The Intergenerational Effects of Economic Sanctions*

Safoura Moeeni†

Abstract

Economic sanctions have become the defining foreign policy tool of the 21st century. A large literature has documented the negative effects of sanction on current generations, but these effects could be even more detrimental and long-lasting for future generations. This paper quantifies the welfare effects of the United Nations sanctions imposed on Iran in 2006. Exploiting variation in the strength of sanctions across industries and using unique survey data, I obtain two main findings. First, the sanctions decreased children's total years of schooling by 0.2 years and the probability of attending college by 8.7 percentage points. Second, households reduced expenditure on children's education by 61% - particularly on expenditure for school tuition. Taken together the results imply that sanctions have a larger effect on the income of children than their parents. Therefore, ignoring the effects of sanctions on future generations significantly understates their total economic costs.

Keywords: Education; Parental investment; Economic sanctions; Intergenerational effects.
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†Department of Economics, University of Calgary, 2500 University Drive NW, Calgary, Alberta, Canada, E-mail: smoeeni@ucalgary.ca

1 Introduction

Economic sanctions have become the defining foreign policy tool of the 21st century, sometimes as a prelude to warfare, and sometimes as an alternative to it.[1] While humanitarian impacts often feature prominently in the debate about economic sanctions, traditional estimates of the effects of sanctions have mainly focused on the effectiveness of sanctions in achieving political objectives (Hufbauer et al. (2010)). More recent literature has investigated the adverse consequences of sanctions on the civilian population while sanctions are in place (Petrescu (2016)). However, as the effects of sanctions may last beyond the lifting of sanctions, effects on current generation may not fully capture the negative impacts of sanctions. In particular, if sanctions reduce the educational attainment of young people, the effects of sanctions may last long after they are lifted. As early human capital investment is hard to substitute with the investment in later life (Heckman (2011)), sanctions could put children at a disadvantage for the rest of their lives. Moreover, human capital is an important factor in productivity growth and economic development. These negative externalities caused by disinvestment in human capital are not documented in the current literature of adverse consequences of economic sanctions. In this paper, I study welfare effects of economic sanctions, in particular I evaluate how targeted sanctions affect investment in children's education by using Iranian data.

The theoretical effect of sanctions on children's education is ambiguous. Sanctions significantly reduced household income, which is the major source of education funding in Iran.[2] How income matters for children's education is a hotly debated issue. On the one hand, a rich theoretical literature following Becker and Tomes (1986) argues that parental resources may affect educational decisions through budget and credit constraints because education is a consumption good, not only an investment. On the other hand, another influential literature following Cameron and Heckman (2001) argues that parental investment in children's human capital needs not be related to parental income. One possible reason for this disagreement is that temporary and persistent, small and large changes in household income may have different effects on children's education. Households are more likely to reoptimize the consumption in response to large and persistent shocks. Therefore, a large and persistent reduction in household income would be expected to affect children's education, whereas a small and temporary reduction in household income will not necessarily affect children's education. As Browning and Crossley (2009) suggests, households who are temporarily constrained (if they are unable to smooth through borrowing) will cut back more on goods that exhibit high

[1] Economic sanctions are trade and financial restrictions imposed against a targeted country by one or more countries. Sanctions are designed to pressure the targeted countries to change offending policies, and/or to weaken the ability of them to govern (Askari et al. (2001)). For the first time, the United Nations (UN) applied multistate sanctions to Southern Rhodesia in 1991. Since that date, the Security Council has established 25 sanctions regimes, in South Africa, the former Yugoslavia, Haiti, Al-Qaida and the Taliban, Iraq, Iran, etc. Today, there are 14 ongoing sanctions regimes which focus on supporting the political settlement of conflicts, nuclear non-proliferation, and counter-terrorism.

[2] Household expenditure on education as a percentage of GDP is 5% and government expenditure on education is 4% of GDP in 2006. Moreover, like most Middle Eastern countries, a large share of Iranian government spending on education is allocated to post-secondary education in large urban areas. The main reason for this allocation is that governments are very sensitive to demands of the urban middle class, and college education is very important for this group (Richards and Waterbury (1996)).

intertemporal substitution, e.g., luxuries because the utility cost of fluctuations would be lower. Thus, parents can invest in their children's education by reducing other expenditures, selling assets, or raising their own working effort. However, a persistent reduction in household income hampers their ability to consumption smoothing, especially when the shock increased uncertainty about future income (Stephens Jr (2001)). Moreover, the same shock can have different effects on households consumption depend on households' characteristics including budget constraints, adjustment costs, and their preferences.[3] Even when parental spending on children's education reduces, much of which may be offset by financial aid, e.g. college loans. Economic sanctions may also affect children's education through changes in government spending. While direct benefits of public spending on education are widely agreed upon, the effect of sanctions on public spending is unclear. Economic sanctions target government revenues by imposing trade and financial restrictions. However, the effect of a government revenue shock on sub-categories of government expenditures (e.g., expenditure on education) is not clear and depends on fiscal and political institutions.

The key empirical challenge of measuring the effects of sanctions on children's education is one of identification. Sanctions that are not confounded with other factors, that also affected children's education, are difficult to come by. Farjo (2011) finds a reduction in primary school enrollment during 1990-2003 when the UN imposed economic sanctions on Iraq. However, its causal implications are limited because this study does not distinguish the effects of sanctions from the effects of several other relevant factors such as war and political instability.[4] Credible estimates of sanction effects on children's education require a solution to the identification problem. The second challenge is a dearth of reliable data. In most cases, the presence of conflicts poses a substantial obstacle to the collection of survey data especially on the displaced populations and people in conflict areas (Barakat et al. (2002)). Even if data are collected, their accuracy is an open question.

In this paper, I investigate the effects of the 2006 UN sanctions against Iran to identify the impacts of sanction on children's education. For estimation of the sanction effects on children's education, the Iranian setting is well suited for two reasons. First, other factors which affect children's education (e.g., political stability) arguably remain unchanged after the sanctions (Borszik (2016)). Second, there are rich data, Iranian Household Income and Expenditure Survey (HIES), that roughly span the four decades from the 1980s to 2010s (before, during, and after the sanctions). These surveys collected detail information on the children's years of schooling and their family income and

[3] On average, changes in household income or liquidity cause significant changes in household spending among households with low liquid wealth or low income, even when the shock is predictable (Johnson et al. (2006); Stephens Jr (2008); Jappelli and Pistaferri (2014)). Moreover, adjustment costs vary across households depends on their consumption commitments. For example, an adjustment is more costly for homeowners who have to pay the mortgage, especially in the short run. Consumption of many other durable goods (vehicles, furniture) and services (insurance, utilities) may also be difficult to adjust (Chetty and Szeidl (2007)).

[4] Although there are a few studies which analyze the education trends during the years of sanctions, there is a growing literature on the effect of armed conflict on schooling. The results of these studies cannot be generalized to the sanctions cases. Besides that the overall evidence is mixed (depending on the context of conflict and intensity of recruitment during warfare), channels through which education might have been affected are different. Children's education usually decreases during the war because of child soldiering, forced migration and displacement, household labor allocation decisions, security shock, changes in returns to education, and changes in quality and availability of school facilities (Verwimp and Van Bavel (2013), Justino (2011)).

expenditure including spending on education.

On 23 December 2006, the UN Security Council passed Resolution 1737 and imposed economic sanctions after Iran declined to suspend its uranium enrichment program. The UN sanctions include trade and financial restrictions. Trade restrictions targeted specific firms and individuals including oil and gas production and shipping companies, nuclear research and production companies, and military and security services companies owned or controlled or performing on behalf of the Islamic Revolutionary Guard Corps. Theses sanctions mostly targeted investments in and export of oil and gas. Financial restrictions include any transactions with the Central Bank of Iran, disconnecting Iranian bank from the SWIFT, and freezing assets of specific firms and individuals. A consequence, crude oil exports declined to less than one million barrels per day and the growth rate reached -6% in 2012. The targeted sanctions were associated with large, sudden reductions in households' income and consumption. As Figure 1 shows, very shortly after the implementation of the sanctions, the average real income of Iranian households decreased and the decreasing trend lasted for seven years. During 2007-2013, households' real income on average decreased by 35%. As a result, households cut their spending on education by 43%. The reduction in education spending reflects both young children not attending school and parents cutting back on school expenditures.

My identification strategy uses variation in the impact of sanctions on labor income across industries. The empirical strategy to evaluate this shock relies on a difference-in-difference approach. I define households in which the head works in the oil and gas industry as the treated group because these households were directly affected by the sanctions through labor earning reductions. I use water supply and information industries as the control group because there are little income changes for households in these industries, as they are heavily regulated by the government. Therefore, the sanctions have little effects on wages and employment levels of these sectors. Moreover, these industries are not dependent on trade, thus making them unaffected by the changes in the exchange rate. As I show later, these two groups have parallel trends in education outcomes in the absence of the sanctions. I also use the synthetic control method (Abadie et al. (2010)) to construct a control group which resemble relevant characteristics of households in oil and gas industries before the sanctions.

My analysis reveals two main findings. First, sanctions decreased the years of schooling significantly by 0.2 years and probability of attending a four-year college by 8.7 percentage points. This effect on children's education is more than six times larger than previous estimates of the effect of family income on attending college (e.g., Acemoglu and Pischke (2001); Blanden and Gregg (2004); Hilger (2016)) likely because of the persistent shock and lack of adjustment possibilities.[5] I also find this effect is larger for children at crucial ages (high school dropout age and matriculation at a university). In particular, the economic sanctions decreased the probability of attaining college at age 18 and 19 (the average age of matriculation) by 37% and decreased the enrollment rate at the

[5] Acemoglu and Pischke (2001) find a 10% decrease in family income is predicted to decrease college enrollment by 1-1.4 percentage point. Other studies find even smaller effects, for example, Hilger (2016) finds a father's layoff reduces children's college enrollment by less than half of one percentage point, despite dramatically reducing current and future parental income (by 14% initially and 9% after 5 years). He explains that much of reduction in parental spending on education may be offset by greater financial aid.

high school by 12% among children at high school dropout age (16 years old). I consider a simple back of the envelope calculation to understand the economic significance of these results. My calculation shows if these children were able to enroll in college at the same rate as college enrollment in the year 2006 and have the wage rates of the year 2006, their lifetime earnings would increase by 41%. I also find that 45% of the costs to the society associated with the reduction in earnings comes from decreased earning for the current workers, and 55% comes from decreased earning for the next generation. It suggests that the cost estimates using only earnings of current generation may only capture less than half of the overall cost.

Second, I examine the effects of the sanctions on investment in children's education by looking at household spending on education. I find that after the sanctions, households reduced expenditure on education by 61% - particularly on expenditure for school tuition. This finding indicates households respond to the reduction in income by switching their children from higher-quality, more expensive private schools to lower-quality, free public schools.[6] This negative effect on education expenditure is larger than implied by the income elasticity estimates from the previous literature (Qian and Smyth (2011); Huy (2012); Acar et al. (2016)). Most of these studies find that the income elasticity of education spending is significantly less than one.[7] I find an income elasticity of 2.2, indicating households allocate a smaller share of their budgets to education spending after the sanctions.

Overall, after the sanctions, both educational attainment and investment in education measured by family education spending have decreased. This reduction in children's education will reduce their future earnings (by 41%) such that affected children will experience a larger decline in their earnings than their parents. Although the effects of sanctions depend on the context and severity of the sanctions and how government and households cope with this shock, establishing this potential negative shock to human development can edify future policy regarding the use of the economic sanctions.

This paper also contributes to the literature on the effect of family income on children's education in several ways. First, my analysis adds to recent quasi-experimental literature that exploits income shocks by estimating the effect of a persistent income shock caused by the 2006 UN sanctions and lasted seven years (2007-2013). As explained before, persistent changes in family income could have different effects on children than do temporary changes. Most of previous studies exploits temporary income shocks generated by, for example, lotteries, cash transfer, tax credit, housing prices, and oil revenue (Bleakley and Ferrie (2016); Bulman et al. (2016); Dahl and Lochner (2012); Duryea et al. (2007); Løken et al. (2012); Lovenheim (2011); Lovenheim and Reynolds (2013); Manoli and Turner (2018)). The estimated results vary widely (from more than one percentage point per $1,000 to less than one percentage point per $100,000) likely because the research designs (the affected populations, the size, and timing of changes) are different (Bulman et al. (2016)). Despite these differences, all of these papers look at the case in which the exogenous shock in family income is temporary and

[6] In contrast, expenditures on consumption goods, health, savings, etc did not decrease as much as the expenditure on education.

[7] Previous studies find that even for those group of household that education spending is a luxury good, income elasticity is less than 2.

find small effects compared to my results. Even when the shock is large e.g. lotteries, as Bulman et al. (2016) and Manoli and Turner (2018) show, households usually spend lump-sum transfers on durable goods e.g. housing. Therefore, these shocks have small effects on children's education. In the case of parental job loss that the shock has a long-run effect on family income, in developed countries much of reduction in parental resources is offset by greater financial aid e.g college loans (Coelli (2011), Pan and Ost (2014), Hilger (2016)). There are a few studies examine the effect of parental job loss on children schooling in cases that other financial resources are not available to children. Skoufias and Parker (2006) and Duryea et al. (2007) find no effect and positive effect on children schooling during economic crises in Mexico and Brazil respectivly. During recessions, the opportunity cost of education decreases. Moreover, people anticipate economic recovery sooner or later. Thus, recessions may have a positive effect on children's education. Di Maio and Nisticò (2016) show parental loss job caused by a conflict in the Occupied Palestinian Territories increases child school dropout. My study complements these papers by studying a case in which the income shock is persistent and the exception is different because people could not predict whether sanctions would be lifted or not.

Second, I add to the distributional debate about the burden of family income effects. Unlike the existing studies, I estimate differential effects on education investment for households with low, average, and high level of financial resources. As explained before, households respond to an income shock could vary across different income quantiles.[8] The results of existing studies that exploit persistent income shocks are limited to a specific population. For example, Akee et al. (2010) and Bastian and Michelmore (2018) evaluate persistent income changes generated by a casino revenue and tax credits policy respectively. They find larger effects compared to the above studies (1.3 and 4.3 percent increases the likelihood of high school and college completion per $1,000). Different responses of households to a persistent versus a temporary income shock could explain these larger effects. The results of these studies are limited to the population of low-income households.[9] Thus, there was no change for middle and high income households. Contrary, the sanctions affect treated households at any level of income. Therefore, I can estimate the effects for high income households as well as low income households. Moreover, these studies look at positive shocks in the family income. Household responses to upward versus downward shocks could be asymmetric. My paper complements this literature by studying the effects of a negative persistence shock in the family income. By comparing the effects for heterogeneous groups of households, I find that the effect of sanctions on income of middle-income household is larger (18%) and significantly different from the average effect (10%).[10] I also find the negative effects on schooling outcomes are larger for children from low and middle-wealth families. In particular, only children from the lowest wealth quantile

[8] For example, as many studies show, lower income families have a higher income elasticity of education expenditure whereas the higher income families have a lower income elasticity of education.

[9] The casino revenue studied in Akee et al. (2010) is distributed to all Indian households regardless of their characteristics. However, American Indians are a particular group with a low level of income and a high rate of poverty. EITC studied in Bastian and Michelmore (2018) is an antipoverty program that focuses on families whose incomes lie between 75% and 150% of the poverty line.

[10] sanctions has no significant effect on income of household who are rank above the 75th percentile.

experienced a reduction in the years of schooling. Also, sanctions decreased investment in education most for children from the middle-wealth households (-72%).[11]

This paper proceeds as follows. In section 2, I provides the institutional setting. In Section 3, I discuss mechanisms behind the impacts of the 2006 UN economic sanction on children's education and outline a simple model of investment in schooling. In section 4, I describe the data and present the identification strategy. In Section 5, I present the main empirical results on the impacts of the 2006 UN economic sanction on family income and children's education. Section 6 reports some robustness checks. Section 7 explores heterogeneous effects. Section 8 concludes the paper.

2 Institutional Setting

2.1 The 2006 UN Sanctions

On 23 December 2006, after Iran declined to suspend its program for uranium enrichment, the UN Security Council passed Resolution 1737 and imposed economic sanctions against Iran. While Iran's programs to enrich uranium were stopped in 2002, they restarted in late 2005. In July 2006, the UN Security Council in Resolution 1696 had expressed concern at the intentions of Iran's nuclear program and asked Iran to stop its uranium enrichment program by August 31. Although, Iran did not comply with the requirements of the Security Council and the International Atomic Energy Agency (IAEA), the Council did not show any action after the ultimatum, because Iran warned it would break off all talks over nuclear program if any sanctions were imposed. Unexpectedly, in December 2006, the Council imposed trade and financial sanctions on Iran. UN sanctions targeted the oil and gas industry (by imposing restrictions on investments in oil and gas productions, and exports of refined petroleum products) and the Iranian Revolutionary Guard Corps (by banning any business dealings with it). Trade restrictions targeted specific firms and individuals including oil and gas production and shipping companies, nuclear research and production companies, and military and security services companies owned or controlled or acting on behalf of the Islamic Revolutionary Guard Corps. Theses sanctions mostly targeted investments in and export of oil and gas. Financial restrictions encompass banking and insurance transactions (including any transactions with the Central Bank of Iran and Iranian commercial banks). The 2006 sanctions were effective to pressure Iran to negotiate on its nuclear program. In 2013, Iran accepted negotiation for a framework deal over the nuclear program with permanent members of the UN Security Council and Germany (P5+1).[12] On 2 April 2015, they finalized an agreement on a framework deal (Joint Comprehensive Plan of Action (JCPOA)) known as the Iran deal. Thus, the European Union, the United States, and the UN Security Council have terminated all nuclear-related resolutions and sanctions.[13]

[11] These effects are not significant for children from high-wealth families.

