valid, they reject the hypothesis of a negative supply response to cash transfers.

DID

The DID estimates here 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 did so only in the second period. The standard formulation of the DID equation is as follows:

$$y_{it} = \alpha_0 + \alpha T_{it} + \beta Year + \delta T_{it} \times Year + \mathbf{X}_{it}\beta + \epsilon_{it}, \tag{2.3}$$

where T is a dummy variable equal to one if the household received the transfer in the second period only, which means that α is the difference in labor supply between the two groups; β is the over time change in labor supply for the group that received transfer in both years, and δ is the program impact. As Salehi-Isfahani et al. (2015) show, the probability of being an early recipient varied by rural-urban residence, education, and gender of the household head. Hence, we control for the observable characteristics of individuals and households to preserve conditional exogeneity of the transfers. $\mathbf{X}$ shows these variables which are age, education level, marital status, rural residence indicator and household unearned income.

Our formulation of the DID equation is slightly different from the usual case because our comparison group is the treated in both years whereas the program group is treated only in

Table 2.8: 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 in parentheses * ($p < 0.05$), ** ($p < 0.01$).

the second year. Table 2.9 show how our model identifies the program impact. It is easy to see that a standard DID regression identifies the impact of cash transfers in our case. Here, the parameter δ of the interaction term in equation 2.3 is the program impact.

The DID results are presented in Table 2.10. These results are consistent with the fixed effects results, though the estimate of program impact (coefficient of Year x Treatment) is no longer significant for men. 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 1.4 fewer hours, though these differences are not significant. The other

Table 2.9: Identification of the impact in DID method

	Control (T=0)	Program (T=1)	Difference groups
Year=0 (1389)	α_0	$\alpha_0 + \alpha$	α
Year=1 (1390)	$\alpha_0 + \beta$	$\alpha_0 + \alpha + \beta + \delta$	$\alpha + \delta$
Difference years	β	$\beta + \delta$	δ

noteworthy coefficients are the large and positive effects of education on hours worked for women but not men. Like the fixed effects, the DID results do not provide any evidence of a negative supply response.

2.6.2 Heterogeneity in impact

We repeat the regressions of Table 2.10 for wage and salary workers, youth and youth who reported to work and attend school. The estimate of program impact for wage and salary workers is not much different from the whole sample: a positive but insignificant effect. Interestingly, we notice a negative impact for youth and students. The coefficient is significant for youth but insignificant for student-worker youth. This is not surprising since, unlike older workers, youth are in the early years of their careers and therefore less attached to their jobs. On average the youth who received cash only in winter of 2012 reduced 9 hours more than those who received cash in the winter of both years. The estimated impact of receiving cash was much larger – 23.5 fewer hours – for youth who were also student, though the number of observations is very small in this case. The effect of log unearned income on hours worked is not significant in the case of youth.

To sum up, the average impact on labor supply appears small, and positive if anything. The negative impact is limited to youth, especially those with the option to reallocate their time to education, which from a policy point of view should be a desirable outcome.

2.6.3 Heterogeneity in impact by sector of employment

Application of equation 2.3 to subsamples of individuals employed in agriculture, industry and services tests the most prevalent belief about the negative impact of cash transfers on agricultural workers whose jobs are physically demanding and seasonal. We could not find

Table 2.10: 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 ** ($p < 0.05$), *** ($p < 0.0$).

any evidence of an impact for workers in this sector. However, fixed effects estimate shows an increase of 36 minutes in hours of work for a 10% increase in intensity for service sector employees. DID estimate for the progrm impact in service sector is also positive but it is insignificant.

Table 2.11: DID: Individual hours of work per week, wage and salary workers

	Prime-age		Youth			
			All youth		Student youth	
	(1) Fixed effects	(2) DID	(3) Fixed effects	(4) DID	(5) Fixed effects	(6) DID
Intensity of Treatment	0.03		0.08		0.46	
	(0.02)		(0.05)		(0.31)	
Year × Treatment		2.31		-8.97*		-23.47
		(1.77)		(4.18)		(20.49)
Change in unearned income	-0.05		0.02		0.09	
	(0.03)		(0.03)		(0.06)	
Age	0.18	-0.50	-1.08	12.02	-16.28	-33.15
	(0.85)	(0.65)	(8.82)	(6.73)	(32.68)	(62.84)
Age squared	-0.00	0.01	0.00	-0.23	0.40	0.63
	(0.01)	(0.01)	(0.17)	(0.13)	(0.64)	(1.27)
Log unearned income	-0.15	-0.26**	0.32*	-0.00	1.75**	-0.42
	(0.09)	(0.06)	(0.14)	(0.14)	(0.53)	(0.75)
Year		-1.77		2.51		10.61
		(0.97)		(2.25)		(12.52)
Treatment		-2.42		5.23		4.66
		(1.49)		(3.53)		(29.97)
Observations	3215	2204	1251	945	72	62

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

2.6.4 Participation

Participation is a binary variable, which requires non-linear estimation leading to complications in the estimation of the fixed effects model (Greene 2004). We therefore limit our estimation of program impact on participation to DID method, which lends itself to the non-linear function. Following Eissa and Liebman (1996), we write the DID equation as follows:

$$\Pr[y_{it}] = \Phi(\alpha_0 + \alpha T_{it} + \beta Year + \delta T_{it} \times Year + \mathbf{X}_{it}\beta), \tag{2.4}$$

where $\Pr[y_{it}]$ is the probability of participating in the labor force for individual i at year t. Eissa and Liebman (1996) showed that equation 2.4 can be consistently estimated using a probit regression.

Table 2.12: Impact on individual hours of work 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.

Before looking at the estimation results for equation 2.4, it is useful to examine the simple transition matrix in table 2.13, which forms the basis for 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 2010 and 2011. 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 in 2010 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 present two sets of DID results. The first set is for the same two groups as in Table 2.10, the early and late participants (see Table 2.14). The second set compares individuals living in households with low and high intensity of treatment (see Table 2.15 for participation results for this group). The second DID results convey a similar assessment of impact as the fixed effects, but with a computationally simpler method (see our discussion above).

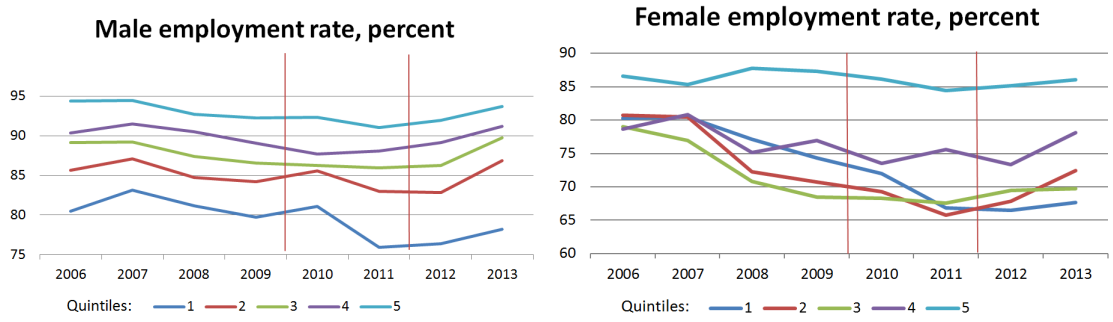
Table 2.13: Transition matrix of labor force participation status of men and women, 2010-2011

	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.

The comparison of low and high treatment intensity is very close to a comparison of poor and rich households (see Table 2.6), for which the assumption of parallel trends may not hold. However, in this case we do have a means to gauge its validity by looking at the trends in labor force participation of men and women in earlier years. Figure 2.4 shows that employment rates for men and women have moved together during this period.

Figure 2.4: Checking parallel trend assumption for employment



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

Table 2.14 shows the results of estimating equation 2.4 for early versus late participants. There is no significant impact on participation for men and women, however, the point estimate for is negative for men and positive for women. Participation of women increases by 3.6% for each year they become older. As with the hours for work, participation of men decreases if they live in households with more unearned income (compare with tables 2.10 and 2.8).

Table 2.14: 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.

The second identification compares those living in highly treated households with low treated ones (or poor with rich). The highly treated are those with an intensity of treatment of 20% or above and low treated are with intensity of less than 5%. This way, high intensity are households in the two richest quintiles and low intensity are those in the two poorest. There is still no significant impact on participation (see table 2.15).

2.6.5 The role of expectations

The government promise of a steady monthly transfer of cash affects the permanent income of households, which in the presence of a well functioning credit market can make the timing of the transfer irrelevant. Such a 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

Table 2.15: 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.

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.[8] We consider a program having started when households are able to enroll in the program and give a bank account number into which the cash transfers are deposited. Withdrawal from these accounts became possible at the same time, on December 19, 2010, for households in all provinces. About 850,000 individuals registered in these select provinces with cash being deposited but not withdrawable into their accounts before December 2010. Arguably, households in these provinces had formed their *expectation* of the change in their permanent incomes several months earlier than households elsewhere. If expectations of future cash transfers were as good as the cash itself, as they would be if credit constraints did not exist and the government promise of future payments were credible, we would expect an earlier shift in the labor supply

[8]The choice of the three provinces – Ardabil, Gorgan, Mazandaran – is not clear, except that Ahmadinejad had earlier served as provincial governor in Ardabil; they seem otherwise undistinguished.

behavior of households in the test provinces compared to the rest of the country.

We define a new treatment variable based on residence in the three provinces chosen for testing the program. In keeping with our previous definition of treatment, we define our comparison group as households in the three test provinces, who presumably experienced the positive shock to their permanent income in both summers of 2010 and 2011. We define the program (treatment) group as those living in the rest of the country who did not have this experience in summer of 2010 but did in summer of 2011. The DID equation is therefore the same as equation 2.3.

We present the DID results for hours worked in table 2.16, separately by age group and rural-urban residence and for the bottom 40% (a similar picture holds for the total hours supplied for the pooled household hours, which are not presented). 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 their labor supply. The workers in the bottom 40% had larger negative impacts, -5.37 compared to -1.38 hours per week. Despite the consistent negative estimates of impact, because they are very imprecisely estimated, 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 the assumption of a binding credit constraint we have made throughout this paper is plausible.

2.7 Conclusions

In this paper we examined impact of Iran's nationwide cash transfer program on labor supply. Critics of the program have advanced the claim, supported by economic theory, that the unconditional transfer of about $45 per month per person has reduces the labor supply of the poor. However, the size and the direction of the labor supply effect is theoretically indeterminate. Though leisure is a normal good, its income elasticity may be very small for low income people. Credit constraints, which are prevalent in any less developed country, may even cause labor supply effects if it enables credit constraint self-employed workers to invest in productive opportunities which induces them to work more. As a result, the question of impact of cash transfers on labor supply is an empirical one.

Table 2.16: Testing the effect of possible increase in permanent income: DID regression of change in hours worked

		Bottom 40%				
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Total	Urban	Rural	Youth	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 treated group. Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$.

We use panel data constructed from Iran's rotating expenditure and income surveys to examine if cash transfers in fact reduced labor supply. Answering this question is not straightforward because in the aftermath of the subsidy reform Iran's economy contracted, in part because of the shock of higher energy prices and in part because of international sanctions that intensified in 2011. We therefore employ estimation strategies – difference-in-differences and fixed effects – that help us identify the causal impact of the transfers on labor supply.

Our findings do not support the claim that cash transfers reduce the labor force participation or hours of work of individuals. While we observe negative program impact in a few places, the impact is only significant for youth, who probably lack investment opportunities and have the option to go to school.