[12] China, France, Russia, the United Kingdom, and the United States plus Germany

[13] United Nations Security Council Resolution 2231, passed on 20 July 2015, suspends UN sanctions and sets out a schedule for lifting them gradually. This resolution also considers reimposing the sanctions in case of Iran's failure to comply with the framework agreement. Resolution 1737 was terminated on the day of implementation of the Joint Comprehensive Plan of Action (JCPOA), 16 January 2016, by Resolution 2231 of the United Nations Security Council.

The 2006 sanctions are the most severe sanctions ever put on Iran because most countries including the European Union stopped buying oil from Iran. Moreover, the United States has introduced sanctions for punishing other countries that buy oil from Iran. Furthermore, since sanctions limited access to many products and technologies needed in the oil and energy industries, many oil companies withdraw from Iran oil industry and Iran's oil production decreased. Therefore, Iran lost $160 billion oil revenue. In addition, more than $100 billion in Iranian assets was held in restricted accounts outside the country. As a result, Iran's economy got 15 to 20% smaller than it would have been absent the sanctions (U.S. Treasury Secretary Jacob Lew report, 2015). Since Iran's economy depends heavily on oil exports and goods imports, economic activity declined which led to a two-year recession. The growth rate has reached an all-time low of -6% in 2012. Meanwhile, the value of the Rial (the currency of Iran) declined by 56%, and inflation reached 35%. As Figure 1 shows, very shortly after the implementation of the sanctions, the average real income of Iranian households have decreased. During 2007-2013, households' real income on average decreased by 35%. As a result, households cut their total expenditure and spending on some classes of goods. Households' spending on education showed the highest drop of -43%.

2.2 Educational Trends in Iran

Although Iran's economy has faced many challenges during 1995-2006, the years before the sanctions were instituted, educational attainment and household spending on children's education have never stopped growing.[14]

Educational attainment in Iran has improved substantially in the past four decades. Education has expanded in MENA faster than in any other region of the world (World Bank). Some countries such as Iran, Turkey, Egypt, and Jordan experienced more growth in education. In Iran, enrollment rates exceed 90% at the primary and secondary levels, comparable to that of Western countries. Thus, the youth literacy rate has increased from 56% in 1976 to 97% in 2006 (World Bank).[15]

The rapid growth in the education sector is supported by both private and public spending. The average private and public investment in education as a percentage of GDP is 5% and 4% of GDP in 2006 respectively. Over the past three decades, because of increases in youth population and demand for education, the Iranian government has shown a strong commitment to funding public education and promoting access to fee free public schools at all level of education.[16] However, like most Middle Eastern countries, a large share of Iranian government spending on education is allocated to post-

[14] Over these years, Iran's economy has been under various economic sanctions. The first economic sanctions on Iran were imposed by the United States following the Iranian Revolution of 1979. US sanctions were gradually expanded to the present level with a total embargo on all bilateral trade and investment. The studies show US sanctions' economic and political effects have been insignificant (Alikhani (2000); Askari et al. (2001)). According to Hufbauer et al. (2012), the average welfare loss caused by US sanctions on Iran over the period 1984-2005 was around $80 million, less than 1% of Iranian GDP over that period.

[15] The youth literacy rate is the percentage of people ages 15 to 24 who can read, write, and understand a short simple statement about their everyday life.

[16] Based on the article 30 of the Constitution of the I.R. of Iran, "the government is obliged to provide free of charge education for all individuals up to the end of the secondary level of education and to facilitate free higher education up to achieving self-sufficiency" (Source: UNESCO, the World Education Forum report for Iran (2015)).

secondary education in large urban areas.[17] Thus, public universities are of high quality and free-tuition, but the number of places at public universities is limited. A highly competitive university entrance examination rations these free-tuition places at public universities.[18] The competition to succeed in school and the university entrance examination have encouraged parents to spend on their children's education such as sending on private schools and private tutoring to help their children in this competition (Salehi-Isfahani (2012)).[19] As Figure 1 shows, Iranian households' spending on education, which is the major source of education funding in Iran, has increased by 67% during 1995-2006. Spending on primary and secondary schools tuition is a significant share of total household expenditure on education in Iran. Many of the best overall primary and secondary schools in Iran are privately funded. Parents believe that private primary and secondary schools offer a better education, an environment more conducive to learning, additional resources, and better policies and practices. Indeed, results from value added to cognitive achievement show that private school students averaged higher than their public school counterparts. Moreover, children who attend private schools perform better in school final exams and the university entrance examination and have better academic outcomes than those in public schools (Dolatabadi (1997); Rabiei and Salehi (2006)).

Evidence of how the 2006 sanctions affected children's education can be found in the time series trends. While the enrollment rates did not change for primary and secondary education, the enrollment rate in the undergraduate program dropped after the sanction. According to the Statistics Center of Iran (SCI), during 2007-2013, the enrollment rates in primary and secondary school were always around 97% and 89% respectively. At the same time, the number of first-year students in four-year college decreased by 11.5% (source: Statistics Center of Iran).

Moreover, during the sanctions, the investment in children's education measured by household spending on education has decreased. Households' spending on education on average decreased by 43%. This reduction in households' education spending could be because young children do not attend school and/or parents cut spending on school expenditures, for instance, choosing free public school instead of private school. The data shows although the enrollment rates did not change for primary and secondary, the proportion of primary and secondary students who were enrolled in private schools decreased from 21% in 2006 to 10% in 2013 (source: Iranian Households' Income and Expenditures Surveys).

3 Conceptual Framework

In this section, I explore the mechanisms by which economic sanctions may decrease investment in children's education and then I outline a simple model of investment in schooling based on Acemoglu

[17] Tertiary education was nearly all public until the 1980s. In 2006 about half of all university students were enrolled in public universities.

[18] Only 10% of students who take the university entrance exam, win that scholarship.

[19] For instance, in HIES, 58% of pre-university students receive private tutoring to increase their probability of success at the university entrance examination. Such tutoring spending is a significant item in households' education expenditure (52%).

and Pischke (2001).

3.1 Mechanisms behind Sanctions

The sanctions affect children education through changes in demand side (labor income and relative prices) and supply side of schooling (government budget).

One mechanism by which the sanctions affect children education is through labor income. As explained before, the sanctions targeted Iran's oil and gas industry. Therefore, the growth rate in this industry has reached an all-time low of -37% in 2012.[20] As a result, labor earnings decreased in this industry. The changes in labor income may affect investment in children's education through two channels: family budget constraint and changes in returns to education.

First, labor income shocks may affect children's education through family budget constraint. In influential work, Acemoglu and Pischke (2001) provide theoretical and empirical support for the idea that parental resources can affect education decisions through budget and credit constraints because education is not a pure investment and can be a consumption good too. Since children are dependent on others, their family's economic circumstances make them enter or avoid poverty. Children cannot change their family conditions, at least until they reach adulthood (Brooks-Gunn and Duncan (1997)). Reduction in family income after the sanctions may have made it harder for children to attend school. However, as explained before, households might adjust this shock to mitigate the impact of sanctions on children. For example, they can draw down savings or sell off assets to smooth consumption in response to a negative income shock (Deaton (1992); Browning and Lusardi (1996)). However, if sanctions increased uncertainty about future income, households consume less and save more (Sandmo (1970)). I estimate Eq (1) for family savings,[21] debt and non-labor income.[22] I find no significant effect (Table 11). Thus, there is no evidence that sanctions changed family saving.

Second, labor income shocks may affect children education by decreasing returns to education, a theoretical possibility explored formally by Eckstein and Zilcha (1994). The accumulation of human capital is an investment decision. Since education is costly (tuition fees and forgone earnings), individuals will invest in additional schooling only if sufficiently higher future earnings compensate for these costs. Therefore, optimal investment in children's education requires parents to take into account their children's income gain due to their education. Falling labor income due to economic sanctions affects the returns to education. However, the effect of this change on education is not clear. On the one hand, it can decrease educational attainment by reducing expected earnings from additional schooling. On the other hand, poor labor market opportunities could increase the incentive for investment in human capital by increasing competition. Moreover, the wage rate decreased for all level of education. As a result, the income of some low education level that used

[20] The average growth rate for oil value added is -6.4% during the years of the sanctions.

[21] The family saving is not reported in HIES. I calculate the saving by subtracting total consumption from total family income (saving=income-consumption).

[22] The non-labor income of each member of a household includes financial transferred aids, real estate incomes, subsidies, interest on bank deposits, bonds yield and share dividends, scholarships and cash gifts from others. I consider the summation of all members' non-labor income as the family non-labor income.

to be above the poverty line moved down to below the poverty line after the sanction. Thus, the incentive to invest in education can increase after the sanctions. Since the effect could go either way, I need a structural model incorporating different features of the sanctions to identify the effect of this channel. It is left for the future work.

Another mechanism by which economic sanctions may affect children's education is through changes in the government's budget. The 2006 UN sanctions did not affect public spending on education. The sanctions affected Iranian government revenue and its internal composition because on average 60% of Iranian government revenues come from oil and gas which was affected by the sanctions.[23] However, as Farzanegan (2011) shows the Iranian government only reduced the military and security spending after the sanctions. Other social spendings of the Iranian government including health and education does not show a significant response to this shock.[24]

Moreover, economic sanctions may affect children education through changes in the relative prices. In addition to the reduction in household income, rising prices decreased households' spending capacity after the sanctions. During the sanctions, prices of many commodities spiraled upwards and inflation reached 35%. However, the magnitude of this change is different across goods and services.[25] Thus, the relative prices and so the budget shares of the different commodities have changed (for more detail see Online Appendix).[26] Although education prices doubled, the changes are not as much as other commodities.[27]

3.2 A Model of Investment in Schooling

I now outline a simple model to identify the channels (illustrated in the previous part) through which sanctions affect children's education. People live for two periods. In period one, parents work, consume, save, and decide how much money to spend on their children's education. Households receive utility from consuming goods and children's human capital:

$$U = u(c, c') + h(HC)$$

[23] Iranian government spending includes current and capital expenditures. Current expenditures include all spending on government employees' wage and pensions, military, health, education, and cultural and social activities. Spending on defense and security expenditures is the major component of Iranian government spending, followed by spending on education (Farzanegan (2011)). Moreover, the composition of public spending for primary to tertiary education has not changed after the sanctions. Government expenditure per student at the primary, secondary and tertiary level were always about 26%, 37% and 37% of total government spending on education respectively (source: World Bank).

[24] Habibi et al. (2001) shows that oil revenue fluctuations in Iran and other Middle Eastern oil exporting countries did not affect the basic social spending on education, health, and welfare.

[25] In particular, the prices of tradables (typically goods) have risen significantly relative to non-tradables (typically services).

[26] The budget shares of the various commodities are related to the real total expenditure and relative prices (Deaton and Muellbauer (1980)). Indeed, the descriptive analysis showed that sanctions significantly changed the households' consumption pattern. The most significant change is related to the expenditure share allocated to food. While food prices became sixfold in 2013 since expenditure on food is necessary expenditure and unsubstitutable, expenditure share on food increased by 6% (from 40% to 46%).

[27] Education Price Index (EPI) have increased in average 8% less than the overall rate of inflation.

where c and c' are the first period and second period household's consumption, respectively. Children's human capital (HC) is determined by quantity (Edu) and quality ($QEdu$) of education. Parents expect that their investment in children's education will have payoffs in terms of higher income later for their children. Parents may value children's education for a number of reasons. First, in the second period, they depend on their children and highly educated children will be better providers. I assume parents receive μ percent of their children's income in the second period. Second, the happiness of children may make parents happier ($h(HC)$), so they have an incentive to spend money on children's education. The cost of schooling for a family is $exp(Edu, QEdu, \theta)$, where θ is children's ability which is transmitted from parents. Parents' ability reflects in their income. Thus, this model allows for heterogeneity among households. A low quality education is provided by the government which is costless for parents. Low educated workers receive w^u and return to education for any additional year of schooling is w^e and to any additional spending on schooling is w^q. The household maximization problem with income y is choosing consumption (c and c') and children's education (Edu and $QEdu$) subject to:

$$c + exp(Edu, QEdu, \theta) + s \leq y$$
$$c' = \mu[w^u + w^e(1 + w^q QEdu)Edu] + s$$

where s is household saving in period one ($s \geq 0$). Therefore, the cost of investment in children's education is lower consumption in the first period. If parent's income and/or return to education are high enough, parents would like to spend on their children's education. From a comparative static analysis, total derivatives are:

$$dEdu = constant + f_y(.)dy + f_e(.)dw^e + f_q(.)dw^q + f_u(.)dw^u$$
$$dQEdu = constant + g_y(.)dy + g_e(.)dw^e + g_q(.)dw^q + g_u(.)dw^u$$

Amusing u and h are strictly concave functions, $g_u, f_u < 0$ and sign of f_y, f_e, f_q, g_y, g_e, and g_q are positive (for more detail see Online Appendix). Labor income shocks caused by sanctions may affect family income (y) and/or return to education (w^e, w^q) and thus discourage parents from investing in children's education (e.g., if $dy < 0$ then $f_y(.)dy < 0$ and $g_y(.)dy < 0$). However, sanctions also decease wage rates for low educated workers ($dw^u < 0$ then $f_u(.)dw^u > 0$ and $g_u(.)dw^u > 0$). Thus, the incentive to invest in education can increase after the sanctions. The overall effect is ambiguous and depends on the relative strength of different effects. I empirically test the overall impact of sanctions.

4 Data and Identification Strategy

4.1 Data

The main data source is the Iranian Households' Income and Expenditures Surveys (HIES). This sample covers near 40,000 households every year. It is conducted yearly by the Statistics Center of Iran (SCI). These surveys which are rotating panels gather extensive data on expenditures of households including detail information on education spending.[28] Moreover, this data contains rich information at the individual level including age, gender, years of education, income, marital status, and relation with the head of family.[29]

The Iranian data are ideal for studying the effects of family income shock on children for two reasons. First, I can link children to their parents and observe their family characteristics (such as family income, parents' education, family size, family expenditure). Second, the HIES contains child years of schooling, enrollment at different level of education, and family education spending. Education spending includes payments for books, tuition, private tutoring and donation to the school for the different level of education (pre-primary and primary, secondary, post-secondary non-tertiary, tertiary and education not definable by level).

I restrict my main sample to the households with children aged 6-24 because children start school at age 6 and most individuals complete their education by age 24 in Iran.[30] Children aged six who born at the start of the academic year (September 23th) or later are excluded because they are not eligible to enroll in school.

I choose my sample period to be all observations during years 1995 to 2013 (1374 to 1392 in Persian Calendar), 12 years before and 7 years after the sanctions. I exclude the years 2014 and 2015 when Iran and P5+1 were negotiating over the nuclear program, and people would expect the sanctions to be terminated. I re-conduct the analysis including 2014 and 2015 as robustness check in section 6.

For the main analysis, I study households who live in urban regions of the country because there are differences between rural and urban areas in factors affecting education expenditure. Computed elasticities indicate that spending on education by rural households is more sensitive to changes in income compared with urban households (see, for example, Mussa (2013)). Moreover, education opportunities are different between cities and rural communities, in particular all rural schools are public and private supplementary tutoring are not available to students in rural areas. I do not lose too much of the sample because 75% of the population live in urban areas.

[28] HIES reports detail information on expenditures on education according to the Classification of Individual Consumption According to Purpose (COICOP) for each household.

[29] HIES reports detail information on labor income including permanent and non-permanent incomes, and non-labor incomes for each member of the family.

[30] Less than 5% of students are aged above 25.

4.2 Identification Strategy

The empirical strategy to evaluate the 2006 economic sanctions relies on a difference-in-difference approach. The first difference is over time. The second difference is across groups of households. My identification strategies use the different severity of the effects of sanction across industries. The difference-in-difference comparison is implemented by estimating regressions of the following type:

$$Y_{ispt} = \alpha + \gamma\ (Oil_i \times Post2007_t) + \beta\ Oil_i + \lambda_t + X'_{ispt}\delta + \phi_p + \psi_s + \varepsilon_{ispt} \quad (1)$$

where Y_{ispt} is the outcome variable of interest (family income, family expenditure, and children education outcomes) of individual (or household) i in province p and industry s at time t. The variable Oil_i is a dummy for treatment group (equals one if household's head works in the oil and gas industry, and zero otherwise) to control for group-specific differences; $Post2007_t$ is a dummy to reflect sanctions being imposed in 2007; λ_t is a vector of time fixed effects to control for changes in macroeconomic conditions. I also add province and industry fixed effects, ϕ_p and ψ_s, to control for time-invariant local market and industry characteristics that affect family income but are not observable to me. The vector X_{ispt} is a set of individual or region specific characteristics to control for any observable differences that might confound the analysis (for instance age for estimation years of schooling). Since outcome variables, e.g. income are likely to be correlated within local labor market and industry levels, all observations are clustered at province (29 provinces) and industry (3 industries) level (87 clusters) to account for correlation within observations, which may result in an underestimation of standard errors. Since there are few clusters at the industry level, t tests based on cluster-robust variance estimator (CRVE) tend to be over-rejected (MacKinnon and Webb (2018)). Moreover, different variants of the wild cluster bootstrap can over-reject or under-reject.[31] To solve this problem and calculate p-values, I follow MacKinnon and Webb (2018) and apply the correction for the small number of clusters by using wild bootstrap randomization inference (WBRI). The coefficient of interest is γ which measures the average effect of the economic sanctions on the treated group relative to the comparison group, using variation over time.

I define households in which the head works for the oil and gas industry as the treated group.[32] Although the sanctions affected many sections of Iran's economy, the severity is different across industries.[33] Based on detailed policy documents on the 2006 sanctions, only people who work for oil and gas industry were directly affected by the sanctions.[34] As Figure 2 shows, the average real annual income of households that the head works in the oil and gas industry decreased from 198 to 115 million Rials (decrease by 42%). The reduction in household income can be related to a decline in working hours or wage rate (or both). Figure 2 also shows the working hours have not changed

[31] When a few clusters are treated, in many cases the restricted wild cluster bootstrap under-rejects, and the unrestricted wild cluster bootstrap over-rejects (MacKinnon and Webb (2018)).

[32] The sample contains some households with an old head that have married children older than 40 years living with them. These families consider the eldest person as the head of the household. I consider such families as extended grandparent families. Therefore, I redefined the household head as the person earning the highest monetary income, mostly the same as the person reported as the head of the household.

[33] I use SCI classifies industries according to International Standard Industrial Classification (ISIC).

[34] United Nations Security Council Resolutions 1696, 1737, 1747, 1803 and 1929

during the sanctions. However, the average real wage per hour in this industry decreased from 45 in 2006 to 25 thousand Rials in 2013 (decrease by 45%).

The definition of the comparison group is crucial, as it should capture counterfactual outcomes trends in the absence of the sanctions. In the ideal case, sanctions would be an independent random event for oil and gas industries that had no spillover effect to other industries. The present analysis is not such ideal case because Iran's economy is dependent on exports of oil. Thus, sanctions indirectly impacted other industries through the government budget and exchange rates. Comparing households in oil and gas industries with any control group lead to underestimation of the effects of sanction. One potential comparison group would be households in which the head works for non-oil industries. This group is not a good comparison group because workers in the oil and gas industries differ from workers in non-oil industries in characteristics that are thought to be related to the potential for children's education. In fact, the pre-treatment trends of family income and education outcomes are not parallel for these two groups.

I use two identification strategies to approach this problem and decrease underestimations. First, I compare income and children's education of households in most affected industries (oil and gas) with that of least affected industries which have similar characteristic of households in oil and gas industries before the sanctions. As I show later, households in water supply and information industries are comparable to households in the oil and gas industries for two reasons. First, these two groups have parallel trends in outcomes in the absence of the sanctions. Second, households in water supply and information industries experienced the lowest incidence of family income changes after the sanctions. Two features of these industries protect them from the sanctions. First, these industries are heavily regulated by the government. Therefore, their wages and employment size are little responsive to the market conditions. Second, these industries are not dependent on trade, thus making them unaffected by the changes in the exchange rate due to trade restrictions after the sanctions. Figure 3 shows the trends in head's real wage and salary income over the sanctions among oil and gas industry (as treated) and Water supply and Information industries (as control group). Figure 9 shows the trends in head's real wage and salary income in other industries compared to the one for households in oil and gas industry. Table 14 reports summary statistics for the full sample and separately by treatment status, as well as tests of treatment-control balance.

My second identification strategy is using the synthetic control method (Abadie and Gardeazabal (2003); Abadie et al. (2010)) to estimate the counterfactual for households in oil and gas industries. I use a weighted combination of less affected industries to construct a synthetic control group which resemble relevant characteristics of households in oil and gas industries before the sanctions. I restrict the donor pool (the set of control industries) to industries that experienced less than 10% change in the real labor income during the years of sanctions (Agriculture, Water supply, Accommodation and food service, Information, and Human health and social work activities), though I check the sensitivity of results using different selected donor industries. Weights are determined to maximize the similarity between the synthetic control and the treatment unit in terms of matching variables including parent's education, employment status, age, etc. The optimal weights are positive for three

industries water supply, information, and health with values 0.864, 0.103, and 0.034 respectively and take value zero for the other potential controls. Compare to the control group used for the first identification strategy, households in the health industry are added but the weight of this group of households is only 0.034. Moreover, unlike the control group used for the first identification strategy, the weights of households in water supply and information industries are not equal, such that households in the water supply have the major weights in the synthetic control. Table 14 (column 4) reports households' characteristics in the synthetic group comparing to treated households. I report the results using the synthetic control in the Section 5.3.

To explore the dynamic impact of the sanctions, Eq (1) is generalized by replacing $Oil_i \times Post2007_t$ with a full set of treatment times year interaction terms:

$$Y_{ispt} = \alpha + \sum_{l=1995}^{2013} \gamma_l \ (Oil_i \times d_l) + \beta \ Oil_i + \lambda_t + X'_{ispt}\delta + \phi_p + \psi_s + \varepsilon_{ispt} \tag{2}$$

where d_l is a dummy that is 1 in year l and 0 otherwise. The pre-2007 interaction terms provide pretreatment specification tests, although they may capture possible anticipation effects.

This estimation method requires several identifying assumptions. First, the key identifying assumption is that treated and control groups are comparable. I check whether treated and control groups have parallel trend of outcome variables and similar household and children characteristics in the absence of the sanction. Also, sanctions should not have any effects on outcomes that are not supposed to be affected by the treatment (falsification test). Second, since the data is repeated cross sections, I need to make sure the composition of the sample has not changed between periods. This assumption is necessary so that if any trend change occurs between groups, I can attribute the deviation from the time trend to the effect of the sanctions, not to the change in the composition of the group members. For observed characteristics, I check the covariate balance and labor movement. For unobserved characteristics, as explained before, all observations are clustered at province and industry level. In appendix C, I provide analysis on the validity of these assumptions.

5 Results

I analyze the direct impact of the 2006 economic sanctions on family income and the indirect effects on children education. First, I document the effects comparing most affected with least affected households (Section 5.1 and Section 5.2). I then report the effects using the synthetic control method (Section 5.3). For all specifications, I report the results estimated using OLS regressions.

5.1 Effect on Family Income

I first examine how the sanctions affected family income. To do so, I look at the effects on total family income as well as labor market earnings, wage rate, and employment. UN sanctions targeted investments in and exports of oil, gas, and petrochemicals. As a result, crude oil exports had declined from 2.5 million barrels per day to less that one million in 2013. This change could potentially affect

the income of workers in the oil and gas industry through unemployment, inflation and falling wages.

As Table 1 reports, labor income and total income of families that the head works in the oil and gas industry decreased by 13% and 10% respectively. Columns 3 shows that the real wage rate in the oil and gas industry decreased by 12% after the sanctions. In fact, the nominal wage rate increased in the oil and gas industry, but it had not been synchronized with the rate of inflation. There is no significant effect on working hours (Columns 4). This reduction in income is independent of worker's abilities since it is due to a shock in the economy whose effects does not depend on skills and abilities.

Figure 4 shows the DID estimates of dynamic effects on the labor income (coefficients of the year treatment interaction terms in (Eq (2)). While the estimated coefficients are not significantly different from zero before 2007, they turn significantly negative after the sanction imposed in 2007. Although, there are fluctuations in the income of households in oil and gas industries even before the sanction, as we will see these temporary shocks had no effects on children's education.

Table 2 shows the effect of this negative income shock on household expenditure. As Table 1 shows, the total income of families that the head works in the oil and gas industry decreased by 10%. Consequently, they reduced the total spending by 7% (Table 2). Although spending decreased for most components, it did not decrease by the same rate. As Table 2 shows, households cut spending on education by 61%. Moreover, spending share on education decreased by 0.7% (for more detail about the effects on sub-categories of households expenditures see Online Appendix).

5.2 Effect on Children's Education

The reduction in education spending reflects the combination of young children not attending school and parents cutting back on school expenditures. For instance, parents may choose free public school instead of private school for their children. I measure education outcome using enrollment rate, completed years of education and education spending. The sample consists of all children aged 6-24 over the period 1995 to 2013.

5.2.1 Effect on Enrollment and Years of Schooling

First, I find the impact of the sanctions on the educational attainment measured by the enrollment rate and years of education. In Iran, education is compulsory until the end of high school one or grade 9. Figure 7 shows non significant effects on enrollment in primary school and high school one during the sanction years, as expected. Table 3 presents the effects on school enrollment in high school two, and attending any college.[35] As the first column of Table 3 shows, the sanctions have no significant effect on enrollment in high school two. The second column of this Table shows that the probability of attending college significantly decreased by 8.7 percentage points after the sanctions. I Also find years of schooling significantly decreased by 0.2 years after the sanctions for

[35]The sample for high school two is children of the age group who are officially eligible to enroll in high school two (15-18 years old). The sample for college is high school graduates who are under the typical college graduation age ($\leq$24 years old).

whole sample (panel A, column 4 of Table 3), and decreased by 0.4 years for children age 15-24 who completed grade 9 (panel A, column 3 of Table 3). Also, a simple calculation (Average Years of Schooling $= \sum_{S_i=0}^{22} (P_i S_i)$ where S_i is years of schooling and P_i is percentage of children age 6-24 at different level of education) confirm that at the prior rates of college attendance and enrollment at different education levels, years of education on average decreased by about 0.2 years after the sanctions. Moreover, panel B of Table 3 shows that the effects are not different across gender.

Figure 5 shows the DID estimates of dynamic effects on the years of schooling. As this Figure shows, before 2007 there was no effect on years of schooling although there were temporary shocks in labor income. Estimated coefficients turn significantly negative after the sanction imposed in 2007.

I compare my results to current literature and the overall effects on the current generation to find how big these negative effects on children's education are. My finding is consistent with the literature documenting a connection between family income and children's education(Acemoglu and Pischke (2001); Blanden and Gregg (2004); Akee et al. (2010); Løken (2010); Coelli (2011); Lovenheim (2011); Lovenheim and Reynolds (2013); Pan and Ost (2014); Bastian and Michelmore (2018); Bleakley and Ferrie (2016); Hilger (2016); Manoli and Turner (2018)). My result is large compared to other studies. I find that a 10% decrease in family income is predicted to decrease college enrollments by 8.7 percentage points. Acemoglu and Pischke (2001) find that a 10% increase in family income increases college enrollments by 1-1.4 percentage points. Bulman et al. (2016) find the modest per-dollar effects of a positive income shock caused by lottery. They find the relationship is weakly concave, with a high upper bound for amounts greatly exceeding college costs. They also find the effects are smaller among low income households because lump-sum transfers are more likely to be spent on durable e.g. housing. My results are also larger compared to the results of existing studies that exploit persistent income shocks generated by, for example, tax credit and job loss. For example, Hilger (2016) finds a father's layoff reduces children's college enrollment by less than half of one percentage point, despite dramatically reducing current and future parental income (by 14% initially and 9% after five years). He explains that much of reduction in parental spending on education may be offset by greater financial aid. Such financial aids, e.g. college loans are not available to Iranian children. Therefore, the large effects estimated in this paper are expected because of the persistent shock and lack of adjustment possibilities to the shock.

I consider a simple back of the envelope calculation to understand the economic significance of these results. Children growing up after the imposition of sanctions may have lower earnings throughout their adult lives. Sanctions can affect the lifetime income of the next generation through two channels: lower wage rates and lower education levels. To find the children's earnings loss due to the sanctions, I compare the present value of future lifetime earning of children with and without the sanctions.

$$I_j = \sum_{t=0}^{T} \beta^t (w_j^H Income_{tj}^H + w_j^C Income_{tj}^C) \quad , j = s, ns \tag{3}$$

where I_s and I_{ns} are children's lifetime earning with and without the sanctions respectively. w_j^H and w_j^C are the percentage of children with a high school or less and the percentage of children with university degree, respectively. T is the number of working years and β is the discount rate (0.95). I do not observe $Income^H$ and $Income^C$ (real annual income at different ages for high school graduates and college graduates) because these children who are affected by the sanctions are not yet old enough to directly measure their earnings. Children's future annual income may be imputed from the information on children's levels of education, using the relationship between earnings and education in observed data. I consider different scenario for their income: (i) median/average of (all/oil and gas) workers' income in the last year of sanctions (year 2013),[36] and (ii) median/average of (all/oil and gas) workers' income before the sanctions (year 2006).[37] Since HIES is a cross-sectional survey, I observe single-year measures of the earnings. Short-run measures of workers' earnings include both measurement error and transitory fluctuations in earnings. Thus, I select a period to observe the representative-workers when their earnings are most likely to accurately reflect permanent earnings, ages 30-50 (the prime earnings years). Similarly, I estimate the present value of lifetime earning of current generation employing the annual income before and after the sanctions to find parents' earnings loss.

The first exercise is to calculate what the expected magnitude of the children income would be if the sanctions had not been imposed. As mentioned before, the sanctions can affect the lifetime income of the next generation through two channels: lower wage rates and lower education levels. To find the total effect, I compare the case where college enrollment rate has decreased, and the real income is constant at its lowest value in the last year of sanctions (year 2013), to the case where children were able to enroll in college at the same rate as college enrollment in the year 2006 (before the sanction), and real income equals to its highest value in the year 2006. A back of the envelope calculation shows a 41% reduction in children's lifetime earnings. I also decompose the total effect of the sanctions on the children lifetime income into the sole effect of the reduction in education levels and the sole effect of the reduction in the wage rates. My calculation shows that the reduction in college enrollment rates will decrease children's future lifetime earnings by 3-4%. A similar calculation shows that the reduction in wage rates will decrease children's future lifetime earning by 38%.[38]

It is also interesting to ask, how large is the children income loss in economic terms? One way to assess the size of this loss is to compare it with earnings loss of the current workers due to the sanctions, and real GDP. My calculations suggest that a one dollar reduction in parents' permanent

[36] For this scenario, I assume the wage rates cannot recover after the lifting sanctions.

[37] For this scenario, I assume the wage rates will recover after the lifting sanctions.

[38] If children were able to enroll in college at the same rate as college enrollment before the sanction, but the wage rates decreased from the rate in 2006 to the rate in 2013.

earnings leads to a subsequent reduction in children's earnings of 1.2 dollars.[39] I also find that the costs to the society associated with the reduction in earnings after the implementation of the sanctions total about 18% of GDP. 45% of this reduction comes from decreased earning for the current workers, and 55% comes from decreased earning for the next generation. It suggests that the cost estimates using only earnings of current generation may only capture less than half of the overall cost.

There is, however, some potential drawbacks of this method. First, this procedure relies on the assumption that cohort effects on the earnings profile are minimal. Second, this simple calculation ignores individual characteristics that can affect children's earning.

5.2.2 Effect on Education Spending

So far, I have looked at the educational attainment measured by the enrollment rate and years of education. Now, I examine the effect of the sanctions on investment in children's education measured by household spending on education.[40] The education spending is the explicit costs associated with payments in cash such as books, school tuition, donation, tutoring, university tuition and other education expenditures (for instance extra classes). Based on HIES, before the sanctions, the average percentage of family educational spending was about 3%.[41] The school tuition fee constituted a significant proportion of total education costs (20%). Table 15 in appendix C shows the share of education spending to each item before the sanctions for the full sample and separately by treatment status, as well as tests of treatment-control balance. The variables overall are well balanced between the control and the treatment groups.

Table 4 presents the effect of the sanctions on education spending (it includes zero education spending for non-enrolled children, since I used this variable in log form, I added one to values of this variable and then log-transformed). As this table shows, households spent less on school tuition, books, and private tutoring after the sanction. Column 1 shows that spending on school tuition significantly decreased by 60%. This finding indicates that households respond to the sanctions by substituting away from higher-quality private schools towards lower-quality public schools for their children. Moreover, households spend 22% less on books after the sanctions. Also, spending on private tutoring decreased by 76%. The big share of spending on private tutoring is for pre-

[39] This effect is larger to previous studies. Oreopoulos et al. (2008) using Canadian data find that a one dollar reduction in father's permanent earnings due to a job loss leads to a subsequent reduction in his son's earnings of 66 cents. One possible reason for this difference is that previous studies looked at cases that affect the lifetime income of the next generation only through a reduction in the education levels. In the case of Iran, the economic condition, e.g., wage rates have also changed after the sanctions. Moreover, as Grawe (2001) shows the intergenerational earnings mobility in the developing countries is larger because of the larger credit constraints.