We acknowledge several difficulties with our empirical tests, which may have hidden a negative causal impact from view. One is related to credit constraint and the DD estimation. If there is no credit constraint, the promise of transfers in the future is as good as payment

now, properly discounted, so the timing of the start of cash transfers, which we used in defining our treatment group, may not distinguish them from the control group with the same expected lifetime transfers. We tested the impact in a setting in which the credit constraints issue would not have arisen and still found no negative labor supply effect. We took advantage of a test launch in three provinces who were asked to register for the cash transfer (but not actually paid). We compared the change in labor supply of individuals included in the early treatment, who had a reason to expect an increase in their lifetime income with those in the rest of the country most of whom were not even aware of the plan to offer cash transfers months later and therefore did not have a similar income shock. The fact that we did not detect any difference in the labor supply behavior of the two groups suggests that either the expected increase in lifetime income did not play a large role in labor supply of the poor or that they did not take the promise of transfers in the future seriously. In either case this test strengthens our belief that any negative labor supply effects were negligible.

These findings did not surprise us. 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 not reason enough for a poor worker to quit his or her job. No doubt some did, especially those with marginal attachment to the labor market – like youth – or those in more physically demanding jobs. The important policy question is whether the reduction in work, if any, represents real loss of value to the economy. If an agricultural worker who works with hazardous pesticides and without proper equipment quits his or her job when he or she is able to live off the cash transfer, is the society any worse off? To answer in the affirmative, one would have to place a higher value on the driving of a rich person than the health of a poor agricultural worker.

From a more neutral policy perspective, the decision to transfer a part of the oil wealth unconditionally to citizens is a real one. There is no reason to believe that letting the government spend the money would produce higher value or that it would not cause distortions in the labor market or elsewhere in the economy. It is therefore important to know if such transfers would affect the incentives of their citizens to work and to acquire productive skills. Our findings shift the burden of proof to those who argue cash transfers make poor people lazy.

Chapter 3

Consumer Subsidies in Iran; Simulations of Further Reforms

3.1 Introduction

Iran is a major producer of oil and gas, and therefore it is not surprising that the country subsidizes energy heavily. In 1995 energy subsidies were estimated at $5 billion or 6 percent of gross domestic product (GDP) (Salehi-Isfahani et al. (1996)), and with rising world prices in the following decades, the subsidies rose several times over to reach more than 15 percent of GDP (Jensen and Tarr (2003); Salehi-Isfahani (2014)). During the oil boom of the 2000s, when the world price of energy trebled, the country's domestic price failed to keep pace, and subsidies ballooned. Despite several small adjustments in the domestic price of oil and gas since 1995, energy prices in the Islamic Republic of Iran have diverged from their opportunity cost.

In January 2010 a bold law was enacted that required the government to raise energy prices to a level equal to 90 percent of the free on board (FOB) price of energy in the Persian Gulf. The law also stipulated that the revenues from the price increases should be divided into three parts: 50 percent to compensate households, 20 percent to compensate firms, and the remaining 30 percent to be added to government revenues. In December 2010 prices of consumer goods were increased, by factors ranging from 2 (for bread) to 9 (for diesel), and monthly cash transfers of 455,000 rials (Rls), or about $90 (U.S. dollars) in purchasing power parity (PPP) per capita started reaching about 95 percent of the population. Although the reform was successful in raising energy and bread prices several times over and the cash transfer scheme allowed the price shock to go forward without any protest, four years later much of the program's initial gains have been lost to inflation, and opposition to further sharp price adjustments is strong. In the meantime, the collapse of the price of oil in the world markets has narrowed the gap between prices in the Islamic Republic of Iran and the outside world, diminishing the urgency of further subsidy reform. President Hassan Rouhani, who took office in August 2013, introduced the second phase of price increases, raising the average price of energy and bread by about 30 percent. His administration appears determined to follow up with gradual increases in energy prices. This chapter examines the consequences of further price reforms for consumer welfare and the government budget. It presents simulation results that compare the effects of gradual price reform, which is the likely course of action, with a one-time increase that removes all the subsidies, which is similar to the 2010 reform. Although energy subsidies are lower than they were in 2010, the logic of removing them is stronger, especially for the government. Lower world oil prices, which have ostensibly reduced the need to raise domestic prices, have at the same time made it more urgent for

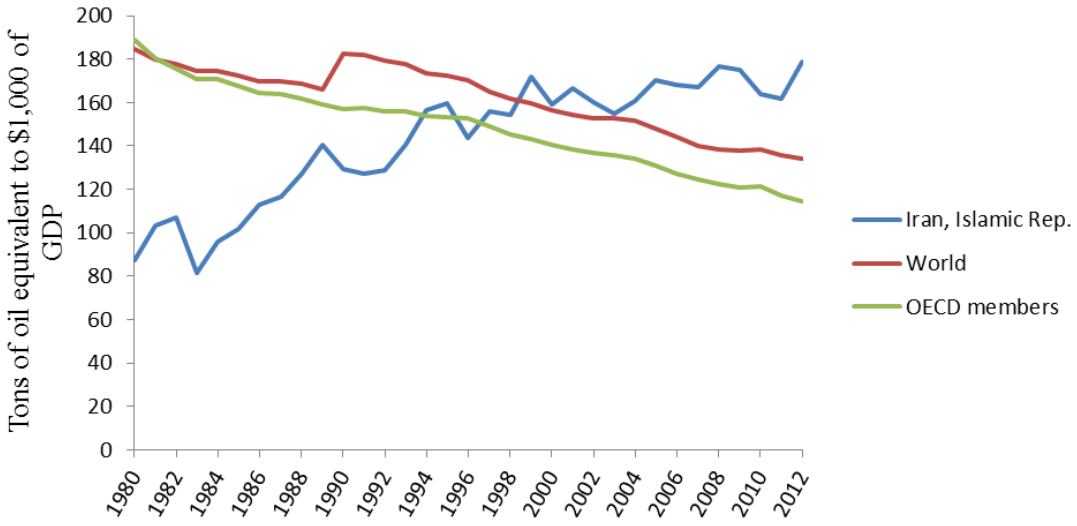
the government to seek more revenue from its domestic sale of energy, which is more than three times what it exports.

Besides budgetary concerns, energy subsidies raise equity issues because they distribute the national hydrocarbon wealth unequally. This chapter shows that subsidies for energy products accrue mainly to upper-income groups, who use more energy than the poor. Efficiency is another concern. Decades of cheap energy distorted Iranian production to be more dependent on energy and less efficient in its use. As shown in figure 3.1, before 1987 Iran consumed less energy for each dollar of production compared to the world and Organization for Co-operation and Development (OECD) countries. Since then the country has increased its use of energy per dollar of GDP, and the rest of the world has decreased it. In 2009 the Islamic Republic of Iran consumed 50 percent more energy per unit of GDP than the rest of the world. Moreover, subsidized energy is detrimental to the environment. The country produces more than its share of greenhouse gases, and pollutants have made the air in its major urban centers unbearable. As with snow days in the United States, Tehran's schoolchildren get days off from school because of pollution, which has become a part of normal life. Finally, low energy prices have also encouraged the use of capital-intensive technologies, which limit demand for labor at a time when youth are entering the labor force in record numbers.

There is a small literature on Iran's subsidy reform. Several papers describe the reform. Guillaume et al. (2011), Salehi-Isfahani et al. (2015), Salehi-Isfahani and Mostafavi-Dehzooei (2014), and Salehi-Isfahani (2014)) evaluate the impact of the cash transfer on household labor supply. Gahvari and Karimi (2016) use an Almost Ideal Demand System (AIDS) model to study the reform and find that cash transfers improve welfare, at least for poor deciles. Gahvari and Taheripour (2011) use pre-reform data and the Quadratic Almost Ideal Demand System (QAIDS) to predict the impacts of a price reform in the country. In their general equilibrium framework, they find that eliminating subsidies for utilities results in substantial welfare losses. Jensen and Tarr (2003) use a computable general equilibrium (CGE) model to simulate the effect of reform of subsidies and find that "even nontargeted direct income payments to all households (not just the poor) would enormously and progressively increase the incomes of the poor."

The plan of this chapter is as follows. The next section offers a more detailed account of the evolution of subsidies and is followed by a section that explains our sources of data. The next sections derive the distribution of subsidies as they existed in 2013, present the simulations results, and discuss the political economy of subsidy reform.

Figure 3.1: Energy Consumption in the Islamic Republic of Iran, the World, and OECD Countries



Source: WDI, various years and authors' calculations.
Note: OECD is Organisation for Economic Co-operation and Development.

3.2 Evolution of Subsidies

Iran has subsidized a variety of goods besides energy - bread and medicine, in particular - but energy subsidies have been by far the largest part and the part that has increased the fastest in recent decades. One reason for this increase was the rise of global prices. From 1999 to 2008 the price of oil increased tenfold, raising the opportunity cost of oil used domestically and the amount of subsidies to oil-based products. Energy subsidies have also increased because domestic consumption of oil and gas has grown from about 1 million barrels per day (mbd) in the 1970s to about 4 mbd oil and gas in 2013.

In oil exporting countries, subsidies tend to rise and fall with the global price of energy. Governments let energy prices stagnate during the periods of rising global oil prices because they are flush with revenue and see no need to charge domestic consumers the world price. Distortions increase further because the expenditure of rising oil revenues leads to inflation, led by the price of non-tradable goods and services, which reduces the price of energy products relative to other goods. At the end of an oil boom, as in 2014-15, revenues from exports decline, and governments become more interested in eliminating subsidies. The Iranian gov-

ernment delivers more than 4 million oil equivalent barrels of energy (gasoline, natural gas, and electricity) each day to consumers inside the country. In 2013, before the collapse of oil prices, the total value of this energy reached $100 billion per year. With the domestic price of energy roughly about one-third of the world market, some $66 billion of this can be counted as subsidy. In 2014, as a result of the collapse of oil prices, the amount of the implicit subsidy declined substantially. Given the uncertainty about the future price of oil, it is difficult to define a zero-subsidy price for future years.

A major part of subsidies in the Islamic Republic of Iran are implicit and due to the gap between the domestic and world price of energy, but a good part, especially the subsidies for food and medicine, are explicit and are financed from the general budget and therefore compete with other expenditures more directly. The rationale for both types of subsidies is social protection. Protecting the poor was a widely advertised slogan of the 1979 revolution. Although subsidies existed for many of these commodities before the revolution, they took a more essential role as the ethos of the populist state.

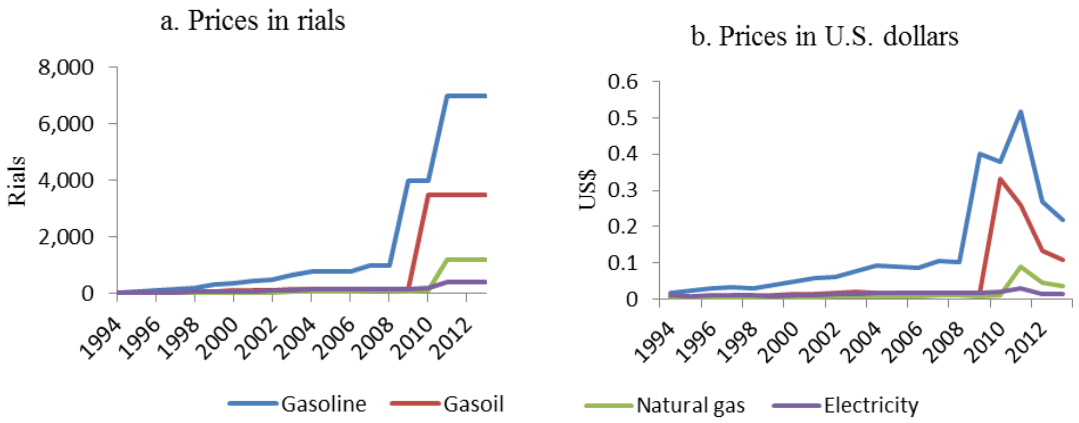
There were several attempts at energy price reform in the 1990s, but none succeeded in closing the gap between prices in the Islamic Republic of Iran and the world markets to any significant degree. During the administration of President Mohammad Khatami (1997-2005), the conservative political opposition dominated the parliament and stymied any major reduction in subsidies. In 2004 the conservative-dominated parliament passed a law preventing the government from raising energy prices.

Figure 3.2 shows the history of energy prices since 1994 in Iranian rials (Rls) and in U.S. dollars ($).1 The impact of fixing the price of energy products is visible in this graph after 2004 when global crude prices doubled.