[40] While the effect of high-quality education on the returns to schooling and economic growth is well known (Castelló-Climent and Hidalgo-Cabrillana (2012)), the effect on household spending on education is not documented in the current literature. Previous research has largely focused on children's educational attainment.

[41] For Canada and UK, the percentages were about 1.1 and 1.2% respectively in 2009. Furthermore, according to Huston's study (1995) using 1990-1991 Consumer Expenditure Survey for the US, the household educational expenditure consisted of about 1.95% of total household income. For the 25 EU countries, the private expenditure on education as a percentage of total household consumption during 1995-2004 ranged from 0.1 to 2.9%. The average was about 1% (Lin and Lin (2012)). The share of education expenditure in household expenditure is 4.3% in all India (Azam and Kingdon (2013)).

university students to increase their probability of success at the university entrance examination. The second Column of Table 4 shows the effect of the sanctions on the share of each item in the total household expenditure. The percentage allocated to school tuition significantly decreased by 0.4%.

I also test the effect of the sanctions on the education spending per child. The decline of fertility in Iran over the past decades can explain the reduction in household education expenditure. The decline of fertility drives the number of students in households to fall. The average number of students in household declines from 1.7 in 1995-2006 (before the sanctions) to 1.2 in 2007-2013 (after the sanctions) period. The average number of children in households who are enrolled in elementary and high schools also shows a decrease from 1.6 to 1. However, the average number of college students in household shows an opposite trend and increases from 0.14 to 0.25. In other words, the average number of college students is not affected by the recent decline in fertility. Column 3 and 4 of Table 4 present the results for education spending per child. For school tuition, the sample consists of all children aged 6-24 who have not graduated high school. For university tuition, the sample consists of children aged 6-24 who have graduated high school. For spending on books and private tutoring, I have considered all children aged 6-24. As column 3 and 4 of Table 4 show, the spending on school tuition for each child significantly decreased by 57% and the percentage allocated to school tuition of each child significantly decreased by 0.2%. Moreover, the percentage allocated to books for each child significantly decreased by 13%.

Although college attendance significantly decreased after the sanctions, as Table 4 show, there is no significant effect on the university expenditure. The baby boom in the 80s can explain it. The population of this group has increased as the result of the baby boom in the 80s. The percentage increase in the population of this group was greater than the increase in the population of college students. Thus, the enrollment rate has decreased. However, since the number of college students in households has increased, the household spending on university has not changed.

5.2.3 Income Elasticity of Education Spending

To compare these negative effects on education spending to the current literature, I calculate the income elasticities of education spending. Following Grimm (2011), I use a 2SLS estimator and instrument income with the interaction effect being a child in an oil and gas household after the sanctions conditional on being in an oil and gas household and the time effects. As explained before, since HIES is a cross-sectional survey, I observe single-year measures of the earnings which include both measurement error and transitory fluctuations in earnings. Following (Tansel and Bircan (2006)), I use total family expenditure as a proxy for family income because total expenditure represents permanent income better than current income. Moreover, there are fewer errors in measuring total expenditure than in measuring income. I also use family income itself as a robustness check. I estimate the following equation:

$$lnEdu\_exp_{ipt} = \alpha + \xi \widehat{lnTotal\_exp_{ipt}} + \beta\ Oil_i + \lambda_t + X'_{ispt}\delta + \phi_p + \psi_s + \varepsilon_{ispt} \quad (4)$$

where $\widehat{lnTotal\_exp_{ipt}}$ is the fitted value of total household expenditure derived from the the first stage equation given by

$$lnTotal\_exp_{ipt} = \upsilon + \gamma\ (Oil_i \times Post2007_t) + \iota\ Oil_i + \kappa_t + X'_{ispt}\nu + \varphi_p + \Psi_s + \varsigma_{ispt} \tag{5}$$

where i denotes family, p denotes province, s denotes industry, and t denotes time. Edu_exp_{ipt} is household education spending and $Total\_exp_{ipt}$ is total household expenditure as a proxy for family income. The vector Z_{ipt} is a set of family specific characteristics that are correlated with both educational spending and income like parents' education. Since education spending and total household expenditure are both in logarithmic form, ξ denotes elasticity. I use the Tobit model for the second stage because education spending is zero for some households. Thus, this variable is censored at zero.[42]

Table 5 shows the maximum likelihood estimation results of Eq (4) (the unconditional marginal effects). I find that income elasticity is significantly greater than one. Thus, as total expenditure decreases, education spending decreases more rapidly than total expenditure. The F-statistic in the corresponding first-stage regression is far above the critical value, indicating that the used instrument is relevant.

This negative effect on education spending is large compared to studies which have found the income elasticity of education spending (Qian and Smyth (2011); Huy (2012); Acar et al. (2016)). The findings of these studies suggest that the income educational expenditure elasticity is different across countries, level of family income, and other household characteristics such as parents' occupation. However, most of these studies find that the income elasticity of education spending is significantly less than one implying that education is a necessity item. For those group of household that education is a luxury good, income elasticity is less than two. I find an income elasticity of more than two. Using family income, the estimated elasticity of education spending is smaller (2.1), but still large compared to existing studies (the last column of Table 5).

Overall, after the sanctions, both the educational attainment (measured by enrollment rates and years of schooling) and investment in children's education (measured by family education spending) have decreased. First, the sanctions decreased the probability of attending college. Therefore, the years of schooling decreased. Second, spending on school tuition significantly decreased that suggests households respond to the shock by switching their children from higher-quality, more expensive private schools to lower-quality, free public schools. Reduction in children's education will reduce their future earnings such that affected children will experience a larger decline in their earnings than their parents.

5.3 Synthetic Control Method Results

As an alternative approach, I apply the synthetic control analysis and compare results with those of the first identification strategy. As explained before there is no unaffected industry. However, the

[42] Zero values for education spending are also included. As explained before, since I used this variable in log form, I added one to values of this variable and then log-transformed.

effect of the sanctions is different across industries (Figure 9). For the main analysis, I compared the most affected households (in oil and gas industries) with the least affected households (in water supply and information industries). An alternative approach is using a weighted combination of industries that are less affected by the sanctions as the control group (Agriculture, Water supply, Accommodation and food service, Information, and Human health and social work activities). The optimal weights that maximize the similarity between the synthetic control and the treated households are positive for three industries water supply, information, and health with values 0.864, 0.103, and 0.034 respectively and take value zero for the other potential controls.

Table 6 reports the effects on family income and education outcomes using synthetic control method (SCM). As the Table shows, the total income of families that the head works in the oil and gas industries decreased by 12% compared to households in the synthetic control. The sanctions decrease college enrollments by 5.2 percentage point. The years of schooling significantly decreased by 0.3 years (0.5 years for children age 15-24 who completed grade 9) after the sanctions. Also, spending on children's education significantly decreased by 57%. Overall, the results using SCM are close to the results from the first identification strategy, mostly because the synthetic control is similar to the control group in the previous sections. These SCM results provide a level of confidence to the previous results.

6 Robustness Checks

I consider several robustness checks of the main results from both identification strategies. First, I analyze whether considering different periods (1995-2015) and excluding the years 2007 and 2009 affects the results. Then, I compare the results with and without control variables. My results pass these robustness tests. In appendix D, I conduct several other sensitivity tests (using different selected donor industries, different matching variables, different matching year range, different methods for selecting weights, in-time and in-place placebos) to assess the robustness of results using SCM. Overall, these sensitivity tests verify the robustness of the original synthetic. Finally, I discuss whether the estimated effects are related to the sanctions or other changes in economic and political factors.

For the main analysis, I restrict the data to 1995 and 2013 and exclude the negotiation years (2014 and 2015) because the end of sanctions might be expected by Iranian people when Iran and P5+1 started negotiation over the nuclear program in 2013. I re-conduct the analysis using a different period including 2014 and 2015 and found the signs of the coefficients and significance are all the same (Figure 4). I also consider the robustness of my results by excluding the years 2007 and 2009. First, I exclude the first year of the sanctions, the year 2007, because Iran could have come up with some ways to avoid sanctions after the first year when sanctions imposed unexpectedly. The results are not sensitive to this change. Second, I exclude 2009 because the 2009 presidential election (in Iran and the US) could affect the Iranian economy. The signs of the coefficients and significance are all the same. The election results are unlikely to change the long run economic trend largely

because Ahmadinejad's policies in the second term were similar to his policies in the first term

Finally, I perform an analysis excluding covariates altogether to compare the results with and without control variables. The idea is that if the results are not affected, successful randomization would be confirmed. The outcome of this exercise is not significantly different from the baseline model. Overall, these sensitivity tests verify the robustness of the original results.

Other Factors

To make sure the estimated effects are solely due to the sanctions, I check whether there were other changes in economic (including Great Recession and oil price changes) or political factors that affected the treated and control groups differently.

First, I discuss two events (Great Recession and oil price changes) that can affect the time trend of the treated and control groups differently. While the sanctions period (2007-2013) includes the Great Recession of 2008-2009, Iran's economy experienced few effects from the global recession because as a result of economic sanctions Iran had been a closed economy. The other important factor is oil prices. The Iranian economy is vulnerable to fluctuations in oil prices (Farzanegan and Markwardt (2009); Berument et al. (2010)). However, oil prices were steadily rising from $50 to $80 during sanctions, except for a spike followed by a sharp drop. Thus, I assume that there are no significant events that affect the time trend of the sample groups differently.

Finally, I show during the sanction years no major political changes took place. As Borszik (2016) shows economic sanctions did not weaken the Iranian regime. In Iran, the Supreme Leader, who ranks above the President, is the ultimate political and religious authority, and sets the national course. From 2005 to 2013, Ahmadinejad was the president who had adopted the same policies consistent with the Supreme Leader strategic preferences. While Iran's nuclear program was stopped in 2002, Ahmadinejad, shortly after taking office, announced the restarting of uranium enrichment activities. These policies led to the economic sanctions (Meier (2013)).

Although there were no major changes in Iran's policies between 2005 and 2013, sanctions led to some political changes in 2013. As a result of such adverse economic impacts of the sanctions, the political elite agreed that the nuclear strategy needs to be revised (Borszik (2016)). On June 2013, the moderate Hassan Rouhani won the presidential election. President Rouhani's campaign promised to improve economic growth and unemployment. He also emphasized the need to negotiate with the Security Council over nuclear program by highlighting the negative effects of the UN sanctions on Iran's economy. As a result of such adverse economic impacts of the sanctions, the political elite agreed that the nuclear strategy needs to be revised. President Rouhani and his team were successful in finalizing the nuclear deal and terminating the sanctions.

7 Heterogeneous Effects of the Economic Sanctions

In this section, I examine whether the effects of 2006 economic sanctions are heterogeneous across different contexts. The estimates results in Section 5 show the average impact of the sanctions. How-

ever, these effects could also be heterogeneous across demographic groups. Finding heterogeneous effects is important to understand the distribution of the costs associated with the sanctions. Thus, I can determine the groups of children who are more vulnerable to the changes from the sanctions.

7.1 Heterogeneous Effects on Family Income

I first examine how the sanctions affected family income across different quantiles. Based on results in Section 5, the total income of families that the head works in the oil and gas industry on average decreased by 10%. Table 7 (and Figure 6) presents estimated coefficients from OLS and quantile regression for family income. As this Table shows, the effect of sanctions on income of middle-income household is larger (18%) and significantly different from the average effect (OLS coefficient=10%). The effect for low-income household is not significantly different from the average effect. There is no significant effect on income of household who are rank above the 75th percentile.

7.2 Heterogeneous Effects on Children's Education

I also find the impact of sanctions on children's education (enrollment, years of schooling, and education spending) across different contexts (by age, family financial resources, and structure). Table 8 presents estimates of the effects of the sanctions on the enrollment rate by crucial ages. Age plays an important role in the enrollment. The crucial ages for children's enrollment/dropout rates are at the entrance to the first grade (6 years old), high school dropout age (16 years old) and matriculation at a university (18 and 19 years old). As this Table shows, the economic sanctions increased the probability of dropping out from high school. The enrollment rate of children at high school dropout age (16 years old) decreased by 12%. Moreover, the economic sanctions decreased the probability of attaining college at age 18 and 19 by 37%. Lack of access to financial resources for post-secondary education prevents marginal students from making such investments (Bound and Turner (2007); Zimmerman (2014)). Consequently, some students may perceive a reduced benefit from a high school degree if they are unable to access post-secondary education.

To further explore heterogeneity in the effects of the sanctions, individuals are grouped based on their family financial resources (as measured by family wealth and family nonlabor income). I approximate wealth using an asset index based on Filmer and Pritchett (1999) that aggregates various assets of a household relying on principal component analysis (PCA). In HIES, respondents were asked about their ownership of durable goods (e.g. car, bicycle, TV, radio) and housing ownership and characteristics (e.g. size, number of rooms, and appliances). I use this information as asset indicators to construct an asset index. To drive weights, I use principal component analysis. I also group individuals based on their family non-labor income which is summation of the non-labor income of each member of a household including financial transferred aids, real estate incomes, subsidies, interest on bank deposits, bonds yield and share dividends, scholarships and cash gifts from others. Table 23 in the appendix E shows that the wealth index and non-labor income (and their components) are not affected by the sanctions.

Table 9 presents the effects on years of schooling and education spending over the wealth and non-labor income distributions. As this table shows, only children from poor families experienced a reduction in the years of schooling. Children from the 25th percentile (in total family wealth and non-labor income) experienced 0.2-0.5 years decrease in years of schooling. This effect is not significant for children from families with middle and high level of financial resources. I also find parents of children from middle class families (in wealth and non-labor income) spent less on their children's education by 72% and 88%. The effect is not significant for children from low and high wealth families. Low wealth families are less likely to spend money on education even before the sanctions, for example, most of these children go to public schools.[43] Overall, children from low wealth families are more affected in term of the educational attainment, and children from middle wealth families are more affected in term of investment in education. The sanctions have no significant effect on the education of children whose family rank above the 75th percentile.

I also look at the effect by mother's employment and income. There are numerous ways that maternal employment may affect children's education. First, maternal working brings more income to the family, which can be used to spend on children's education. Second, mothers who have income have more bargaining power on the decision regarding the children's education. Third, maternal employment may increase children's education if working mothers serve as role model. Last, all else equal, a working mother will spend less time with her child than one who does not work. Depending on the quality of mother-child time together and the quality of the alternative, this may either improve or decrease a child's education. To explore this heterogeneity, I estimate Eq (1) separately for individuals in different groups based on their mother's employment and income. Table 10 presents the results of these estimations. As the first and second columns show, for children whose mother is not employed, school enrollment and college attendance decreased by 4.5% and 24% respectively after the sanctions. I also find a 45% reduction in education spending among this group of children. The effect of sanctions is insignificant on the education of children whose mother has a job. Since mothers can have income from other sources than wages and salaries, in the two last columns, I show the effect of mother's income on children's education. The results are the same: for children whose mother's income is zero, school enrollment and college attendance decreased by 4.5% and 24% respectively after the sanctions. Moreover, education spending decreased by 41%. The effect of sanctions is insignificant on enrollment of children whose mother has a positive income.

In sum, the sanctions had a negative effect on children's education, and the effect is larger for children at crucial ages, children from low and middle wealth families, and children's whose mother has no income.

[43] While middle and high wealth households spent an average of 26 (2% of their total consumption) and 83 (3%) thousand Rials on education in 2006 respectively, households in the lowest wealth quantile spent only 4 thousand Rials on education (0.4% of their total consumption).

8 Conclusion

This paper analyzes the welfare effects of economic sanctions on current generation (through changes in labor income) and next generation (through changes in children's education and their lifetime earnings). Economic sanctions either as a prelude or best alternative to warfare found new prominence in the 21st century. Recent evidence has indicated that economic sanctions pose significantly adverse impacts on the current generation. While the short term effects of economic sanctions on the current generation are well explored, little is known about their long lasting effects on the next generation. This paper seeks to fill the gap by examining the effects of UN economic sanctions against Iran on children's education. The targeted sanctions were associated with large, sudden reductions in households' income that last for seven years.

Relying on a difference-in-difference approach and using a sub-sample of data on the Iranian Households' Income and Expenditure (oil and gas industry as the treated group, water supply and information industries as the control group), the empirical analysis suggests that the sanctions had a significant negative impact on the family income and children's education. The analysis reveals two findings. First, the sanctions decreased children's probability of attending college by 8.7 percentage points and years of schooling by 0.2 years. Second, households reduced expenditure on children's education by 61% - particularly on expenditure for school tuition. This finding indicates that households respond to the shock by substituting away from higher-quality private schools towards lower-quality public schools for their children. The sanctions impact on children's education is larger than implied by the income elasticity estimates from the previous literature likely because sanctions have persistent effects on parent income. Overall, after the sanctions, both educational attainment and investment in education have decreased. Reduction in children's education will reduce their future earnings (by 41%) such that affected children will experience a larger decline in their earnings than their parents.