Khatami's populist successor, Mahmoud Ahmadinejad, had the support of the parliament for energy price reform, but little was done on this during most of his first term (2005-09). In 2008 the government and the parliament started discussions for a major price reform, which eventually became the Targeted Subsidy Reform Act in January 2010, six months after Ahmadinejad's controversial election to his second term, 2009-13. Subsidy reform was the centerpiece of his economic program, but its implementation was delayed until December 2010, when prices for bread and energy products were raised in one go by factors varying from 2 to 9 times.

The decision whether to increase prices in one step or gradually was a difficult one. Gradual

Figure 3.2: Energy Prices in Iran, 19942012



Source: Iran Ministry of Energy, 2013. Note: During much of this period the Islamic Republic of Iran had multiple exchange rates. We use the rial-dollar exchange rate that is reported by the Central Bank of Iran for the parallel or free market. For energy prices with two rates, rationed and free, we use the latter.

increases are preferred if they can be maintained over several years as prices catch up with their intended targets. In the Islamic Republic of Iran the experience with gradual increases had not been encouraging. Getting both the government and the parliament to commit to future increases proved unsuccessful because of the country's fluid politics. Small increases in one year were rarely followed by further increases as the powerful lobbies for low energy prices (such as the petrochemical and auto industries) often mustered enough support in the following year to block further increases. This experience, plus the government's interest in generating enough revenue for redistribution, provided the impetus for shock therapy.

The reform included a massive cash transfer program, which was launched simultaneously with the price hikes. The cash transfer program was efficiently executed, depositing Rls 445,000 per person per month in individual bank accounts. Initially, this amount was 28 percent of the median household income, and 50 percent of the income of a minimum-wage worker with a family of four (Salehi-Isfahani et al. (2015)). According to the government, during the first four months of the program, about 62 million people (about 82 percent of the total population) started to receive cash transfers. This number increased quickly to cover about 95 percent of the population. Survey data indicate that coverage in rural areas where banks are less accessible was lower than in urban areas (Salehi-Isfahani et al. (2015)).

3.3 Data

The data used in this chapter are derived from the Household Expenditures and Income Survey (HEIS) collected annually by the Statistical Center of Iran (SCI). The survey is nationally representative and two-stage stratified, at the urban and rural level and by province. The survey is weighted, and the sampling weights are provided by the SCI. This survey includes information on expenditures and incomes of urban and rural Iranian households. We use the most recent sample collected in Iranian year 1392, which corresponds to March 20, 2013, to March 19, 2014, and we refer to it as 2013-14 hereafter.

Table 3.1 presents the descriptive statistics for the 2013-14 sample. The survey frequencies have been inflated using sampling weights to reflect population level values. The population of 80 million is divided into ten equal size deciles (with varying number of households). Per capita expenditures is Rls 53 million per year (about $1,664 and 6{,}200$ in PPP).

Table 3.1: Population and Household Expenditures, 2013-14

Expenditure decile	Population ($\times 10^6$)	Number of households ($\times 10^6$)	Household size	Total expenditures ($\times 10^12$ rials)	Expenditures per capita ($\times 10^6$ rials)	Expenditures per household ($\times 10^6$ rials)
1 (poorest)	8.1	1.8	4.5	116.4	14.5	65.4
2	8	1.9	4.2	174.9	21.7	92.1
3	8	2	4	217.9	27.1	109.3
4	8.1	2.1	3.8	260.4	32.3	123.9
5	8	2.2	3.7	304.2	37.9	140.4
6	8.1	2.3	3.6	358.6	44.5	158.3
7	8	2.3	3.4	426.1	53	182.2
8	8	2.4	3.3	516	64.1	213.1
9	8.1	2.6	3.1	671.2	83.4	256.2
10 (richest)	8.1	3	2.7	1,242.80	154.4	409.8
Total	80.5	22.6	3.6	4,288.50	53.3	189.6

Source: Authors' calculation HEIS 2013. Note: We use the sampling weights provided for the HEIS by the Statistical Center of Iran to inflate sample values to population level. These weights overestimate Iran's population by about 3 million.

Prices of subsidized items were set through both government control and subsidy. For bread, for example, the government bought domestically produced wheat at Rls 10,150 per kilogram in 2013-14, which was close to international market price. Wheat was then sold at the

subsidized price of around Rls 460 to flour producers, who sold it at Rls 5,900 ($0.20) per kilogram to bakers. The government then controls the price of bread sold at bakeries: each kilogram of bread was then sold at Rls 10,274. In rural areas where households bake their own bread, the government sells flour up to a quota at subsidized price.

Liquefied petroleum gas (LPG) is also sold at a subsidized price mostly in regions without natural gas pipeline. Alongside bread, LPG and kerosene have linear pricing, but other subsidized items are subject to nonlinear pricing with quotas that vary according to season and a region's climate (natural gas and electricity) and type of vehicle (gasoline and diesel). LPG sold at Rls 1,800 ($0.06) per kilogram at the time, and the kerosene price was Rls 3,500 ($0.11) per liter. Prices of subsidized goods are given in table 3.2.

Table 3.2: Price of Subsidized Items and Free Market

	Gasoline (Liter)		Diesel (liter)	Kerosene (liter)	Natural gas (m^3)	LPG (m^3)	Electricity (kWh)	Bread (kg)	Flour (kg)
Price in 2013	Up to 60 liters	More than 60 liters							
Iran	4,000	7,000	3,500	1,000	742[b]	1,800	337.5[b]	10,274	5,900
Free market	23,811[a]	23,811[a]	22,986[a]	22,639[a]	13,317[c]	10,800[c]	4,800[d]	21,800[e]	14,700[e]

Source: Ministry of Energy 2013.

a. Based on FOB Persian Gulf price, Platts.com.

b. Effective national average price, Ministry of Energy 2013.

c. Average Europe price, FERC and www.cngeurope.com, 2013.

d. Price in Turkey, Turkish Statistical Institute 2013.

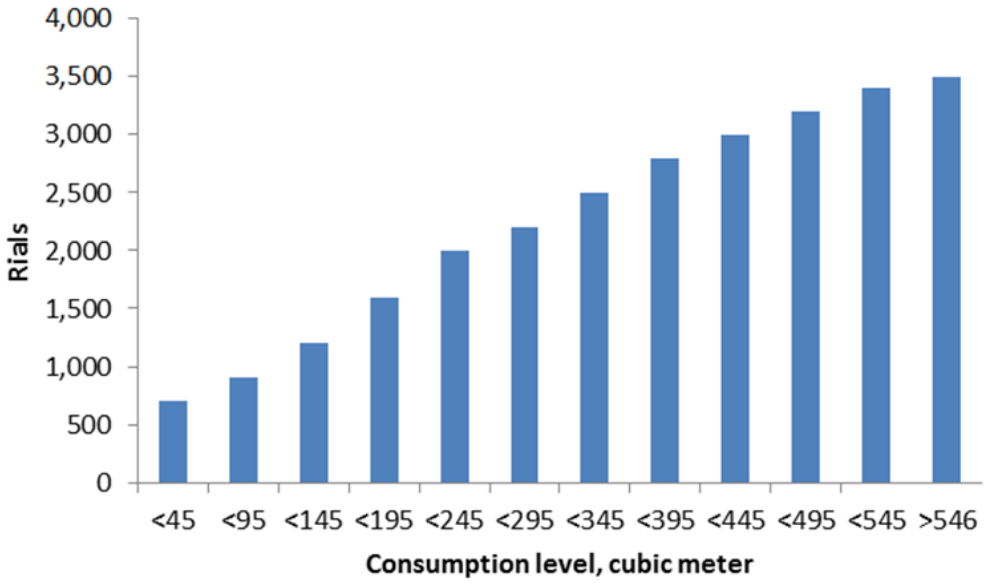
e. Based on international wheat price and authors' calculations.

Gasoline had a two-tier price to begin with: Rls 1,000 per liter for rationed and Rls 4,000 for free market gasoline from 2010, and these prices rose to 4,000 and 7,000, respectively, in 2013. To control the quota, all vehicles have an electronic card that helps the government keep track of their monthly consumption. The quota differs by type of vehicle. Motorcycles had 25 liters per month of the subsidized gasoline in 2013-14. Cars, other than taxis and government vehicles, had 60 liters. In our data, we have the information only on how much gasoline each household bought altogether, but a household may have a car, a motorcycle, or both. In our calculations we assume that all consumed gasoline is used in cars.

Natural gas and electricity prices have more tiers, and they also depend on the season and regional climate. The effective national average price of natural gas was Rls 742 per

cubic meter (m^3) (Ministry of Energy 2013). Prices started at Rls 700 per m^3 (about \$0.01), increasing to Rls 3,500 (about \$0.12) for large users. Similarly, the average price of electricity for households was Rls 337 per kilowatt hour (kWh), with tariffs increasing from Rls 300 to Rls 2,150 per kWh. The rising tariff for natural gas is shown in figure 3.3.

Figure 3.3: Natural Gas Price Schedule in 2014, in rials per cubic meter



Source: National Iranian Gas Company, 2013.

3.4 Distribution of Subsidies

This section describes the distribution of subsidies for bread and energy products in 2013-14. Calculating the exact level of the subsidy is not a trivial task. Many subsidies, such as gasoline sold to households, are direct, while others, such as gasoline used in transportation, are indirect. Here, we are concerned with direct subsidies only. The calculation of direct subsidies is also complicated by two facts. First, most of the subsidies are implicit, so they do not appear in the budget. World market prices serve to estimate the value of implicit subsidies. Second, except for bread, kerosene, diesel, and LPG, other subsidies are nonlinear. Gasoline is sold at two prices - a rationed and a free price - and tariffs for natural gas and electricity are differentiated by volume. In addition, prices for electricity and natural gas

vary according to the season and a region's climate.

At the start of the reforms in 2010, gasoline had a two-tier price: Rls 1,000 per liter for rationed and Rls 4,000 for free market gasoline. In December these prices were increased to 4,000 and 7,000, respectively. The new free market price was about $0.70 per liter, which was close to its border price, but by 2014, following the 200 percent depreciation of the rial, it had fallen to about $0.25 per liter, well below the border price. The price of diesel, which had the highest subsidy, was initially set to increase 22 times, but was reduced to 9 times following protests by truck drivers. In 2013-14, the price of diesel was raised again, to Rls 3,500 ($0.11) per liter, which was about one-sixth of its border price. Table 3.2 presents the prices of the main energy products and bread in 2013-14 and their respective free market levels.

The prices we use in the calculation of subsidies in this section, as well as in simulations in the next section, are more detailed than appear in table 3.2; in particular, they take into account the nonlinear price structure of energy products in the Islamic Republic of Iran. For example, the effective national average price of natural gas was Rls 742 per cubic meter (Ministry of Energy 2013). In reality, prices started at Rls 300 per m^3 (about $0.01) and increased to Rls 3,500 (about $0.12) for big users. Similarly, the average price of electricity for households was Rls 337 per kWh, and tariffs increased from 300 per kWh to 2,150 for the high-end users. Bread prices are set through government control and subsidy. The government buys domestically produced wheat at Rls 10,150 per kilogram, which is close to international market price. Wheat is then sold at the subsidized price of around Rls 460 to flour producers. In 2013-14 flour sold at Rls 5,900 ($0.20), per kilogram to bakers. Each kilogram of bread was then sold at Rls 10,274.

Using these data from the survey with SUBSIM (SUBsidy SIMulation) enables us to estimate the distribution of subsidies among households. Table 3.3 shows the distribution of per capita expenditures on subsidized goods by deciles of per capita expenditures. Except for bread and kerosene, per capita expenditures on subsidized goods increase sharply with the decile of expenditures. The ratio of expenditures on bread between the richest and poorest deciles is 1.24, compared to 11.1 for gasoline and 3.7 for natural gas (household consumption of diesel is very small, so this ratio is not very informative). The SUBSIM estimates show that the total value of the subsidy paid directly to households (implicit plus explicit subsidies) amounted to Rls 540 trillion per year, or about $18 billion at the market exchange rate (Rls 30,000 = $1.00). This amount is considerably below the $66 billion mentioned at the

beginning of this chapter. That calculation was based on the gap between the total value of energy products consumed in the Islamic Republic of Iran evaluated at world and domestic prices.