This paper also investigates the cause of the heterogeneity. I find that the negative effect of the sanctions on children's education is larger for children at crucial ages, children from low and middle wealth families, and children's whose mother has no income. First, the enrollment rate of children at high school dropout age (16 years old) and matriculation at a university (18 and 19 years old) decreased by 12% and 37% respectively. Moreover, only children from poor families experienced a reduction in the years of schooling (0.2-0.5 years of schooling). Second, I find parents of children from middle wealth families spent less on their children's education by 72%. The effect is not significant among children from high wealth families. Third, while the effect of sanctions is insignificant on the education of children whose mother has income, there are negative effects on school enrollment, college attendance, and education spending among children whose mother has no income.

This paper completes the literature documenting the negative effects of economic sanctions. Current studies show the negative effects of sanctions on economic growth and living standards and humanitarian situation of the civilian population during the years of sanctions. In the case of Iran, Iran's economy got 15 to 20% smaller than it would have been absent the sanctions (U.S. Treasury Secretary Jacob Lew report, 2015). Moreover, previous studies find adverse impacts of the 2006 UN

sanction on the current generation by showing a reduction in the total welfare level of final consumers (Ezzati and Salmani (2017)) and public health (Karimi and Haghpanah (2015)). My results go beyond these studies and show that economic sanctions have long lasting consequences on children's well-being even after they are lifted by a reduction in children's education. Moreover, human capital is an important factor in productivity growth and economic development. These negative externalities caused by disinvestment in human capital are not documented in the current literature of adverse consequences of economic sanctions. I find that the costs to the society associated with the reduction in earnings after the sanctions total about 18% of GDP. 45% of this reduction comes from decreased earning for the current workers, and 55% comes from decreased earning for the next generation. It suggests that the cost estimates using only earnings of current generation may only capture less than half of the overall cost. This paper also adds to the literature on the effect of family income on children's education. I find larger effects compared to previous studies because the income shock is persistent and large. Moreover, other financial resources had not been available to children during the years of sanctions. The estimates presented in this paper suggest that although economic sanctions against Iran were successful in term of political goals, such negative effects on human development are not ignorable. The effect of sanction on children's education depends on the context and severity of the sanctions and how government and households cope with this shock. However, establishing this potential negative shock to human development can edify future policy regarding the use of economic sanctions.

There are several worthwhile directions for future research. First, a structural model incorporating different features of the sanctions may offer other policy counterfactual implications. Second, estimating the impacts of the lifting sanctions on children's education using the data for years after the lifting of the sanctions would be interesting. The households' responses to positive and negative changes in income may be asymmetric. Third, it would be fruitful to estimate the long term effects of the sanctions on labor market outcomes of affected children.

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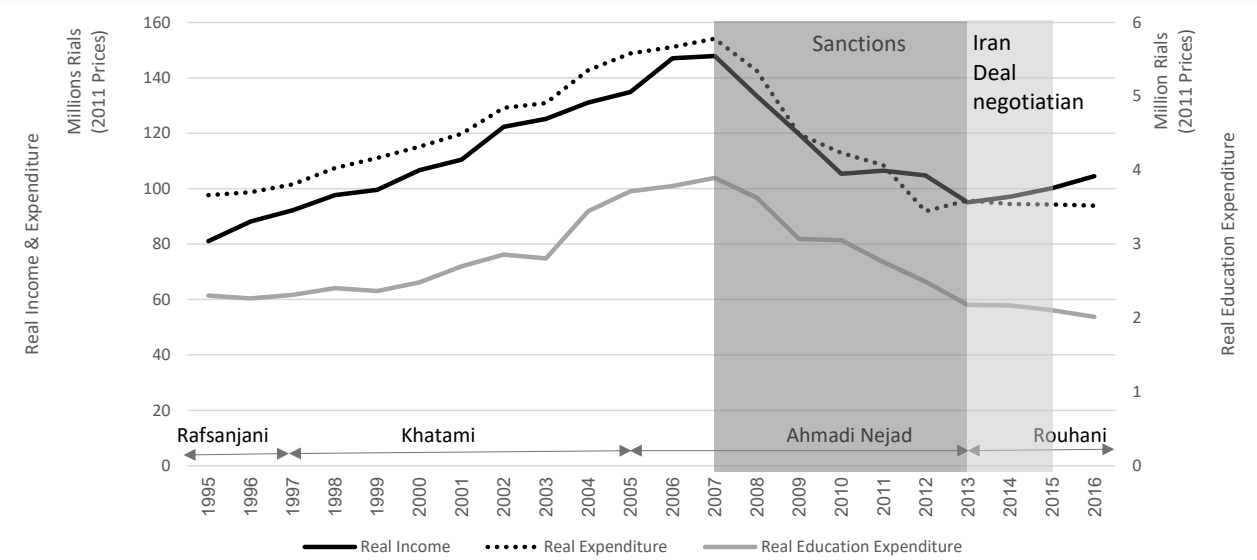
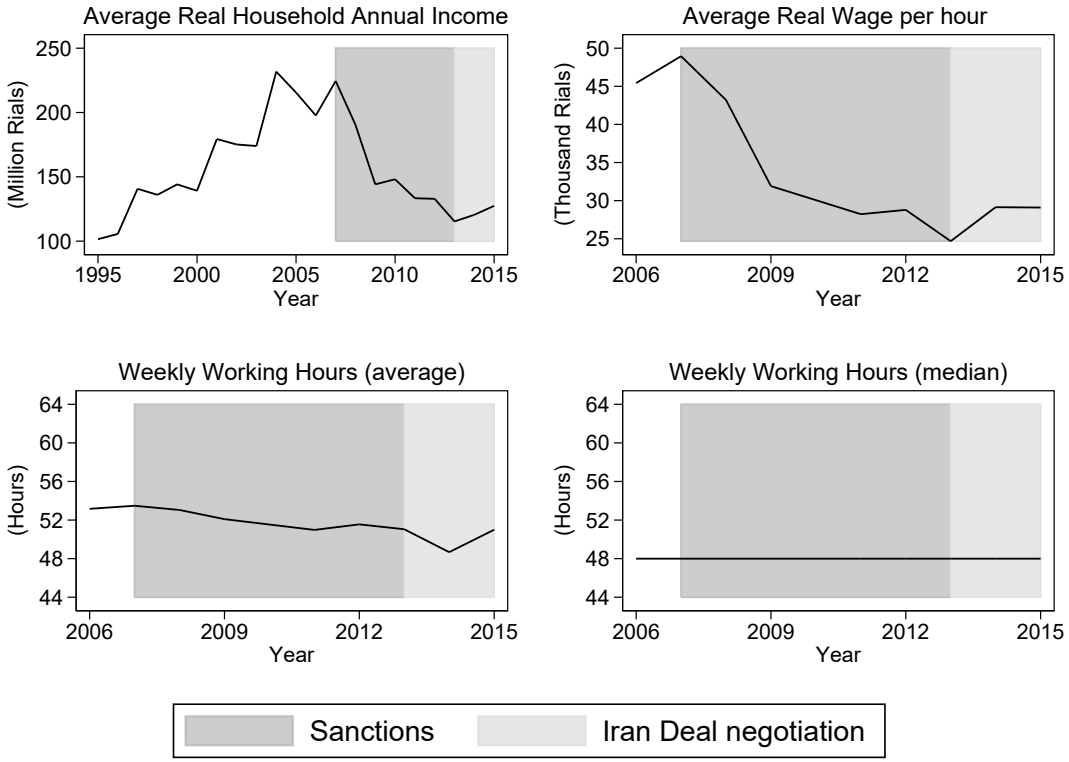
A Figures

Figure 1: Average Real Income, Expenditures and Education Expenditures for Iranian Household



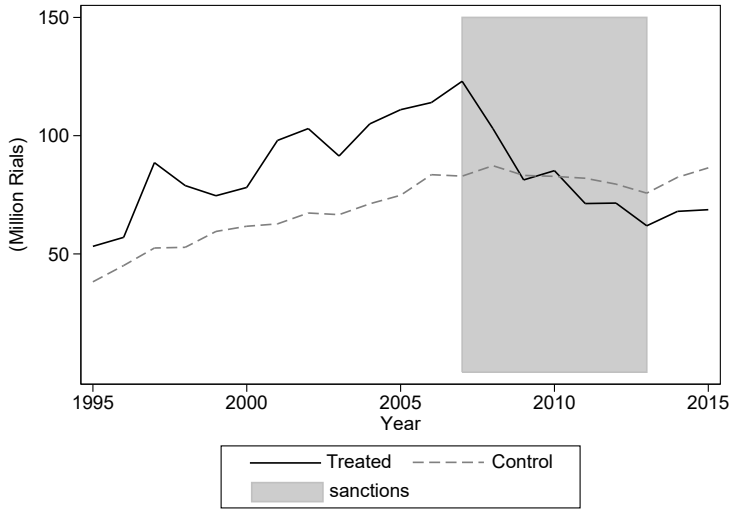
Note: Figure shows the decreases in average real annual income, expenditures and education expenditures for Iranian household during the economic sanctions.
Source: Author's calculations from HEIS data.

Figure 2: Household Income, Wage Rate and Working Hours in Oil and Gas Industry



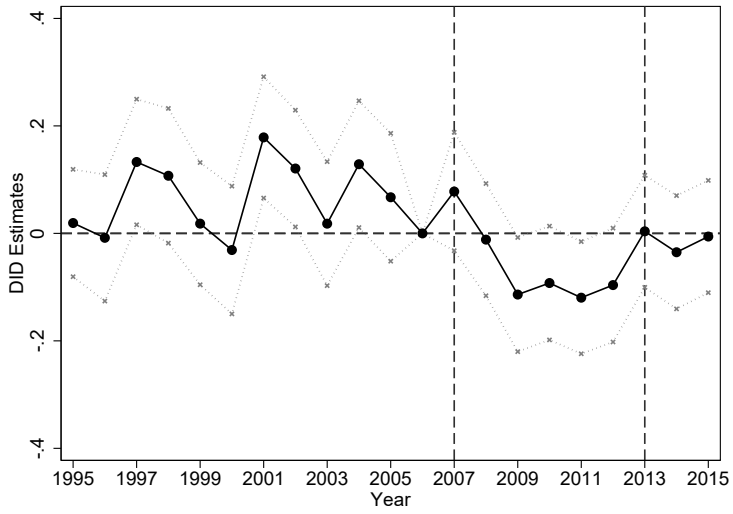
Note: Figure shows the reason of reduction in household income is a decrease in average wage rate in the oil and gas industry. The average real income of households that the head works in the oil and gas industry decreased from 198 to 115 million Rials (decrease by 42%). The average real wage per hour in this industry decreased from 45 in 2006 to 25 thousand Rials in 2013 (decrease by 45%). At the same time, the average working hours was constant around 50 hours per week. Moreover, Median of working hours was always 48 hours per week.
Wage rate and working hours were not asked before 2006.
Source: Author's calculations from HEIS data.

Figure 3: Real Wage and Salary Income



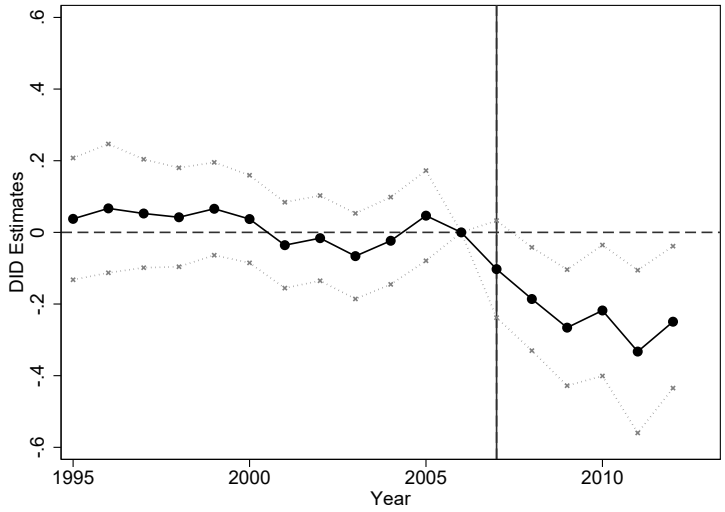
Note: Figure shows the trends in head's real wage and salary income over the sanctions among Oil and Gas industry (as Treated) and Water supply and Information industries (as control group).
Source: Author's calculations from HEIS data

Figure 4: Dynamic Effects on Labor Income



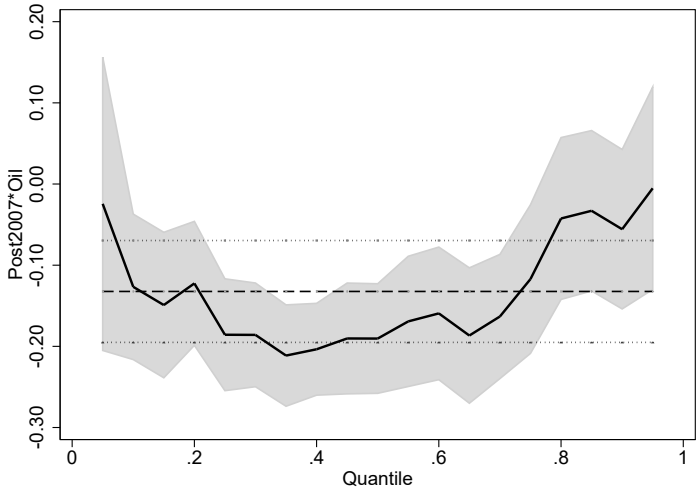
Note: Figure shows the DID estimates of dynamic effects on family labor income (wage and salary) (coefficients of the interaction $year \times Oil$ in Eq (2), with 95-percent confidence interval). The Dependent variable is log transformed, and have been deflated by CPI which equals 100 in year 2011. While the estimated coefficients are not significantly different from zero before 2007, they turn significantly negative after the sanction imposed in 2007. Treatment group: Oil and Gas industry, control group: Water Supply and Information industries.

Figure 5: Dynamic Effects on Years of Schooling



Note: Figure shows the DID estimates of dynamic effects on the years of schooling (coefficients of the interaction $year \times Oil$ in Eq (2), with 95-percent confidence interval). While the estimated coefficients are not significantly different from zero before 2007, they turn significantly negative after the sanction imposed in 2007.
Treatment group: Oil and Gas industry, control group: Water Supply and Information industries.

Figure 6: Heterogeneous Effects on Labor Income



Note: Figure shows the heterogeneous effects on labor income (coefficients of $Post2007 \times Oil$ in Eq (1), with 95-percent confidence interval). The Dependent variable (labor income) is log transformed, and have been deflated by CPI which equals 100 in year 2011. The horizontal dash line shows the OLS coefficient (average effects of sanctions on labor income). The solid line shows quantile coefficients.
Treatment group: Oil and Gas industry, control group: Water Supply and Information industries.

B Tables

Table 1: Effect on Family Income

	Real Family Income		Real	Weekly
	Total Income	Labor Income	Wage Rate	Working Hours
Post2007 × Oil	-0.10***	-0.13**	-0.12**	-0.21
	(0.03)	(0.03)	(0.00)	(1.04)
Oil	0.20***	0.25***	0.26***	0.19
	(0.02)	(0.02)	(0.04)	(0.89)
R-squared	0.13	0.14	0.12	0.01
Observations	5,335	5,334	2,773	2,776

Notes: This table presents estimated coefficients from a linear model for weekly working hours, wage rate and household's income. Dependent variables (wage, labor income and total income) are log transformed, and have been deflated by CPI which equals 100 in year 2011. The time period is 1995-2013. Weekly working hour was not asked before 2006. Thus, it is not possible to find wage rate for years before 2006. Heteroskedasticity-consistent standard errors accounting for clustering at the province and industry level in parentheses. P-values are calculated using wild bootstrap randomization inference (WBRI). *Significant at 10% level; **significant at 5% level; ***significant at 1% level. Treatment group: Oil and Gas industry, control group: Water Supply and Information industries.

Table 2: Effect on Consumption Expenditure of Households

		γ			
	share of total expenditure	All Households		Households with children	
Dependent Variable	(2006)	log	share	log	share
Education	2.19	-0.61**	-0.007**	-0.58**	-0.007**
		(0.25)	(0.003)	(0.31)	(0.003)
non-Education	97.81	-0.07***	0.007**	-0.06***	0.007**
		(0.02)	(0.003)	(0.02)	(0.003)
Total Expenditure		-0.07***	-	-0.06***	-
		(0.02)	-	(0.02)	-
Observations		5,335	5,335	4,460	4,460

Notes: This table presents estimated coefficients of Post2007 $\times$ Oil (γ in Eq (1)). Dependent variables are family expenditure on education and non-education goods and services according to COICOP classification. Dependent variables have been deflated by CPI which equals 100 in year 2011. Heteroskedasticity-consistent standard errors accounting for clustering at the province and industry level in parentheses. The time period is 1995-2013. The sample for two first columns (All Households) include all households with at least one member at age 6-24 (even if this family member is wife or husband). The sample for two last columns (Households with children) include all households with at least one child at age 6-24. *Significant at 10% level; **significant at 5% level; ***significant at 1% level. Treatment group: Oil and Gas industry, control group: Water Supply and Information industries.

Table 3: Effect on Enrollment Rate and Years of Education

	Enrollment in High School II (15-18 yr old)	Attending Any College (HSG, ≤24 yr old)	Years of Education (15-24 yr old)	Years of Education (6-24 yr old)
A. No differences across gender				
Post2007 × Oil	-0.036	-0.087**	-0.407***	-0.186***
	(0.037)	(0.039)	(0.157)	(0.088)
Oil	0.025	0.012	0.117	-0.010
	(0.021)	(0.021)	(0.088)	(0.041)
R-squared	0.012	0.080	0.094	0.759
B. Allowing differences across gender				
Post2007 × Oil	-0.032	-0.119**	-0.385**	-0.349***
	(0.043)	(0.048)	(0.190)	(0.121)
Oil	0.023	0.002	0.160	0.058
	(0.027)	(0.029)	(0.115)	(0.061)
Female × Post2007 × Oil	-0.013	-0.068	-0.077	-0.035
	(0.054)	(0.054)	(0.238)	(0.155)
Female × Oil	0.004	0.017	-0.082	-0.043
	(0.037)	(0.038)	(0.157)	(0.088)
Female	-0.036	-0.044*	0.160	0.034
	(0.024)	(0.026)	(0.101)	(0.059)
R-squared	0.014	0.082	0.083	0.675
Mean	81.39	76.88	10.37	7.50
Observations	2,084	2,526	3,884	10,060

Notes: This table presents estimated coefficients from a linear model for enrollment and years of schooling. Dependent variable for the first column is being students enrolled in high school II (education is compulsory until the end of high school I or grade 9. The falsification test (Figure 7) shows no significant effect on enrollment in primary school and high school I during the sanction years). The sample for this analysis is children of the age group that officially corresponds to each level. Dependent variable for the second column is ever attending any college. The sample for this analysis is high school graduates who are aged less than 24. Dependent variable for the last two columns is years of schooling for different age groups (children aged 15 to 24, and children aged 6 to 24). The time period is 1995-2013. Heteroskedasticity-consistent standard errors accounting for clustering at the province and industry level in parentheses. In the panel B, I examine gender differences. *Significant at 10% level; **significant at 5% level; ***significant at 1% level. Treatment group: Oil and Gas industry, control group: Water Supply and Information industries.

Table 4: Effect on Education Expenditure of Households by Item

	γ			
	total spending		per child	
	log	share	log	share
School Tuition	-0.600***	-0.004**	-0.574**	-0.002**
	(0.246)	(0.002)	(0.240)	(0.001)
University Tuition	0.056	-0.002	0.002	-0.003
	(0.686)	(0.008)	(0.161)	(0.006)
Books	-0.216***	0.000	-0.126**	0.000
	(0.071)	(0.000)	(0.063)	(0.000)
Private Tutoring	-0.762**	-0.001***	-0.616*	-0.000**
	(0.348)	(0.000)	(0.362)	(0.000)

Notes: This table presents estimated coefficients of Post2007 $\times$ Oil (γ in Eq (1)). Dependent variables are log of different classes of education expenditures according to COICOP classification. All education expenditures have been deflated by Education Price Index, which equals 100 in year 2011. The sample for this analysis is children aged 6 to 24. For school spending the sample consists of all children aged 6-24 who have not graduated high school. For university spending the sample consists of children aged 6-24 who have graduated high school. For spending on books, I have considered all children aged 6-24. The time period is 1995-2013. Heteroskedasticity-consistent standard errors accounting for clustering at the province and industry level in parentheses. *Significant at 10% level; **significant at 5% level; ***significant at 1% level. Treatment group: Oil and Gas industry, control group: Water Supply and Information industries.

Table 5: Income Elasticity of Education Spending

Dependent variable: Ln (Household Spending on Education)		
	2SLS	Robustness Check
Variables	IV: Ln Expenditure	IV: Ln Income
IV	2.194**	2.120**
	(0.906)	(0.876)
Oil	-0.155*	-0.253**
	(0.085)	(0.121)
First stage		
IV: Post2007×Oil	-0.06***	-0.09***
F-stat	105.2	105.2
Log likelihood	-37940.9	-37940.9
LR chi2	2080.1	2080.1
Pseudo R2	0.0267	0.0267

Notes: This table presents estimated coefficients of Eq (4) ($lnEdu\_exp_{ipt} = \alpha + \xi lnTotal\_exp_{ipt} + \beta\ Oil_i + \lambda_t + X'_{ispt}\delta + \phi_p + \psi_s + \varepsilon_{ispt}$). Dependent variable is Ln (Household Spending on Education). Since education spending and total household expenditure are both in logarithmic form, ξ denotes elasticity. The income elasticity of education spending is significantly greater than one (2.194). Thus, as total expenditure (as a proxy for total income) decreases, education spending decreases more rapidly, indicating that education is a luxury item in the households' budget. I also use family income itself as a robustness check (the last column). Using family income, the estimated elasticity of education spending is smaller (2.1), but still large compared to existing studies. Additional controls include household size, head's age, and head's education. *Significant at 10% level; **significant at 5% level; ***significant at 1% level.

Table 6: Sanctions Effects using Synthetic Control Method (SCM)

	Family	Attending college	Years of Schooling		Education
	Income (log)	(HSG,≤24 yr old)	(15-24 yr old)	(6-24 yr old)	Expenditure (log)
Post2007 × Oil	-0.117***	-0.052**	-0.510***	-0.289***	-0.571**
	(0.023)	(0.021)	(0.171)	(0.099)	(0.273)
Oil	0.206***	0.081*	0.254***	0.105*	0.540***
	(0.015)	(0.043)	(0.096)	(0.056)	(0.158)
R-squared	0.133	0.013	0.093	0.753	0.054
Observation	10,859	1,347	7,371	13,985	10,861

Notes: This table presents estimated coefficients of Eq(1) using synthetic control method (SCM). The time period is 1995-2013. *Significant at 10% level; **significant at 5% level; ***significant at 1% level. Treatment group: Oil and Gas industry, synthetic control group: Water Supply, Information, and health industries.

Table 7: Heterogeneous Effect on Family Income

	Average Effect	Quantile Regression		
	(OLS)	at 0.25 quantile	at 0.5 quantile	at 0.75 quantile
Post2007 × Oil	-0.10***	-0.12***	-0.18^{+}***	-0.07^{+}
	(0.03)	(0.04)	(0.04)	(0.05)
Oil	0.20***	0.16***	0.21***	0.20***
	(0.02)	(0.02)	(0.02)	(0.03)
R-squared	0.13	0.07	0.08	0.08
Observations	5,335	1,778	1,778	1,777

Notes: This table presents estimated coefficients from OLS and quantile regression for family income. Dependent variable is log transformed and deflated by CPI which equals 100 in year 2011. The time period is 1995-2013. Heteroskedasticity-consistent standard errors accounting for clustering at the province and industry level in parentheses. P-values are calculated using wild bootstrap randomization inference (WBRI). The effect of sanctions on income of middle-income household is larger (18%) and significantly different from the average effect (OLS coefficient=10%). The effect for low-income household is not significantly different from the average effect. There is no significant effect on income of household who are rank above the 75th percentile. *Significant at 10% level; **significant at 5% level; ***significant at 1% level. $^{+}$Significantly different quantile regression coefficient from OLS coefficient at the 5% significant level. Treatment group: Oil and Gas industry, control group: Water Supply and Information industries.

Table 8: Heterogeneous Effect on Enrollment Rate by crucial ages

Dependent variable: School Enrollment/Attending College			
	Age		
	6 yr old	16	18,19
Post2007 × Oil	0.008	-0.116**	-0.373**
	(0.042)	(0.051)	(0.164)
Oil	-0.008	0.043*	0.300**
	(0.042)	(0.025)	(0.150)
R-squared	0.294	0.032	0.048
Observations	209	676	638

Notes: This table presents estimated coefficients from a linear probability model. The sample for this analysis is children at crucial ages. The time period is 1995-2013. Heteroskedasticity-consistent standard errors accounting for clustering at the province and industry level in parentheses. *Significant at 10% level; **significant at 5% level; ***significant at 1% level. Treatment group: Oil and Gas industry, control group: Water Supply and Information industries.

Table 9: Heterogeneous Effect on Education by percentiles of Family Resources

	Family Wealth			Family non-labor Income		
	25th	50th	75th	25th	50th	75th
A. Years of Schooling						
Post2007×Oil	-0.214**	-0.112	-0.097	-0.523**	-0.144	-0.004
	(0.079))	(0.190)	(0.174)	(0.196)	(0.118)	(0.163)
Oil	-0.089	-0.010	-0.042	-0.039	-0.055	-0.057
	(-0.055)	(0.090)	(0.077)	(0.083)	(0.055)	(0.084)
R-squared	0.786	0.712	0.773	0.720	0.770	0.804
B. Education Spending						
Post2007×Oil	-0.635*	-0.718***	-0.279	-0.635*	-0.877***	-0.145
	(0.382)	(0.270)	(0.418)	(0.446)	(0.263)	(0.346)
Oil	0.501**	0.091	0.148	0.341**	0.317**	-0.12
	(0.232)	(0.171)	(0.230)	(0.252)	(0.158)	(0.226)
R-squared	0.037	0.023	0.035	0.025	0.045	0.050
Observations	2,570	5,281	2,508	2,505	5,414	2,440

Notes: This table presents estimated coefficients from a linear model. Dependent variables (total income and non-labor income are log transformed, and have been deflated by CPI which equals 100 in year 2011. The sample for this analysis is children aged 6 to 24 (children age 6 who born at the start of the academic year (September 23th) or later are excluded because they are not eligible to enroll in school). The time period is 1995-2013. I control for age and age-squared effects for estimating years of schooling. Heteroskedasticity-consistent standard errors accounting for clustering at the province and industry level in parentheses. *Significant at 10% level; **significant at 5% level; ***significant at 1% level. Treatment group: Oil and Gas industry, control group: Water Supply and Information industries.

Table 10: Heterogeneous Effect on Education
by Mothers Activity and Income

	Mother's Employment		Mother's Income	
	employed	non-employed	positive	zero
A. School Enrollment				
Post2007 × Oil	-0.037	-0.045**	-0.063	-0.045**
	(0.062)	(0.020)	(0.059)	(0.021)
Oil	-0.011	-0.003	-0.022	0.000
	(0.025)	(0.010)	(0.024)	(0.010)
B. College Attendance				
Post2007 × Oil	-0.085	-0.239**	-0.111	-0.242**
	(0.125)	(0.104)	(0.122)	(0.112)
Oil	0.122	0.233**	0.111	0.241**
	(0.123)	(0.095)	(0.122)	(0.104)
C. Education Spending				
Post2007 × Oil	-0.240	-0.445***	-0.262	-0.413***
	(0.545)	(0.136)	(0.478)	(0.138)
Oil	-0.107	0.255***	0.059	0.256***
	(0.175)	(0.058)	(0.168)	(0.058)
Observations	1,223	7,576	1,486	7,313

Notes: This table presents estimated coefficients from a linear probability model. The sample for this analysis is children aged 6 to 24 (children age 6 who born at the start of the academic year (September 23th) or later are excluded because they are not eligible to enroll in school). The time period is 1995-2013. Heteroskedasticity-consistent standard errors accounting for clustering at the province and industry level in parentheses. *Significant at 10% level; **significant at 5% level; ***significant at 1% level. Treatment group: Oil and Gas industry, control group: Water Supply and Information industries.

Table 11: Effect on Family Savings and non-Labor Income

	Savings		Debt		non-Labor Income	
	log	share	log	share	log	share
Post2007 × Oil	-0.12*	0.00	0.19	-0.12	0.29	0.02*
	(0.07)	(0.01)	(0.24)	(1.6))	(0.19)	(0.01)
Oil	0.29***	0.04***	0.32**	0.15	0.16	-0.03***
	(0.0))	(0.01)	(0.15)	(1.00)	(0.12)	(0.01)
R-squared	0.08	0.04	0.23	0.08	0.03	0.03
Observations	4,221	4,221	1,114	1,114	5,335	5,335

Notes: This table presents estimated coefficients from a linear model for household's savings, debt and non-labor income. Dependent variables have been deflated by CPI which equals 100 in year 2011. The share values are share of total family income. The sample for analysis of savings/debt is only those households that have positive savings/debt. Heteroskedasticity-consistent standard errors accounting for clustering at the province and industry level in parentheses. The time period is 1995-2013. *Significant at 10% level; **significant at 5% level; ***significant at 1% level. Treatment group: Oil and Gas industry, control group: Water Supply and Information industries.

C Appendix: Identification Assumptions

In this appendix, I provide analysis on the validity of identification assumptions. The coefficient γ in Eq (1) is the DID estimate of the primary interest because it captures the average effect of the economic sanctions on the treated group relative to the comparison group. This estimation method requires several identifying assumptions. First, the key identifying assumption is that treatment and control groups are comparable. Treatment and control groups that differ on observables should not be directly compared (LaLonde (1986); Heckman et al. (1998)). Therefore, I check the trends in real family income in the absence of the sanctions. I conducted a placebo test by allowing a placebo treatment in all years before the actual timing of the sanction implementation. I use households where the head works for different industries as different control groups. Table 12 reports the results of the Wald test ($H0 : \gamma = 0$). If the estimate is different from 0, the trends are not parallel. As this table shows, 12 potential control groups satisfy common trend assumption: households which the head work in agriculture, manufacturing, water supply, construction, wholesale and retail, transportation, food service, information, real estate activities, administrative and support, art, and other service activities.

Second, the sanctions could not influence control group. Based on the sanctions documents, only people who work for the oil and gas industry were directly affected by the sanctions. However, sanctions indirectly affected many sections of Iran's economy through the government budget and the exchange rates because Iranian economy is highly vulnerable to revenue from oil exports. Most of those 12 potential control groups are an inadequate comparison group because they indirectly were affected by the sanctions. However, the effect of the sanctions is different across industries: (1) oil and gas industry directly affected by export and financial limitations caused by the sanctions, (2) the export-oriented industries and the industries that have foreign rivals benefit from the increase

Table 12: Effect on Family Income using Placebo Treatment Years (Wald test: $H0 : \gamma = 0$)

	Fake Treatment Year										
Control Group	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006
All Other Sections (non-oil)	0.97	0.11	0.04	0.06	0.32	0.77	0.33	0.08	0.03	0.01	0.00
Agriculture	0.89	0.13	0.42	0.12	0.35	0.12	0.14	0.08	0.13	0.41	0.93
Manufacturing	0.83	0.95	0.27	0.59	0.83	0.77	0.91	0.44	0.22	0.05	0.06
Electricity supply	0.01	0.01	0.00	0.00	0.01	0.04	0.02	0.00	0.00	0.00	0.00
Water supply	0.48	0.25	0.36	0.12	0.28	0.81	0.24	0.11	0.08	0.07	0.19
Wholesale & Retail	0.11	0.32	0.27	0.36	0.32	0.14	0.46	0.91	0.65	0.21	0.17
Transportation	0.13	0.26	0.19	0.25	0.39	0.26	0.31	0.88	0.98	0.35	0.17
Food service	0.16	0.47	0.40	0.27	0.15	0.12	0.11	0.39	0.53	0.31	0.15
Information & Communication	0.85	0.35	0.59	0.10	0.07	0.20	0.09	0.07	0.05	0.06	0.09
Financial	0.00	0.02	0.00	0.00	0.00	0.04	0.02	0.00	0.00	0.00	0.00
Real estate activities	073.	0.12	0.54	0.18	0.25	0.71	0.56	0.24	0.16	0.04	0.12
Administrative & Support	0.61	0.24	0.18	0.20	0.40	0.35	0.23	0.24	0.32	0.41	0.69
Social Security	0.01	0.01	0.00	0.00	0.00	0.03	0.00	0.00	0.00	0.00	0.00
Education	0.00	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Health	0.00	0.00	0.00	0.00	0.00	0.01	0.01	0.00	0.01	0.00	0.00
Arts, Entertainment & Recreation	0.29	0.38	0.35	0.21	0.85	0.80	0.51	0.12	0.09	0.01	0.09
Other Service Activities	0.20	0.31	0.26	0.32	0.22	0.28	0.57	0.95	0.60	0.14	0.06
Households as employers	0.37	0.37	0.44	0.62	0.54	0.23	0.03	0.01	0.00	0.08	0.27
Central Offices	not enough observations										
Extraterritorial Organizations	not enough observations										

Note: This table presents the effect of sanctions on family income using a placebo treatment in years different from the actual timing of the sanctions implementation and using different control groups for regressing Eq (1). The treated group is households in which the head works for the oil and gas industry. For example, by using the year 2000 as the fake treatment year and water supply industry as the control group, I can check whether the real family income of households in which head works in oil and gas industry and households in which head works in water supply industry were similar in year 2000. This table shows the p-values of the Wald test ($H0 : \gamma = 0$). If p-value is more than the α level (0.05), the results are not significant and I cannot reject the null hypothesis. Thus, the trends are parallel. Therefore, there are 12 potential control groups (gray rows) that satisfy common trend assumption: households which the head work in agriculture, manufacturing, water supply, construction, wholesale and retail, transportation, food service, information, real estate activities, administrative and support, art and other service activities.

in the exchange rate as a result of sanctions (agriculture, food and all most services sectors), (3) the industries that need to import raw materials suffer from import restrictions and the increase in the exchange rate (manufacturing industries), (4) the construction industry is one of the most affected industries by an oil income shocks in Iran. After the oil and gas industry, the construction industry was the first industry that experienced a negative growth rate of value added during the sanctions.[44] While most industries were affected by the sanctions, there were some industries that do well no matter what is happening with the economy. For instance, while workers in many industries experienced a reduction in their real wage, the wage of workers in water supply and information industries have not changed (Figure 3). At the same time, workers in oil and gas industry experienced a large and persistent shock to earnings.[45] Moreover, as Table 13 shows in the absence of the sanctions, trends in family income, family expenditure and education outcomes (enrollment rate and years of schooling) are parallel for these two groups (oil and gas industry as the treated group, water supply and information industries as the control group). These results also suggests no anticipatory effect. If the economy responds to the sanction before its implementation, the estimated effects could, at best, serve as a lower bound. As I mention in the Section 2.1, since the UN Security Council did not show any action after ultimatum on stopping Iran's nuclear program in August 2006, imposing the sanction in December 2006 was unexpected. Moreover, estimated dynamic effects (Eq (2)) confirms that there is no anticipatory effect (Figures 4 and 5).