Table 3.3: Expenditures per Capita on Subsidized Products, in thousand rials

Expenditure decile	*Kerosene*	*Gasoline*	*Electricity*	*Diesel*[a]	*Bread*[b]	*Naturalgas*[c]	*LPG*	*Total*
1 (poorest)	72.6	166	291.9	0.8	1,100.80	213	121.3	1,966.10
2	112.2	275.8	382.6	3.2	1,182.90	326.1	101.4	2,384.00
3	103	365.3	416.6	0	1,187.50	422.6	92.5	2,587.30
4	119.5	481.2	490	4.5	1,252.00	509.7	87.5	2,944.20
5	125.3	569.6	530.6	3	1,251.90	566	76	3,121.90
6	114.7	681.5	563.8	0.4	1,331.00	661.5	70.8	3,423.20
7	104.9	836.6	643.5	4.8	1,259.70	776.6	65.4	3,691.20
8	98.1	902.6	681.2	12.2	1,309.40	807	54.7	3,865.10
9	67.7	1,199.50	762.6	3.6	1,321.30	942.8	42.1	4,339.20
10 (richest)	100.5	1,843.00	1,147.80	12.2	1,364.30	1,196.20	45.9	5,709.30
Total	101.8	732.2	591.1	4.5	1,256.10	642.2	75.7	3,403.30
Ratio of richest to poorest decile	1.38	11.1	3.93	15.37	1.24	5.62	0.38	2.9

Source: Authors' calculation using HEIS 2013.

Note: a. Household consumption of diesel fuel is small compared to its use in transportation, which is included in the indirect effects.

b. Bread includes flour.

c. Natural gas data included compressed natural gas (CNG) used in cars.

Viewed from the perspective of incidence, the value of subsidies for the poor and the rich is quite different. Defining incidence as the proportion of the subsidies to household expenditures, we can see from table 3.4 that subsidized products matter much more for the poor than for the rich. The poorest decile spends 13.6 percent of its expenditures on subsidized goods compared to 3.7 percent for the richest decile. The poor's dependence on subsidies was greatest for bread, natural gas, and electricity. Households in the poorest decile spent 7.6 percent of their budget on bread compared to less than 1 percent for those in the richest decile. The gasoline subsidy, which is unequally distributed between the poor and the rich, accounts for similar proportions of the budgets of different deciles. As a result, the poor would sooner agree to a price reform for gasoline, which would not affect them much, than bread, which makes up a larger proportion of their budget. But, with compensation, they

would stand to gain from a gasoline price reform.

Table 3.4: Expenditure on Subsidized Products over Total Expenditures, in percent

Expenditure decile	Kerosene	Gasoline	Electricity	Diesel	Bread	Natural gas	LPG	Total
1 (poorest)	0.5	1.1	2	0	7.6	1.5	0.8	13.6
2	0.5	1.3	1.8	0	5.4	1.5	0.5	11
3	0.4	1.3	1.5	0	4.4	1.6	0.3	9.6
4	0.4	1.5	1.5	0	3.9	1.6	0.3	9.1
5	0.3	1.5	1.4	0	3.3	1.5	0.2	8.2
6	0.3	1.5	1.3	0	3	1.5	0.2	7.7
7	0.2	1.6	1.2	0	2.4	1.5	0.1	7
8	0.2	1.4	1.1	0	2	1.3	0.1	6
9	0.1	1.4	0.9	0	1.6	1.1	0.1	5.2
10 (richest)	0.1	1.2	0.7	0	0.9	0.8	0	3.7
Total	0.2	1.4	1.1	0	2.4	1.2	0.1	6.4

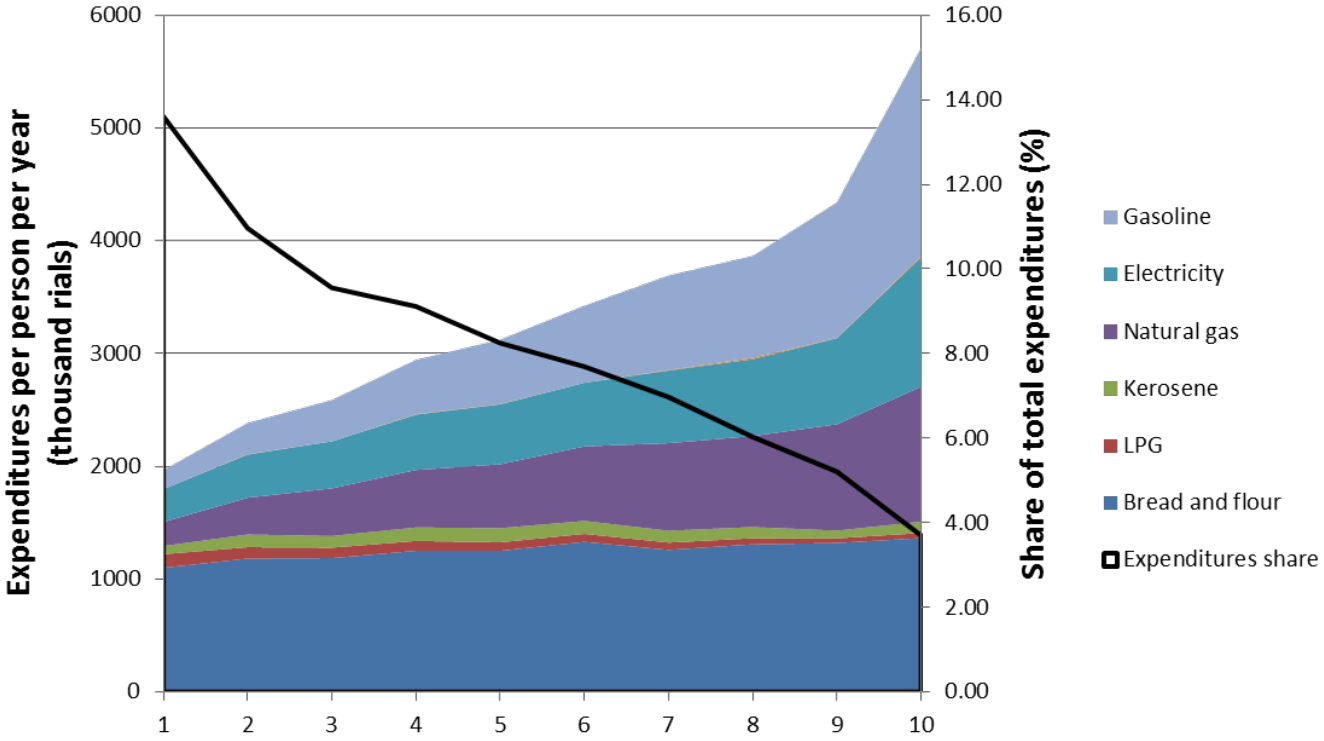
Source: Authors' calculation using HEIS 2013.

Figure 3.4 combines the information in tables 3.3 and 3.4 to depict the main dilemma of subsidy reform. The shaded areas are expenditures per person per year, measured in Rls 1,000, on various energy products and bread (left y-axis). Assuming that the subsidies that directly accrue to households (as distinct from the indirect benefits from lower transportation costs, for example) are proportional to expenditures on these items (which is the case with linear prices), this graph also depicts the distribution of the subsidies. The richest decile spent on average more than Rls 5 million per person (about $584 PPP) per year on these subsidized products, compared to Rls 2 million for the poorest decile (about $234 PPP). In a sense, the gasoline subsidy is the most regressive because the richest decile receives about 15 times as much of it as the poorest decile. By contrast, the bread subsidy is almost uniformly distributed.

The right y-axis captures the main political economy dilemma in subsidy reform. The solid line shows the share of expenditures on subsidized products in total expenditures for each decile of per capita expenditures. As a proportion of total expenditures, the poorest decile spends nearly four times as much on subsidized goods - 13.6 percent compared to 3.7 percent - and therefore stands to lose more if energy prices are increased without compensation. This chart shows that we should expect the direct welfare effects of price reforms to be greater for the poor than the rich. The indirect effects, through higher prices in other goods and services that use energy, are more equally distributed and rise with income. Still, the overall

negative effect on the poor is sufficient to justify some form of social protection, either direct compensation or reliance on the existing social protection mechanisms.

Figure 3.4: Expenditures per Person per Year on Subsidized Goods and Their Share in Total Expenditures in 2013-14, by decile (1,000 rials)



Source: Data from tables 3.3 and 3.4.

3.5 Simulations of Subsidy Reform

This section presents the simulation results of two hypothetical price reforms. Scenario 1, labeled "gradualist," increases the prices of subsidized goods by 10 percent across the board. Scenario 2, "full adjustment," assumes a much larger adjustment, taking all prices to close to their FOB or European levels (for electricity and natural gas) in 2014. Scenario 1 is interesting because it is the choice likely to be implemented. Scenario 2 is not on the agenda at present, but it is useful to consider because it was adopted in 2010 and serves as a comparison for the gradualist scenario.

Since taking office, the Rouhani government has opted for small price adjustments. Following the country's bad experience with full adjustment in 2010, there is a no public support for a large price increase. The sharp decline in the global price of oil in 2014 has also reduced the need or urgency for raising domestic prices of energy. In spring 2014 all prices for subsidized goods were raised by about 30 percent (the bread price increase came in November), except gasoline, which went up by about 50 percent. In spring 2015 prices were again raised, this time by about 15 percent. Both of these increases are less than our gradualist scenario because the 10 percent increase in our scenario is in real terms, and the price adjustments under Rouhani were hardly enough to correct for inflation in the preceding 12 months, which were 34.5 percent in 2013-14 and 15.5 percent in 2014-15. The price increases that would have matched this scenario would have been 44.5 percent in 2014 and 25.5 percent in 2015.

Scenario 2 assumes that global oil prices recover to their average for 2014; that is, it aims for full elimination of subsidies. For bread a 60 percent increase brings its price close to the zero-subsidy level. Bread prices are set by a combination of government control and subsidy. Flour is sold at subsidized prices to bakers, whose prices are monitored. A substantial part of the wheat consumed in the country is imported, which can be considered as opportunity cost. In 2013 the support price set by the government for domestically produced wheat was Rls 10,150 ($0.30) per kilogram, which is close to the world market, so it can be used as the target price. Currently, however, the government sells flour to bakers at Rls 8,490 per kilogram, which would not reach the zero-subsidy level with a 10 percent increase.

Determining the energy prices that would fully eliminate the energy subsidies is difficult. Given the volatility in the global price of oil, it is hard to pinpoint the medium-term opportunity cost of Iranian oil and gas. At $50 a barrel, for example, the FOB price of gasoline in the Islamic Republic of Iran is about the free-market price of gasoline. Scenario 2 assumes that the world oil price returns to the average for 2014, $96.30.

The list of target prices used in both scenarios is presented in table 3.5. For traded commodities, we set the target price at opportunity costs as implied by the average crude price in 2014. For gasoline, diesel, and kerosene, whose global prices declined by nearly 50 percent during 2014, we take the average FOB Persian Gulf level - Rls 21,950 ($0.69) per liter for gasoline, Rls 21,189 ($0.66) for diesel, and Rls 20,869 ($0.65) for kerosene. These average prices would equal opportunity cost if world oil prices were to return to the level prevailing around September 2014.

The price of natural gas varied much less than crude oil during 2014, but has its own complexity because there is no regional market as transparent as the one for gasoline. We set the target price for natural gas at Rls 11,358 per m^3 (about \$0.35), which is less than the export price of Iranian gas to Turkey (about \$0.50), but closer to the export prices charged by Azerbaijan for exports to Turkey. The prices combine compressed natural gas (CNG), used in transportation, with the natural gas supplied to consumers.