I also check household's and children's characteristics in the absence of the sanctions. Table 14 reports summary statistics for the full sample and separately by treatment status, as well as tests of treatment-control balance. The variables overall are well balanced between the control and the treatment groups. Although households in the treated group used to be richer before the sanctions, the trends were parallel. Formal tests suggest that randomization was successful: the p-value for the F-test that characteristics jointly predict treatment is 0.89. Tests for each individual baseline covariate also do not reject equality of means for treatment and control groups (column 6). Also, Table 15 shows the share of education spending to each item before the sanctions for the full sample and separately by treatment status, as well as tests of treatment-control balance. The variables overall are well balanced between the control and the treatment groups. Thus, the group of households in which the head works in water supply and information sectors is an adequate comparison group.

Third, sanctions should not have any effects on outcomes that are not supposed to be affected by the treatment. In Iran, education is compulsory until the end of high school one or grade 9. Therefore, nonsignificant effect on enrollment in these grades can interpret as the falsification

[44]The average growth rate for oil value added is -6.4% during the years of the sanctions. The growth rate in this sector has reached an all-time low of -37% in 2012. The average growth rate for agricultural value added and service value added are 4.6% and 3.7% respectively during the years of the sanctions. The value added of manufacturing industries decreased by 8.5% and 4% in 2011 and 2012 respectively. Although, at first, the 2006 sanction was a positive shock on the construction industry, the growth rate for construction value added became -3.2% in 2009 and remained at this level until 2013.

[45]In fact, nominal wages have been increasing for most industries during the years of sanctions. However, the inflation adaption varies across industries. While some industries such as water supply and information industries fully adapted to the inflation, lack of adaption in the oil and gas industry caused a reduction in the real wage.

Table 13: Effect on Outcome Variables using Placebo Treatment Years
(Wald test: $H0 : \gamma = 0$)

	Fake Treatment Year										
Dependent Variable	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006
Family Income	0.52	0.55	0.19	0.20	0.36	0.62	0.19	0.13	0.31	0.27	0.21
Family Expenditure	0.10	0.41	0.36	0.29	0.45	0.54	0.61	0.58	0.39	0.39	0.40
Education Expenditure	0.58	0.98	0.61	0.49	0.68	0.90	0.19	0.19	0.17	0.24	0.22
School Enrollment (children aged 6-18)	0.65	0.21	0.43	0.65	0.99	0.42	0.96	0.37	0.37	0.45	0.28
College Enrollment (children aged 19-24)	0.65	0.25	0.33	0.17	0.12	0.17	0.12	0.37	0.27	0.30	0.87
Years of schooling (children aged 6-24)	0.54	0.57	0.62	0.40	0.62	0.62	0.82	0.15	0.18	0.70	0.33

Note: This table presents the effect of sanctions on outcome variables e,g. the real family income using a placebo treatment in years different from the actual timing of the sanctions implementation and using the oil and gas industry as treated group and the water supply and information industries as control group for regressing Eq (1). For example, by using the year 2000 as the fake treatment year, I can check whether the outcome variables e,g. the real family income of households were similar across the group of households. Family income and expenditure are log transformed, and have been deflated by CPI which equals 100 in year 2011. For education outcomes, the sample is households with children aged 6 to 24. Education expenditure is also log transformed, and has been deflated by Education Price Index (EPI) which equals 100 in year 2011. This table shows the p-values of the Wald test ($H0 : \gamma = 0$). If p-value is more than the α level (0.05), the results are not significant and I cannot reject the null hypothesis. Thus, the trends are parallel. As this table shows, in the absence of the sanctions, trends in family income, family expenditure and education outcomes (enrollment rate and years of schooling) are parallel.
Treated group: oil and gas industry, control group: water supply and information industries.

Table 14: Mean, Standard Deviations, and Tests of Treatment-Control Covariate Balance Before the 2006 UN Economic Sanctions

	(1) All	(2) Control	(3) Treatment	(4) Synthetic Control	(5) Diff (2)-(3)	(6) H0:diff=0 (p-value)
Household-level variables						
% Family with a male head	97.86	97.68	98.09	95.29	-0.41	0.72
Head's years of schooling	10.80	10.87	10.73	10.87	0.14	0.32
	(3.82)	(3.74)	(3.83)	(4.31)		
Spouse's years of schooling	9.29	9.33	9.23	9.19	0.11	0.43
	(3.66)	(3.74)	(3.56)	(3.94)		
Family size	5.15	5.13	5.17	5.17	-0.04	0.52
	(1.84)	(1.75)	(1.91)	(1.85)		
Total Family income (Millions Rials)	137.28	121.47	156.03	122.88	-34.56	0.00
	(115.70)	(76.33)	(147.35)	(89.90)		
Family Labor income (Millions Rials)	87.48	74.03	103.42	78.26	-29.39	0.00
	(92.33)	(44.49)	(125.80)	(60.67)		
Family Education Expenditure (Millions Rials)	2.96	2.46	3.56	2.28	-1.09	0.00
	(7.24)	(5.24)	(9.01)	(5.21)		
Observations	2,741	1,487	1,254	4,303		
Child-level variables ($6 \leq age \leq 24$)						
Age (female)	14.15	14.10	14.20	14.19	-0.09	0.55
	(4.50)	(4.49)	(4.52)	(4.49)		
Age (male)	14.41	14.27	14.54	14.50	-0.26	0.12
	(4.73)	(4.76)	(4.68)	(4.73)		
% In school: girls	83.94	84.12	83.76	81.27	0.36	0.79
% In school: boys	81.78	82.14	81.41	80.40	0.73	0.61
% In school: girls 6-17	97.16	97.19	97.13	96.39	0.05	0.94
% In school: boys 6-17	97.16	97.09	97.22	95.96	-0.13	0.84
Girls' years of schooling	7.58	7.57	7.59	7.53	-0.02	0.83
	(3.74)	(3.75)	(3.74)	(3.67)		
Boys' years of schooling	7.44	7.34	7.54	7.41	-0.19	0.13
	(3.63)	(3.68)	(3.58)	(3.61)		
Observations	5,800	2,897	2,903	11,100		

Notes: Table reports summary statistics for the full sample and by treatment status. Standard deviations are in parenthesis in columns (1)-(3). The forth and fifth columns contain differences in means between the control and the treatment samples and t-tests of these differences. Tests do not reject equality of means for treatment and control groups.

Treatment group (column 3): Oil and Gas industry, control group (column 2): Water Supply and Information industries, synthetic control (column 4): Water Supply, Information, and health industries.

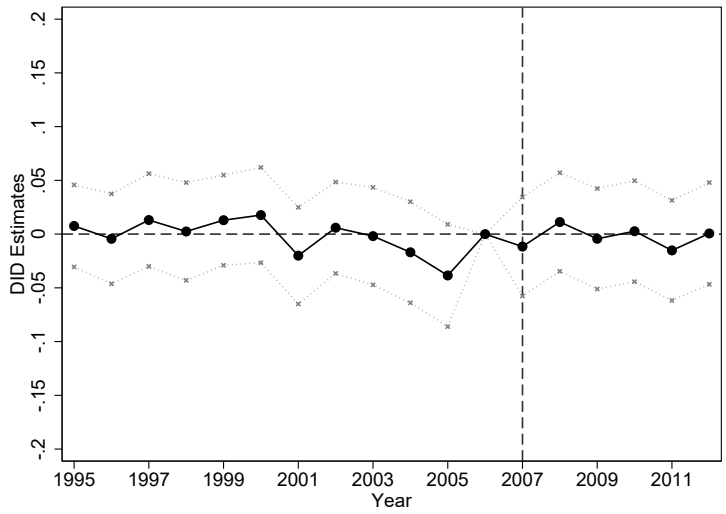
Table 15: Mean, Standard Deviations, and Tests of Treatment-Control Covariate Balance of Education Expenditures Before the Sanctions

	(1) All	(2) Control	(3) Treatment	(4) Diff (2)-(3)
School Tuition	18.83	16.04	23.37	-7.33
	(30.00)	(27.63)	(33.56)	
University Tuition	25.75	27.27	23.27	3.99
	(39.66)	(40.64)	(38.63)	
Books	33.53	30.38	38.67	-8.28
	(34.27)	(33.25)	(35.91)	
Private Tutoring	6.50	6.44	6.60	-0.16
	(12.87)	(13.11)	(12.68)	

Notes: Table reports the share of education spending to each item before the sanctions for the full sample and by treatment status. The sample is households with children aged 6 to 24. Standard deviations are in parenthesis in columns (1)-(3). The last column contains t-tests of the difference in means between the control and the treatment samples. Tests do not reject equality of means for treatment and control groups. *Significant at 10% level; **significant at 5% level; ***significant at 1% level. Treatment group (column 3): Oil and Gas industry, control group (column 2): Water Supply and Information industries.

test, because this group of children attends school anyway. Figure 7 shows the DID estimates of dynamic effects on enrollment in grades 1-9 (Eq (2)) which are not significantly different from zero, as expected. If there were more dropouts before entering college and thus reduction in the proportion of high school graduates, it was hard to separate the effects on college attendance from those on high school or earlier education.

Figure 7: Enrollment in School (grades 1-9)



Note: Figure shows the DID estimates of dynamic effects on enrollment in grades 1-9 as the falsification test (Eq (2)). In Iran, education is compulsory until the end of high school one or grade 9. Therefore, this group of children attends school anyway. The results are not significantly different from zero, as expected. The sample for this analysis is children age 6-14 (children age 6 who born at the start of the academic year (September 23th) or later are excluded because they are not eligible to enroll in school).
Source: Author's estimations based on HEIS data

Fourth, since the data is repeated cross sections, I need to make sure the composition of the sample has not changed between periods. This assumption is necessary so that if any trend change occurs between groups, I can attribute the deviation from the time trend to the effect of the sanctions, not to the change in the composition of the group members. For observed characteristics, I check the covariate balance and labor movement. First, I check the balance of control variables. As Pei et al. (2018) show, a powerful test of the identifying this assumption is to put the control variable on the left-hand side of the regression (Eq (1)) instead of the outcome variable (balancing test). A zero coefficient on the causal variable of interest then confirms the identifying assumption. Table 16 reports the estimated coefficient γ of balancing test for all control variables (X) including parent's education, age, etc. As the results show, the coefficient of interest (γ) is not significantly different from zero. These results show that the selection does not change differentially in terms of gender, age, family size, head's education, and employment status of mother and father.

Table 16: Balancing Test and Selection on Observables

Dependent Variable	Female	Age	Family Size	Head's Education	Employed	
					Mother	Father
Post2007 × Oil	-0.003	0.050	-0.062	-0.603	-0.033	0.002
	(0.023)	(0.289)	(0.149)	(0.384)	(0.025)	(0.023)
Oil	0.010	0.252	0.233*	-2.243***	-0.031	-0.007
	(0.017)	(0.209)	(0.133)	(0.252)	(0.021)	(0.019)
R-squared	0.007	0.021	0.222	0.116	0.039	0.036
Observations	7,065	7,065	7,065	6,935	7,065	7,065
Mean y control	0.459	14.766	5.491	9.716	0.147	0.93

Notes: Table shows the coefficient γ and standard errors from OLS regressions (Eq (1)) for each control variable. The results are not significantly different from zero. Thus, the balancing test is successfully passed. Moreover, these results show the selection does not change differentially in terms of gender, age, family size, head's education, and employment status of mother and father. The sample is households with children aged 6 to 24. Standard deviations are in parenthesis. *Significant at 10% level; **significant at 5% level; ***significant at 1% level. Treatment group: Oil and Gas industry, control group: Water Supply and Information industries.

Although, the sanctions did not affect the family size (the number of observed people in the household), it could affect household composition. In particular, if older children are more likely to be in the household as the result of the sanctions, this would bias the estimates. Thus, I also conduct an analysis of cohort size to make sure the sanctions did not affect household composition. As Table 17 shows, the sanction had not any significant effect on household composition in term of age and relation to the head. Also, sanctions had no effect on the probability of young adults (18-24) to live with their parents.

Table 17: The Effect of Sanctions on Household composition

Dependent Variable	children			Living with parents (18-24 yr old)	relation to the head		
	0-5 yr old	6-15 yr old	16-17 yr old		child	sibling	other
Post2007 × Oil	0.009	0.009	-0.017	-0.120	-0.003	-0.002	0.005
	(0.006)	(0.019)	(0.033)	(0.117)	(0.023)	(0.001)	0.004)
Oil	-0.001	0.025	0.0583	0.057	0.055	0.002	0.000
	(0.003)	(0.020)	(0.133)	(0.039)	(0.078)	(0.002)	(0.003)
R-squared	0.072	0.021	0.026	0.186	0.029	0.008	0.042
Observations	7,065	7,065	7,065	1,109	7,065	7,065	7,065

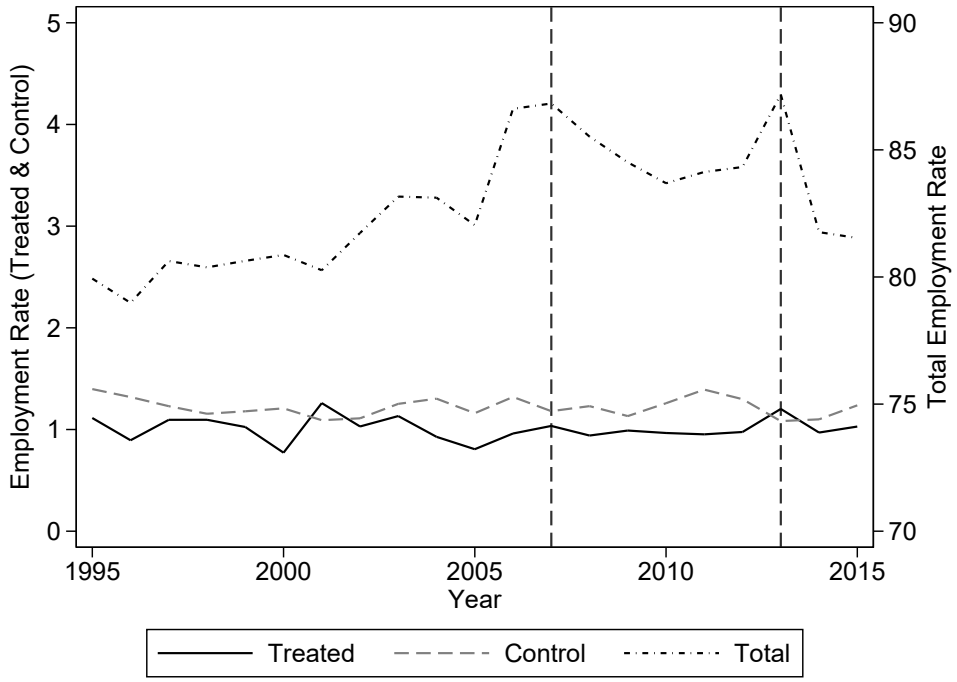
Notes: Table shows the coefficient γ and standard errors from OLS regressions (Eq (1)) for each control variable. The results are not significantly different from zero. Thus, the sanction had not any significant effect on household composition in term of age and relation to the head. Only 1% of households in HIES are not nuclear families. In particular there is no grandparent families among treated and control households. Standard deviations are in parenthesis. *Significant at 10% level; **significant at 5% leve l; ***significant at 1% level. Treatment group: Oil and Gas industry, control group: Water Supply and Information industries.