There is no regional market of any kind for electricity that would guide the setting of the subsidy-free price. The Islamic Republic of Iran does export some electricity to Iraq, but there is no information on pricing for these exports, and in any case may involve a subsidy of its own due to political considerations. We therefore picked the target price of Rls 2,720 (\$0.09) per kilowatt hour, which is close to the rate in Turkey but below the average in most middle-income developing countries (EIA 2015). This price is close to the prevailing price in Turkey, India, and Brazil.

We use a demand price elasticity of -0.2 to calculate the post-reform consumed quantities of subsidized goods and changes in government subsidy payments.

Table 3.5: Price of Subsidized Items, in rials

	Gasoline (liter)		Diesel (liter)	Kerosene (liter)	Natural gas (m^2)	LPG (m^2)	Electricity (kWh)	Bread (kg)	Flour (kg)
Price in 2013:	Up to 60 liters	More than 60 liters							
Iran	4,000	7,000	3,500	1,000	742[a]	1,800	337.5[b]	10,274	5,900
Scenario 1 (10 percent increase)	4,400	7,700	3,850	1,100	816.42	1,980	371.25	11,301	6,490
Scenario 2 (opportunity cost price)	24,000[a]	24,000[a]	23,000[a]	22,600[a]	11,358[c]	10,800[c]	2,720[d]	20,548[e]	13,900[e]

Source: Ministry of Energy 2013.

Note: a. Based on FOB Persian Gulf price, Platts.com.

b. Effective national average price, Ministry of Energy 2013.

c. Average Europe price, FERC and www.cngeurope.com, 2014.

d. Price in Turkey, Turkish Statistical Institute, 2014.

e. Based on international wheat price and authors' calculations.

3.5.1 Scenario 1: Direct Effects

This section reports the results of the gradualist scenario, increasing prices by 10 percent. We evaluate the impact of this reform on individual welfare and government revenues, starting with the direct effects of price increases on energy and bread. Direct effects measure the losses in welfare as reductions in real expenditures for households in different deciles of per capita expenditures. The model takes into account consumer responses to the price increases for these products, but ignores indirect or secondary effects caused by increases in prices of other goods and services. These secondary effects are considered in the next section, indirect effects.

We present our estimates of the direct effects on well-being in table 3.6 and as proportion of per capita household expenditures in table 3.7. The data show that the largest effect in level and share is due to the increase in the price of bread, an average loss of welfare of Rls 125,600 per person per year and 0.24 percent of expenditures. The second largest average loss is for gasoline at Rls 73,200. The reason bread has a relatively large impact is that the average Iranian spends 67 percent more on bread than gasoline. Expenditures on bread amount to more than one-third of the total expenditures on subsidized goods.

The losses due to the increase in the bread price are more uniformly distributed across deciles of per capita expenditures than other commodities, increasing by 24 percent from the poorest to the richest decile. In the case of gasoline this increase is more than 10 times. The total loss on all items is on average Rls 340,300 per person per year (PPP $39.75), which is less than 1 percent of expenditures. The ratio of the overall loss in the richest to poorest decile is 2.9.

The loss of welfare is better reflected as proportion of household expenditures (table 3.7). Contrary to the picture obtained from levels in table 3.6, the distributional impact of gasoline seems the least unequal and for bread the most unequal. Losses due to price increases for bread, natural gas, and gasoline figure prominently in the poorest decile's budgets, but all are less than 1 percent. The overall impact is small because the share of these products in average per capita expenditures is 6.4 percent, so a 10 percent increase in their price does not have a large impact on the average consumer's budget. Changes in quantities reported in annex table A.2 are also modest, showing average reductions of 7 kilowatt hour per person in electricity and 5 m^3 of natural gas. Given the elasticity assumptions of -0.2, a 10 percent price increase reduces the quantity consumed by 2 percent.

Table 3.6: Direct Effects of the Gradualist Scenario on per Capita Well-Being (thousand rials)

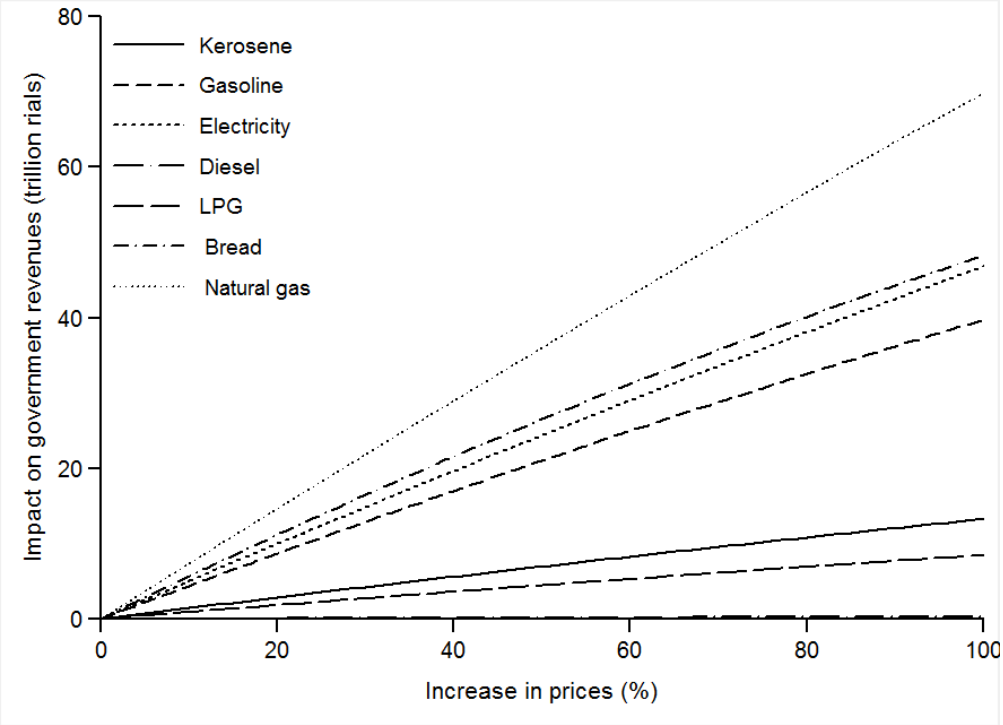
Expenditure decile	Kerosene	Gasoline	Electricity	Diesel	Bread	Natural gas	LPG	Total
1 (poorest)	-7.3	-16.6	-29.2	-0.1	-110.0	-21.3	-12.1	-196.6
2	-11.2	-27.6	-38.3	-0.3	-118.2	-32.6	-10.1	-238.4
3	-10.3	-36.5	-41.7	0	-118.7	-42.3	-9.2	-258.7
4	-11.9	-48.1	-49.0	-0.4	-125.2	-51.0	-8.7	-294.4
5	-12.5	-57.0	-53.1	-0.3	-125.1	-56.6	-7.6	-312.1
6	-11.5	-68.2	-56.4	0.0	-133.0	-66.1	-7.1	-342.3
7	-10.5	-83.7	-64.3	-0.5	-125.9	-77.7	-6.5	-369.1
8	-9.8	-90.3	-68.1	-1.2	-130.9	-80.7	-5.5	-386.5
9	-6.8	-120.0	-76.3	-0.4	-132.1	-94.3	-4.2	-433.9
10 (richest)	-10.1	-184.3	-114.8	-1.2	-136.4	-119.6	-4.6	-570.9
Total	-10.2	-73.2	-59.1	-0.4	-125.6	-64.2	-7.6	-340.3

Table 3.7: Direct Effects of Gradualist Scenario on Well-Being, in percentage of household expenditures

Expenditure decile	Kerosene	Gasoline	Electricity	Diesel	Bread	Natural gas	LPG	Total
1 (poorest)	-0.05	-0.11	-0.20	-0.00	-0.76	-0.15	-0.08	-1.36
2	-0.05	-0.13	-0.18	-0.00	-0.54	-0.15	-0.05	-1.10
3	-0.04	-0.13	-0.15	-0.00	-0.44	-0.16	-0.03	-0.96
4	-0.04	-0.15	-0.15	-0.00	-0.39	-0.16	-0.03	-0.91
5	-0.03	-0.15	-0.14	-0.00	-0.33	-0.15	-0.02	-0.82
6	-0.03	-0.15	-0.13	-0.00	-0.30	-0.15	-0.02	-0.77
7	-0.02	-0.16	-0.12	-0.00	-0.24	-0.15	-0.01	-0.70
8	-0.02	-0.14	-0.11	-0.00	-0.20	-0.13	-0.01	-0.60
9	-0.01	-0.14	-0.09	-0.00	-0.16	-0.11	-0.01	-0.52
10 (richest)	-0.01	-0.12	-0.07	-0.00	-0.09	-0.08	-0.00	-0.37
Total	-0.02	-0.14	-0.11	-0.00	-0.24	-0.12	-0.01	-0.64

The sensitivity of the change in government revenue to the size of the price increase of individual subsidized goods is shown in figure 3.5. Government revenue is most sensitive to the size of increase in the prices of bread, natural gas, and gasoline. For example, a 100 percent increase in the price of bread increases government revenues by Rls 100 trillion (PPP $11.7 billion, or 5 percent of total government revenues), compared to Rls 80 trillion for natural gas and Rls 75 trillion for gasoline. In the present scenario, the total amount of subsidies paid out declines from Rls 484 trillion (PPP $56.5 billion) to Rls 447 trillion ($52.2 billion), a savings of Rls 37 trillion ($4 billion) for the government.

Figure 3.5: Price Changes and the Impact on Government Revenue



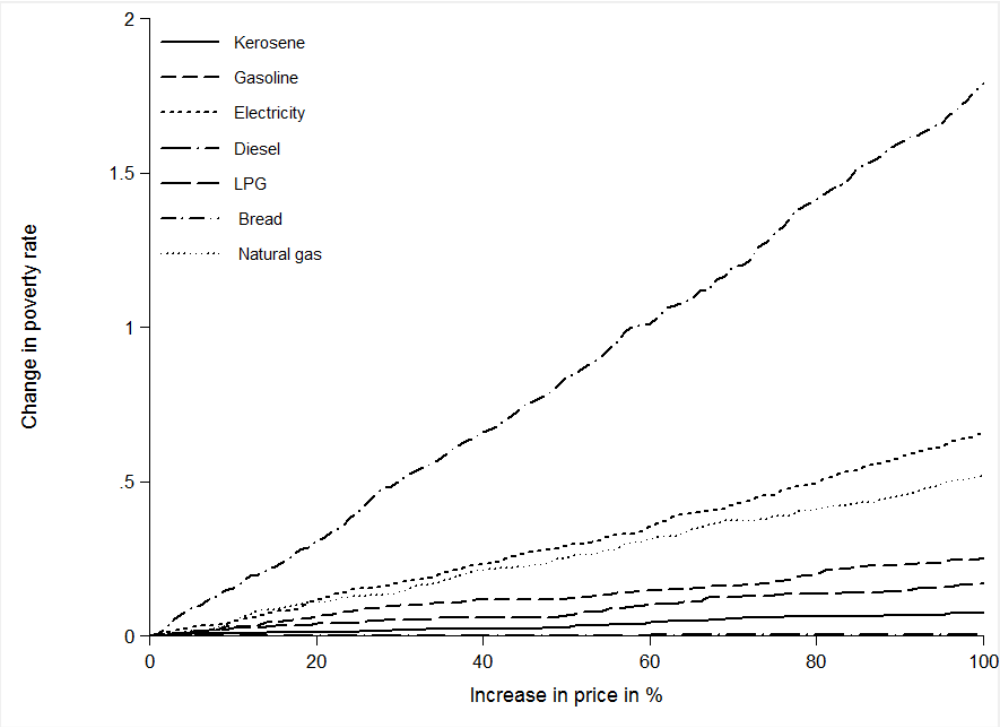
Source: Authors' calculation using SUBSIM and HEIS 2013.

We now turn to the impact of the gradualist reform scenario on poverty and inequality. We measure the poverty rate using the poverty lines of Rls 18 million per person per year in urban areas and Rls 12 million in rural areas. Implementing the gradualist price reforms increases the poverty rate from 4.95 percent to 5.30 percent and the poverty gap from 0.98 percent to 1.04 percent. Inequality, as measured by the Gini index, increases slightly from 37.36 to 37.49. These small changes are not surprising given the small price adjustment envisioned in the gradualist scenario.

How sensitive are these changes in poverty to the size of the price increase? Figure 3.6 shows the sensitivity of the poverty rate to the size of price increases by commodity. Again, from the point of view of increase in poverty, bread is the most important commodity; a 60 percent increase in its price increases the head-count ratio by 1 percentage point. Energy products have much smaller impacts.

If the government wishes to keep the poverty rate from increasing, it must offer compensation.

Figure 3.6: Percentage Change in the Poverty Rate by the Size of Price Increases



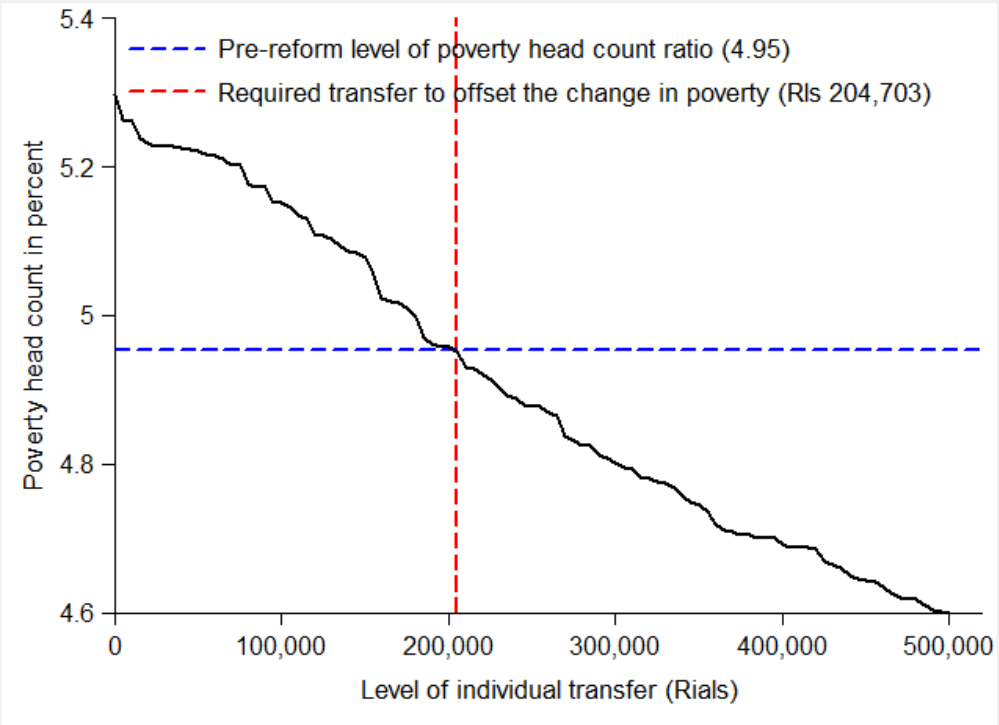
Source: Authors' calculation using SUBSIM and HEIS 2013.

Figure 3.7 estimates the effect of universal and uniform transfers on the poverty rate. To prevent the poverty rate from increasing as a result of the direct effects of the 10 percent price adjustment, the government needs to pay each person Rls 204,703 per year ($23.40), which is less than 4 percent of the current level of transfer). Doubling this amount reduces the poverty rate by 0.35 percentage points.

3.5.2 Scenario 1: Indirect Effects

The indirect effects are the secondary effects on the consumer budget that result from the increase in prices of energy-using sectors. SUBSIM uses an input/output table to take these secondary effects into account. The quality of the indirect estimates depends crucially on having an up-to-date I/O table. The latest I/O table for the Islamic Republic of Iran is from 2001, when energy prices were very low. SUBSIM uses the rial values of intersector flows as input. We update the rial values of the I/O table to 2013 using the consumer price index

Figure 3.7: Impact of the Level of Transfer to Compensate Indirect Effects on Poverty in the Gradualist Scenario



Source: Source: Authors' calculation using SUBSIM and HEIS 2013.
Note: Only direct effects of the reform on well-being are considered.

(CPI). This calculation underestimates the dependence of other sectors on energy products because energy prices rose by a larger factor than the CPI during 2001-13. The CPI rose by a factor of 7, and energy prices rose by factors ranging from 10 to 20.

The country's I/O table does not show individual prices for subsidized products; instead, it combines diesel, gasoline, and kerosene into one group. We include electricity and natural gas as separate items. As with direct effects, we raise the price of the group and individual items by 10 percent in real terms.

In Table 3.8 we add the indirect and direct effects to get a more comprehensive picture of the impact on well-being of the gradualist scenario. These results update the direct estimates of impact shown in tables 3.6 and 3.7 (column 1 reproduces the totals column in table 3.6, and column 4 reproduces the totals column in table 3.7). Looking at per capita losses, we note that except for the tenth decile, indirect effects are smaller than the direct effects, but they

are less equally distributed. The ratio of the loss suffered by the richest to the poorest decile is 2.9 compared to 5.2. Losses as proportion of household expenditures, shown on the right side, indicate that direct effects, measured relative to household expenditures, are smaller than direct effects and their distribution is more equal. Including the indirect effects does not change our assessment of the impact of the price increase on poverty and inequality by much (table 3.9).

Table 3.8: Direct and Indirect Effect of the Gradualist Scenario on Household Welfare

	Per capita, in rials			Percent of total expenditures		
Expenditure decile	Direct effects	Indirect effects	Total	Direct effects	Indirect effects	Total
1 (poorest)	-196.6	-121.3	-317.9	-1.36	-0.77	-2.13
2	-238.4	-169.9	-408.4	-1.10	-0.71	-1.81
3	-258.7	-180.3	-439.0	-0.96	-0.6	-1.56
4	-294.4	-207.5	-501.9	-0.91	-0.58	-1.49
5	-312.1	-234.6	-546.7	-0.82	-0.56	-1.38
6	-342.3	-251.0	-593.3	-0.77	-0.5	-1.27
7	-369.1	-341.8	-710.9	-0.70	-0.58	-1.28
8	-386.5	-333.2	-719.7	-0.60	-0.46	-1.06
9	-433.9	-386.3	-820.2	-0.52	-0.41	-0.93
10 (richest)	-570.9	-631.9	-1202.8	-0.37	-0.37	-0.74
Total	-340.3	-292.6	-632.9	-0.64	-0.48	-1.12

Source: Authors' calculation using SUBSIM and HEIS 2013.

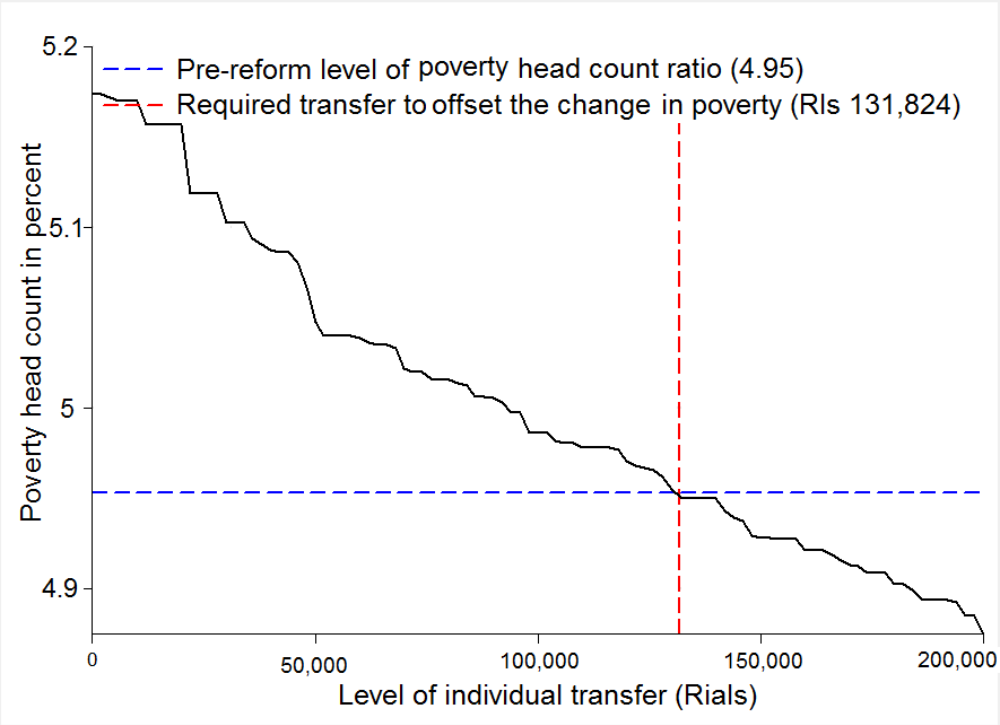
Table 3.9: Direct and Indirect Impacts of Gradualist Subsidy Reform on Poverty and Inequality

	Pre-reform	Post-reform
Change in per capita expenditures (thousand rials)		-632.91
Poverty head count (percent)	4.95	5.48
Poverty gap (percent)	0.98	1.072
Gini (percent)	37.36	37.55

Source: Authors' calculation using SUBSIM and HEIS 2013.

As before, we calculate the required transfer to prevent an increase in poverty. To compensate for the indirect effect so that poverty rate remains at 4.95 percent, the government needs to pay Rls 131,824 per person per year (figure 3.8), compared to Rls 204,703 for the direct effects (figure 3.7).

Figure 3.8: Impact of the Level of Transfer to Compensate Indirect Effects on Poverty in the Gradualist Scenario



Source: Source: Authors' calculation using SUBSIM and HEIS 2013.
Note: Only indirect effects of the reform on well-being are considered.

3.5.3 Scenario 2: Direct Effects

In the full adjustment scenario we increase prices according to the values in table 3.5 by factors ranging from 2 for bread to 20 for kerosene. We use the Cobb-Douglas routine of SUBSIM because the marginal approach is much less accurate for large price changes. We present the results for this scenario first for the direct effects followed by the indirect effects.

As expected, the impact on household welfare in this scenario is much larger than the gradualist scenario. Looking at the impact as a percentage of per capita expenditures (tables 3.10 and 3.11), we note that the average impact is 11.46 percent compared to 0.64 percent in the gradualist scenario, higher by a factor of 17 (compared to a higher average price increase of 7 times). The loss for the poorest decile increased from 1.36 percent in the gradualist scenario to 24.06 percent in full adjustment. The richest decile's loss increased from to 0.37

to 6.61 percent, which is similar to the change in impact for the poor.

Table 3.10: Direct Effects of the Full-Adjustment Scenario on per Capita Well-Being, (thousand rials)

Expenditure decile	Kerosene	Gasoline	Electricity	Diesel	Bread and flour	Natural gas	LPG	Total
1 (poorest)	-432.4	-390.8	-210.2	-4.0	-1,169.7	-664.0	-606.5	-3,477.5
2	-668.3	-640.8	-260.8	-16.4	-1,236.3	-1,005.3	-507.0	-4,334.9
3	-613.7	-840.2	-282.4	-0.2	-1,227.2	-1,255.2	-462.4	-4,681.3
4	-711.5	-1,094.5	-316.6	-22.7	-1,286.8	-1,481.4	-437.4	-5,351.1
5	-746.4	-1,292.6	-337.1	-15.0	-1,283.6	-1,607.8	-380.2	-5,662.7
6	-683.0	-1,542.0	-353.5	-2.2	-1,355.2	-1,850.8	-353.8	-6,140.6
7	-624.6	-1,885.7	-387.0	-24.2	-1,283.0	-2,114.1	-326.8	-6,645.5
8	-584.6	-2,030.8	-404.1	-61.7	-1,327.3	-2,191.2	-273.5	-6,873.2
9	-403.3	-2,679.6	-451.0	-18.4	-1,335.9	-2,574.4	-210.3	-7,672.9
10 (richest)	-598.7	-4,075.1	-596.4	-61.5	-1,375.7	-3,274.2	-229.7	-10,211.2
Total	-606.6	-1,647.3	-359.9	-22.6	-1,288.1	-1,801.9	-378.7	-6,105.3

Source: Authors' calculation using SUBSIM and HEIS 2013.

Table 3.11: Direct Effects of Full Adjustment Scenario on Well-Being, in percentage of household expenditures

Expenditure decile	Kerosene	Gasoline	Electricity	Diesel	Bread and flour	Natural gas	LPG	Total
1 (poorest)	-2.99	-2.70	-1.45	-0.03	-8.09	-4.59	-4.20	-24.06
2	-3.07	-2.95	-1.20	-0.08	-5.68	-4.62	-2.33	-19.93
3	-2.27	-3.10	-1.04	-0.00	-4.53	-4.63	-1.71	-17.28
4	-2.20	-3.39	-0.98	-0.07	-3.98	-4.59	-1.35	-16.56
5	-1.97	-3.41	-0.89	-0.04	-3.39	-4.25	-1.00	-14.96
6	-1.53	-3.46	-0.79	-0.00	-3.04	-4.16	-0.79	-13.79
7	-1.18	-3.56	-0.73	-0.05	-2.42	-3.99	-0.62	-12.55
8	-0.91	-3.17	-0.63	-0.10	-2.07	-3.42	-0.43	-10.72
9	-0.48	-3.21	-0.54	-0.02	-1.60	-3.09	-0.25	-9.21
10 (richest)	-0.39	-2.64	-0.39	-0.04	-0.89	-2.12	-0.15	-6.61
Total	-1.14	-3.09	-0.68	-0.04	-2.42	-3.38	-0.71	-11.46

Source: Authors' calculation using SUBSIM and HEIS 2013.

In contrast to the gradualist scenario, we see a significant quantity adjustment in this case (table 3.12). Average electricity consumption declines by 105.78 kilowatt hours (a decline of 30 percent in consumption), and natural gas by 161.77, which is a decline of less than one-forth. The natural gas consumption by the poorest decile is estimated to decline by about 78 percent, which is unrealistic, and the result of assuming a fixed elasticity for all levels of

consumption and income. In this scenario bread continues to have the largest impact on the welfare of the poor, followed by natural gas and kerosene.

Naturally, the impact of full adjustment on poverty and inequality are larger (table 3.13). The poverty rate increases to 11.59 percent, more than doubling, and the poverty gap more than triples, 0.98 percent compared to 3.91 percent. The Gini index increases from 37.36 to 40.70. The Gini index changes because the reform impact is different for each decile. The poor are affected more by the program relative to their total expenditures compared to the rich (see table 3.11). Note that this impact is before any cash transfer is paid to individuals.

The cash transfer necessary to keep the poverty rate from increasing is estimated at Rls 4.4 million per person per year, 20 times higher than in the gradualist scenario (see figure 3.9). However, the savings of the government outweigh this amount of transfer by Rls 139 trillion (PPP $16 billion), which is a substantial amount (about 9 percent of total government revenues).

Table 3.12: Impact on the per Capita Consumed Quantities in the Full Adjustment Scenario, direct effects

Expenditure decile	Kerosene (Liter)	Gasoline (Liter)	Electricity (kWh)	Diesel (Liter)	Bread and flour (Kilogram)	Natural gas (m^3)	LPG (m^3)
1 (poorest)	-24.20	-12.65	-66.91	-0.23	-26.56	-61.85	-67.39
2	-37.40	-20.43	-81.01	-0.93	-27.00	-94.15	-56.33
3	-34.35	-26.46	-87.46	-0.01	-26.07	-116.68	-51.37
4	-39.82	-34.00	-95.88	-1.29	-26.96	-135.76	-48.61
5	-41.77	-40.05	-101.22	-0.85	-26.73	-146.86	-42.24
6	-38.22	-47.59	-105.48	-0.12	-27.72	-166.70	-39.31
7	-34.96	-57.92	-113.00	-1.37	-26.26	-187.02	-36.31
8	-32.72	-62.23	-117.07	-3.49	-26.82	-195.06	-30.39
9	-22.57	-81.37	-130.44	-1.04	-26.81	-228.62	-23.37
10 (richest)	-33.51	-122.11	-159.34	-3.48	-27.40	-284.93	-25.52
Total	-33.95	-50.48	-105.78	-1.28	-26.83	-161.77	-42.08

Source: Authors' calculation using SUBSIM and HEIS 2013.

3.5.4 Scenario 2: Indirect Effects

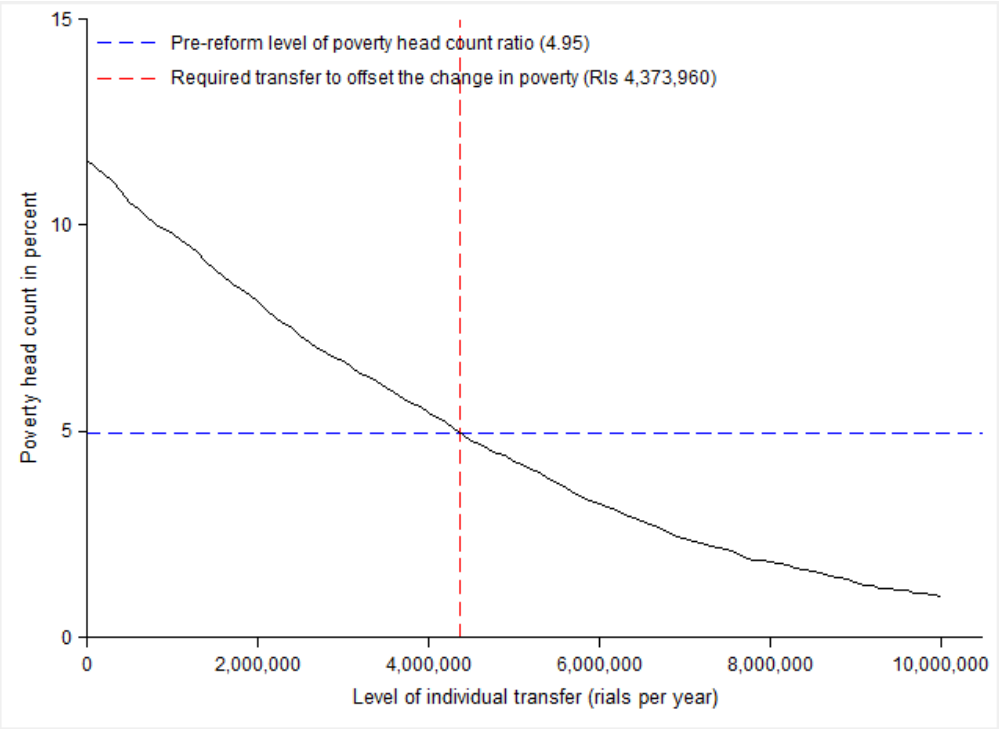
To implement the price changes according to this scenario we need to find the average price increase for energy products that appear in one group in the I/O table. We use a weighted average of increases for prices of gasoline, diesel, and kerosene, which comes to 600 percent.

Table 3.13: Direct Impacts of Full-Adjustment Subsidy Reform on Poverty, Inequality, and Government Budget

	Pre-reform	Post-reform
Change in per capita expenditures (Rls thousand)		-6,105.34
Poverty head count (percent)	4.95	11.59
Poverty gap (percent)	0.98	3.91
Inequality (percent)	37.36	40.7
Subsidies (Rls trillion)	491.41	0
Transfers (Rls trillion)a	0	352.06
Change in total budget(Rls trillion)		-139.35

Source: Authors' calculation using SUBSIM and HEIS 2013.

Figure 3.9: Impact of the Level of Transfer to Compensate Indirect Effects on Poverty in the Full Adjustment Scenario



Source: Source: Authors' calculation using SUBSIM and HEIS 2013.
Note: Only direct effects of the reform on well-being are considered.

For individual commodities, we assume a 200 percent increase for natural gas, 100 percent for bread, and 700 percent for electricity.

The results are presented in table 3.14. In contrast to the gradualist scenario, for richer deciles the indirect effects are larger than direct effects, though on average the effects of the two types are similar in size. The additional transfer required to maintain the poverty rate at pre-reform level of 4.95 percent is Rls 3.2 million per person per year (figure 3.10). Thus, the total required compensation for both the direct and indirect effects is Rls 7.5 million (PPP $876), which is about 40 percent larger than the current level of compensation. However, if we compare the same amount paid in 2011, the first year of the 2010 reform, with the estimated compensation here, we learn that the Ahmadinejad compensation plan exceeded what was necessary to keep poverty constant, by some 70 percent.

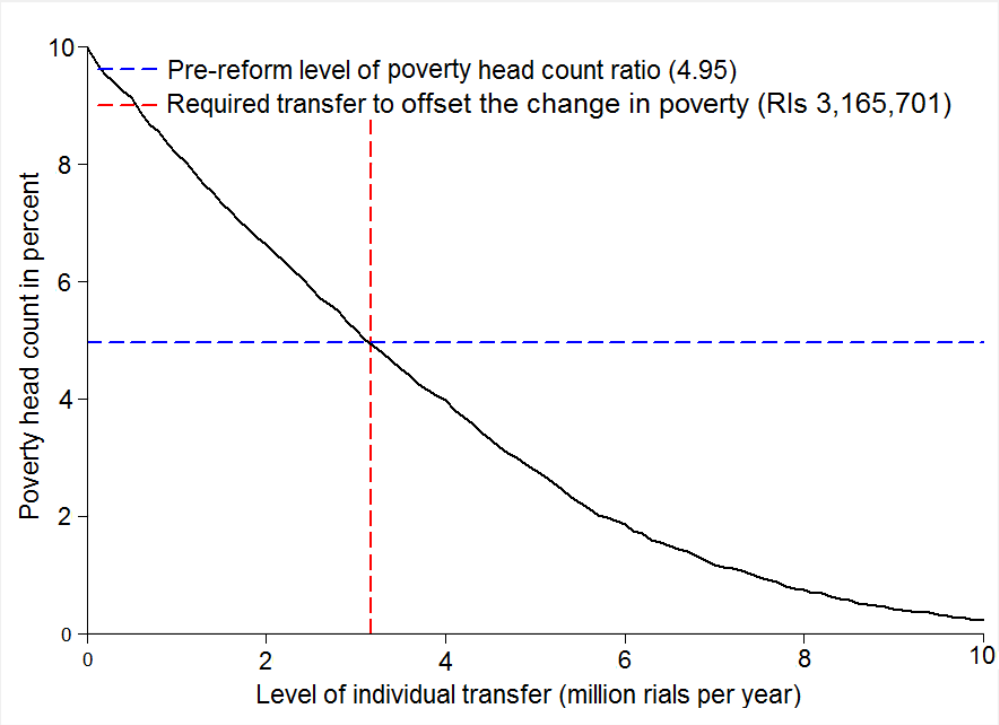
Table 3.14: Direct and Indirect Effects of Price Increases on Well-Being in the Full Adjustment Scenario

	Per capita, thousand rials			Percent of total expenditures		
Expenditure decile	Direct effects	Indirect effects	Total	Direct effects	Indirect effects	Total
1 (poorest)	-3477.5	-2631.0	-6108.5	-24.1	-18.2	-42.3
2	-4334.9	-3702.1	-8037.0	-19.9	-17.0	-37.0
3	-4681.3	-4372.0	-9053.3	-17.3	-16.1	-33.4
4	-5351.1	-4868.6	-10219.7	-16.6	-15.1	-31.6
5	-5662.7	-5626.8	-11289.5	-15.0	-14.9	-29.8
6	-6140.6	-6284.0	-12424.6	-13.8	-14.1	-27.9
7	-6645.5	-7182.9	-13828.4	-12.6	-13.6	-26.1
8	-6873.2	-8411.0	-15284.2	-10.7	-13.1	-23.8
9	-7672.9	-10318.9	-17991.8	-9.2	-12.4	-21.6
10 (richest)	-10211.2	-16333.4	-26544.6	-6.6	-10.6	-17.2
Total	-6105.3	-6973.4	-13078.7	-11.5	-13.1	-24.6

Source: Authors' calculation using SUBSIM and HEIS 2013.

The overall impact on poverty and inequality is reported in table 3.15. As a result of full adjustment, assuming no compensation, the head count ratio jumps fourfold, increasing from 4.95 percent to 20.12 percent, and the poverty gap increases seven fold, from 0.98 percent to 7.31 percent. The Gini index increases by 5.05 points, which is large and shows that price increases for all the items considered here have a greater effect on the poor than on the rich. Removing subsidies has a large adverse impact on inequality because, as shown in table 3.4, the poor spend a larger proportion of their income on subsidized goods. The share of the expenditures on all subsidized goods to total expenditures is 13.6 percent for the poorest decile and 3.7 percent for the richest decile. The highest disparity is for bread which, in 2013 accounted for 7.6 percent of the poorest decile expenditures compared to 0.9

Figure 3.10: Impact of the Level of Transfer to Compensate Indirect Effects on Poverty in the Full Adjustment Scenario



Source: Authors' calculation using SUBSIM and HEIS 2013.
Note: Indirect effects of the reform on wellbeing are considered only. The value of 1.00e+ is 10,000,000.

percent for the richest decile. The next least equally distributed expenditure shares are for electricity, and here the share for the poorest decile is three time higher than for the richest decile. Naturally, any increase in price that is not moderated by a significant decrease in consumption will have a much larger impact on the poor than on the rich, thus increasing the inequality.

It appears that the indirect effects are as important in increasing inequality as the direct effects. The change in the Gini coefficient as a result of the direct effects of removing the subsidies (in scenario 2) is from 37.36 to 40.70, which a about half of the change in Gini with the indirect effects added. This result suggests that half of the adverse impact of the removal of subsidies on inequality comes from the indirect effects.

Table 3.15: Total Impact of Price Increases on expenditures, Poverty and Inequality in the Full Adjustment Scenario

	Pre-reform	Post-reform
Change in per capita expenditure (Rls thousand)		-13,078.73
Poverty head count (percent)	4.95	20.12
Poverty gap (percent)	0.98	7.31
Gini (percent)	37.36	42.41

Source: Authors' calculation using SUBSIM and HEIS 2013.

3.6 The Political Economy of Reforms

The most important political economy aspect of subsidy reform in the Islamic Republic of Iran is that much of the subsidies are government forgone earnings rather than cash expenditures. The government delivers daily about 4 million equivalent barrels of oil and gas, about three times as much as it currently exports, to domestic consumers, enterprises, and power companies at very low prices.

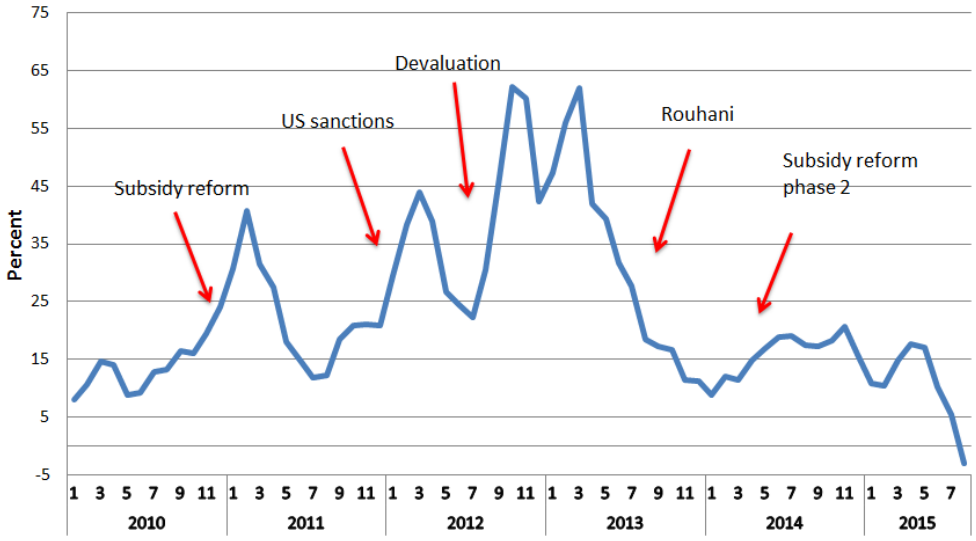
When oil prices are high the government is flush with revenues and does not feel the need to raise domestic prices of energy in tandem with global prices. When the world price of oil is down, government revenues and household incomes are also down, and that is the worst possible time to raise domestic energy prices. Given such price fluctuations, divergence between local and world prices of energy seems a natural part of the country's political economy.

Another political economy reason that energy subsidies are endemic in the Islamic Republic of Iran (and in other oil-rich countries) is that although energy subsidies are unevenly distributed, with most of it going to higher-income brackets, removing them hurts the poor more than rich. As shown in figure 3.4, as a share of household expenditures subsidies are larger for the poor than the rich. Moreover, the credibility of Iranian governments to remove energy subsidies and promise to spend the proceeds more equitably and efficiently is low, which explains why the large price reforms of 2010 had to include a generous cash transfer program.

The unhappy history of energy price reform since 2010 also complicates the political economy of further energy price reform. Since 2010, for reasons unrelated to subsidy reform - sanctions and mismanagement of the economy - Iranians have experienced four years of stagnation and

inflation, making them apprehensive of any new government-initiated price reform. A good part of the inflation in the four years following the reform had little to do with energy and bread price increases, but the Iranian media and public opinion believe otherwise. One contributor to inflation was that cash transfers were too generous and as a result the program was not fully funded. The government filled that gap with borrowing from the Central Bank, which fueled inflation. Another contributor to inflation was the low-cost housing Maskan Mehr program. According to the government, 40 percent of the monetary base was created to cover the deficit in this program. In addition to social spending, the country suffered sizable supply shock during 2011-13, as international sanctions tightened and disrupted its oil sale and general trade. As figure 3.11 shows, monthly rates of inflation decreased a few months after the reform but jumped back up with sanctions and devaluation. The much smaller price hikes in 2014, which were not followed up by other shocks, raised the rate of inflation for a few months before declining.

Figure 3.11: Rates of Inflation and Macroeconomic Shocks from January 2010 to September 2014, 3-month moving averages with annualized rates



Source: Central Bank of Iran, various years and authors calculations.

An important solution to the political economy of reform has been the cash transfer scheme that started in December 2010. Unfortunately, it has come under criticism so that it may not be part of any future reform. There have been claims of negative effects of cash transfers

on the incentives of the poor to work. Although the evidence does not support such claims, anecdotes of poor agricultural workers abandoning their farms continue to appear in the Iranian media (Salehi-Isfahani and Mostafavi-Dehzooei (2014)). The cash transfer program has also been criticized for its unsound targeting because even the richest Iranians receive cash transfers every month. Several attempts have been made to limit cash transfers to poor families only. The 2014-15 budget law required the government to find a way to exclude the richest families from the transfer scheme, but so far the government has avoided the issue because it lacks the necessary mechanism to identify high-income families.

Despite setbacks in public support for the continuation of subsidy reform, the government has strong motivation to raise energy prices and replace lost revenues from oil exports with revenues from the domestic sale of energy. The proposed budget for fiscal 2015/16 projects revenues from oil exports to fall by 24 percent in real terms, forcing the government to cut real current expenditures by 3.3 percent. The increased motivation for raising energy prices is, however, tempered by at least two factors. First, the government itself is very apprehensive of rekindling high inflation. Second, its willingness to raise the price of domestic energy is closely related to the outcome of the current nuclear negotiations, which affect the level of oil exports, and the need for more revenues from other sources. Following the July 14, 2015, nuclear accord between Iran and the six world powers, international sanctions against Iran are expected to be gradually lifted, allowing Iran to export more oil. But this may not be enough to close the budget gap if oil prices continue to remain in the low $50 range per barrel. There is considerable uncertainty regarding the future of oil prices, which suggests that budgetary pressures to raise domestic energy prices could continue for the next several years. Furthermore, the pro-market Rouhani government has already demonstrated its willingness to raise energy prices to market levels, so we should expect further adjustments in energy prices in the near future.

3.7 Conclusions

Despite the significant reform of subsidies in 2010, the Islamic Republic of Iran still subsidizes energy. The public debate over energy subsidies is lively and largely negative, often emphasizing how reform leads to inflation and stagnation. Given the large role that this public debate plays in the internal politics of the country, especially the parliamentary election in March 2016, knowledge of how energy price reform affects household welfare is key to the

future of energy price reform in Iran. In this chapter we evaluate the impacts on household welfare, poverty, and inequality, for two reform scenarios, gradualist and full adjustment. There are important lessons to be learned from each exercise.

A simple analysis of household budgets using the country's 2013 household survey shows that although the benefits of the subsidies generally accrue to richer families, they make up a larger proportion of the income of the poor. This result implies that reform without compensation hurts the poor more than the rich and is likely to face serious opposition. Households in the poorest decile on average spend 13.6 percent of their expenditures on subsidized items, compared to 3.7 percent for the richest decile.

We then incorporate the same survey data into the SUBSIM model to simulate the direct and indirect effect of energy price increases on household welfare. Several interesting policy implications emerge. First, we find that a gradualist approach to energy price reform, even without compensation, does not increase poverty or inequality significantly. The baseline poverty rate of about 5 percent (using a $5 PPP per day poverty line) increases by less than one percentage point as a result of a 10 percent increase in bread and energy prices. The Gini index increases by about 0.2 Gini points. The price increase simulated in this scenario is larger than what the Rouhani government has managed to push through since March 2014. These price increases have barely adjusted energy prices in real terms. So, our simulations indicate that even without compensation, a larger increase that reduces the subsidies in real terms will not cause a significant increase in poverty or inequality.

To keep poverty from increasing, we estimate that about half the savings from price reforms is needed as transfers back to all households. The rest would be added to government revenues, raising them by 0.86 percent. An additional benefit of this scheme is a reduction in inequality of 0.1 Gini points compared to the no-reform case. The necessary amount paid per person is about Rls 28,000 per month, which is quite modest compared to the Rls 445,000 per person per month distributed now. According to this scenario, price increases of 10 percent in real terms (above the rate of inflation) could include modest compensation that insulates the poor and makes further price increases politically easier to implement.

We also simulated the results of a larger one-time adjustment in bread and energy prices that would completely eliminate subsidies. This scenario, which is similar to the price hikes of 2010, serves as a comparison for the gradual case. Without compensation, price reforms have a large effect on the poverty rate, which rose fourfold from 4.95 percent to 20.12 percent. This

is important to know in view of the widespread criticism of the 2010 cash transfer program. Without it, from a social and political point of view, the price reform would not have been possible. To keep poverty from increasing under this scenario, the necessary monthly transfer is Rls 629,000, which is 29 percent less than the current value of the cash transfer paid in December 2010 (about Rls 875,000). Critics of the implementation of the 2010 cash transfer program have pointed out that the amount paid at the time was too generous and was more than the program's earnings. The financing of the deficit contributed to inflation and thereby undermined the energy price reform (Salehi-Isfahani 2013). Under this scenario, the government actually ends up with more revenues, about 5.9 percent more, and inequality drops by 1.2 percent Gini points compared to the no-reform case.

Finally, our simulations provide evidence of the relative sizes of the direct and indirect effects. The indirect impact on welfare, through energy used in the production of other goods and services, appears quite significant, about 13.1 percent of total expenditures compared to 11.5 percent for the direct effect. For the poor the direct impact is higher, whereas for higher expenditure groups it is the indirect effect that dominates.

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