I check whether the sanctions significantly affect moving workers within industries. Workers

movement across sectors could bias estimates of sanctions effects obtained by comparing outcomes according to the family's head economic activity (Rosenzweig and Wolpin (1988)). As mentioned before, the 2006 UN sanctions mostly affected the oil and gas industry. Since real wage decreased in this industry, it is possible that the workers in the oil and gas industry leave their job and move to other industries. To provide evidence on the impact of the sanctions on labor composition, I check changes of both quantity (employment rate and employment share) and quality (measured by years of schooling) of labor across industries in the main sample and a bigger sample from Iranian Labor Force Survey (ILFS). Figure 8 shows a stable employment rates over time in treatment and control industries despite fluctuations in the total employment rate. The employment rate of treated and control industries were always about 1.0% and 1.2%, respectively. Using this data, I cannot check quality and quantity of unemployed individuals who used to work in treated and control industries. Thus, I use another data Iranian Labor Force Survey (ILFS). The advantage of ILFS data is that it provides some information about the former job of unemployed individuals.[46] Using ILFS, I look at changes of four variables: the employment rate of each industry, the percentage of unemployed individuals who used to work in each industry, average skill of workers in each industry, and average skill of unemployed individuals who used to work in each industry. The employment rates remain the same before and after the sanctions. I also check the average and distribution of years of schooling of workers (as a proxy for workers' skill) in each industry. As Table 18 shows, the average years of schooling has not changed over time across treated and control groups. Unfortunately, I can observe the former job only for an unemployed people. I also observe their reason of the leaving job (low income, getting fired or layoff, the company went out of business, family circumstances, temporary job, position ended, going back to school, illness, relocating, retiring, etc). The percentage of unemployed individuals who used to work in the oil and gas industry has not changed during the sanctions years. Only 7% of these unemployed individuals have left their job because their income was low and this percentage is constant over time (years before and after the sanctions). Moreover, I check the average and distribution of education of unemployed individuals who have left their job in different industries. As Table 19 shows, the average years of schooling has not changed over time across treated and control groups.

[46] In particular, the ILFS offers detailed information about the respondents' demographic characteristics, labor supply, residential area, recent migration, the current job for employees, previous job and reasons for leaving for unemployed. The data are repeated cross sections collected under rotating panel design on the same reference population. The ILFS collects the data on over 400,000 individuals quarterly using random sampling.

Figure 8: Total/treated/ Control Employment rate



Note: Figure shows a stable employment rates over time in treatment and control industries despite fluctuations in total employment rate. The employment rate of treated and control industries were always about 1.0% and 1.2%, respectively.
Source: Author's calculations from HEIS data.

Table 18: Employee's Years of Schooling

	Treatment		Control		
Year	Average Years of Schooling	difference between two years	Average Years of Schooling	difference between two years	Diff Control-Treatment
2005	8.91	-	10.43	-	1.51
2006	9.69	0.77	10.79	0.37	1.11
2007	9.36	-0.32	10.85	0.06	1.48
2008	9.24	-0.12	11.08	0.23	1.84
2009	8.79	-0.44	11.05	-0.03	2.26
2010	8.96	0.16	11.10	0.05	2.14
2011	9.21	0.25	11.54	0.44	2.32
2012	9.48	0.27	11.44	-0.10	1.95
2013	10.05	0.56	11.57	0.13	1.52

Notes: This table presents the average education of workers (as a proxy for workers' skill) in treated and control industries for each year. The columns 2 and 4 contains t-tests of the difference in means between years. Tests do not reject equality of means over years. Thus, As this Table shows the average years of schooling has not changed for both treated and control groups. The time period is 2005-2013. *Significant at 10% level; **significant at 5% level; ***significant at 1% level. Treatment group: Oil and Gas industry, control group: Water Supply and Information industries.
Source: Author's calculations based on Iranian Labor Force Survey (ILFS)

Table 19: Unemployed Individuals' Years of Schooling

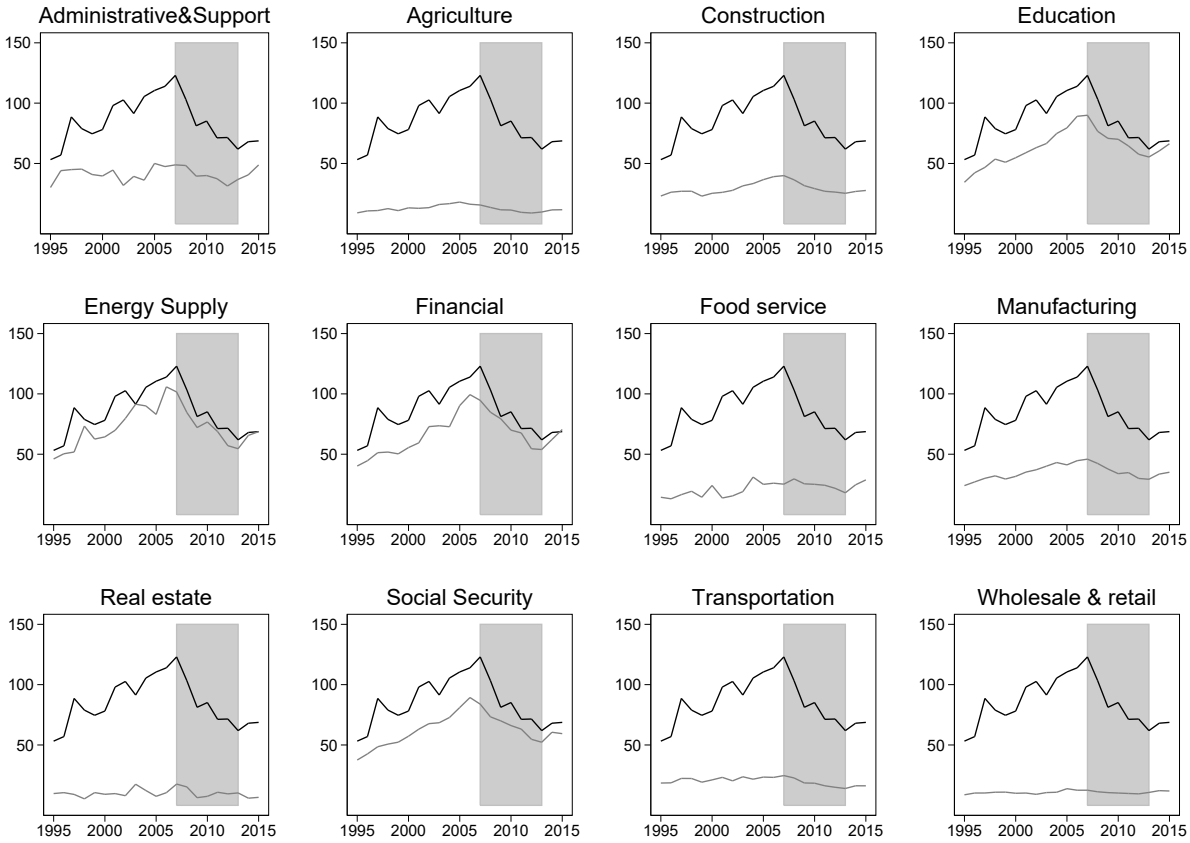
	Treatment		Control		
Year	Average Years of Schooling	difference between two years	Average Years of Schooling	difference between two years	Diff Control-Treatment
2005	7.93	-	10.21	-	2.28
2006	8.23	0.30	10.39	0.18	2.16
2007	7.42	-0.81	10.99	0.59	3.57
2008	7.43	0.01	11.85	0.86	4.41
2009	9.00	1.57	11.95	0.10	2.95
2010	7.33	-1.67	11.88	-0.07	4.54
2011	8.48	1.14	11.45	-0.43	2.97
2012	8.55	0.07	11.34	-0.11	2.79
2013	10.22	1.67	12.16	0.82	1.94

Notes: This table presents the average education of unemployed individuals (as a proxy for skill) who used to work in treated and control industries for each year. The columns 2 and 4 contains t-tests of the difference in means between years. Tests do not reject equality of means over years. Thus, As this Table shows the average years of schooling has not changed for both treated and control groups. The time period is 2005-2013. *Significant at 10% level; **significant at 5% level; ***significant at 1% level. Treatment group: Oil and Gas industry, control group: Water Supply and Information industries.
Source: Author's calculations based on Iranian Labor Force Survey (ILFS)

Workers did not move at least for three reasons. First, during the sanction years, the unemployment rate was high (more than 10%) and increasing, and duration of unemployment after losing a job was one year on average. In fact, the Iranian labor markets were sticky even before the sanctions. Second, different skills needed among industries is another obstacle for the labor movement; for example, oil engineers and technicians have little chance of obtaining employment in other industries. Third, although the real wage of the oil and gas industry was decreasing after the sanctions, the level was higher compared to many other industries. For example, the wage rate of accountants had been higher in the oil and gas industry during the sanctions years. Thus, although they had experience or qualifications to work in other industries, they did not move. Because of these reasons, the sanctions effects on labor movement are ignorable, and most of the members in the treated and control groups remain the same.

For unobserved characteristics, the assumption is that there is no unobserved group specific changes that (1) are correlated with the sanction change and (2) are correlated with group specific changes in the outcome variables. Since outcome variables, e.g. income are likely to be correlated within local labor market and industry levels, all observations are clustered at province (29 provinces) and industry (3 industries in the main identification and 4 industries using synthetic control method) level (87 clusters in the main identification and 116 clusters using synthetic control method) to account for correlation within observations, which may result in an underestimation of standard errors. As explained before, since there are few clusters at the industry level, I calculate p-values using wild bootstrap randomization inference (WBRI).

Figure 9: Real Wage and Salary Income



Note: Figure shows the trends in head's real wage and salary income in other industries compared to the one for households in oil and gas industry.
Source: Author's calculations from HEIS data

D Appendix: Synthetic Control Method

In this appendix, I conducted several sensitivity tests to assess the robustness of results using SCM. First, I check the sensitivity of results using different selected donor industries. Then, I evaluate the effects of the choice of matching variables, matching year range, and methods for selecting weights. Also, I examine in-time and in-place placebos.

As explained before, selected donor industries are less affected by the sanctions (Agriculture, Water supply, Accommodation and food service, Information, and Human health and social work activities). For the leave-one-out test (Abadie et al. (2015)), I iterate over the model to leave out one potential control industry each time to assess whether one of the donor industries is driving the results. The leave-one-out synthetics closely match the original synthetic that includes three industries (water supply, information, and health).

following Cavallo et al. (2013), I check validity of synthetic control for counterfactual by checking

the sensitivity of results to the choice of matching variables. To do so, I include some lags of outcomes in the list of matching variables and check whether the synthetic control matches well the treated households. For the main analysis, I do not include any lags of outcome variables as matching variables. I compare this matching variables selection with combinations ranging from one lag (2000 or 2006) to all lags of family income. Table 20 shows RMSPEs (root mean squared prediction error) as a measure of the pretreatment fit for the different model choices. Including lags outcomes decreases RMSPE, especially the synthetic control that includes more outcome lags as matching variables closely matches the actual oil and gas industries in the pretreatment period. As the Table shows, changing the list of matching variables has no large effect on matching results ($0.14 \leq \text{RMSPE} \leq 0.29$) and the synthetic control closely matches the actual oil and gas industries in the pretreatment period. However, the choice of matching variables could play a major role in selecting industries for the synthetic control if it influences the industries used. Table 21 lists the industries weights for different matching lists. For all cases but one, water supply industry receives the largest weight ($0.811 \leq w \leq 1$).

Table 20: Synthetic Root Mean Squared Prediction Error (RMSPE)

	RMSPE
Main Model	0.27
Family Income lags	
2006	0.25
2000	0.17
1995	0.18
1995,2000	0.14
1995,2006	0.16
2000,2006	0.14
1995,2000,2006	0.15
Matching variables	
Lags only	0.14
Two predictors, no lags	0.27
Two predictors + lags	0.14
Matching year range	
2000-2006	0.29
Method for selecting weights	
Standard	0.27
Nested	0.25

Notes: Table reports RMSPEs (root mean squared prediction error) as a measure of the pretreatment fit for the different model choices. The synthetic control in the main identification closely matches the actual oil and gas industries in the pretreatment period (RMSPE=0.27). Including lags outcomes decreases RMSPE, but not a large effect.

Table 21: Synthetic Weights for Various Matchings

	Agriculture	Water supply	Accommodation& food service	Information	Health& social work
Main Model	0	0.864	0	0.103	0.034
Family Income lags					
2006	0	0.897	0	0.103	0
2000	0	0.938	0.015	0.0.47	0
1995	0	0.811	0	0	0.189
1995,2000	0	0.818	0	0	0.182
1995,2006	0	0.927	0	0.073	0
2000,2006	0	1	0	0	0
1995,2000,2006	0	1	0	0	0
Matching variables					
Lags only	0	0	0	0.645	0.355
Two predictors, no lags	0	0.900	0	0.100	0
Two predictors + lags	0	0.963	0	0.037	0.034
Matching year range					
2000-2006	0	0.906	0	0.048	0.045
Method for selecting weights					
Standard	0	0.864	0	0.103	0.034
Nested	0	0.950	0	0	0.050

Notes: Table reports the industries weights for different matchings. For all cases but one, water supply industry receives the largest weight ($0.811 \leq w \leq 1$).

Following Abadie et al. (2015), I examine two additional sensitivity tests: in-time placebo and in-place placebo. For the in-time placebo test, I estimate the effects by reassign the treatment to occur during the pretreatment period. As an in-place placebo effects, I estimate the effects for each industry in synthetic control, assuming it was treated at the same time. If the placebo effects are as large as the main estimate, then it is likely that the estimated effect was observed by chance. As Table 22 shows, the path of outcome variables (family income and children's education) for households in oil and gas industries did not drift down with synthetic control during the pretreatment period. Moreover, the results are insignificant for placebo treated industries.

Overall, these sensitivity tests verify the robustness of the original synthetic.

Table 22: In-time and In-place Placebos

	Family Income (log)	Attending college (HSG,≤24 yr old)	Years of Schooling (15-24 yr old)	Education Expenditure (log)
in-time placebo				
1996	-0.105	0.199	0.128	0.341
	(0.199)	(0.321)	(0.101)	(0.241)
1997	-0.062	0.078	0.182	-0.091
	(0.083)	(0.121)	(0.118)	(0.311)
1998	-0.101	0.049	0.010	-0.352
	(0.065)	(0.222)	(0.115)	(0.379)
1999	-0.091	0.008	-0.024	-0.489
	(0.059)	(0.040)	(0.138)	(0.492)
2000	-0.041	0.105	-0.145	-0.681
	(0.053)	(0.148)	(0.164)	(0.748)
2001	-0.023	0.077	-0.187	-0.683
	(0.056)	(0.051)	(0.120)	(0.757)
2002	-0.063	0.037	-0.121	-0.890
	(0.048)	(0.040)	(0.118)	(0.940)
2003	-0.074	0.020	-0.336	-1.072
	(0.045)	(0.040)	(0.147)	(1.280)
2004	-0.065	0.025	-0.217	-0.961
	(0.051)	(0.046)	(0.140)	(1.258)
2005	-0.071	0.026	-0.131	-1.002
	(0.051)	(0.045)	(0.169)	(1.254)
2006	-0.076	0.054	-0.290	-0.865
	(0.056)	(0.045)	(0.199)	(0.880)
Actual (2007)	-0.117***	-0.052**	-0.510***	-0.571**
	(0.023)	(0.021)	(0.171)	(0.273)
in-place placebo				
Water Supply	0.063	0.034	-0.063	0.178
	(0.038)	(0.061)	(0.139)	(0.124)
Information	-0.035	0.074	0.307	0.186
	(0.030)	(0.064)	(0.351)	(0.132)
Health	-0.035	-0.071	-0.204	0.014
	(0.026)	(0.123)	(0.262)	(0.072)
Actual (Oil and Gas)	-0.117***	-0.052**	-0.510***	-0.571**
	(0.023)	(0.021)	(0.171)	(0.273)

Notes: This table presents estimated coefficient of Post2007 × Oil in Eq(1) using different treatment year (in-time placebo) and different treated industries (in-place placebo) in the synthetic control method (SCM). The time period is 1995-2013. *Significant at 10% level; **significant at 5% level; ***significant at 1% level.

E Appendix: Effects of the Economic Sanctions on Non-Labor Financial Resource

In this appendix, I show that the wealth index (based on Filmer and Pritchett (1999)) and non-labor income (and their components) are not affected by the sanctions.

Table 23: Sanction Effects on Wealth Index and non-Labor Income

Dependent Variable		Post2007 × Oil
Wealth Index		-4.890
		(-3.294)
components:	durable goods	0.046
		(0.039)
	housing ownership	0.011
		(0.031)
	housing characteristics	-4.890
		(3.294)
non-Labor Income	(*log*)	0.292
		(0.193)
	(*share*)	0.020*
		(0.010)
components (log):	scholarships and cash gifts	-0.170
		(0.401)
	transferred aids	-0.334
		(0.446)
	interest on bank deposits, bonds yield, and share dividends	1.240
		(1.219)
	real estate incomes	-0.168
		(0.209)

Notes: Table shows the coefficient γ and standard errors from OLS regressions (Eq (1)) for wealth index and non-labor income. Non-labor incomes are deflated by CPI which equals 100 in year 2011. The results are not significantly different from zero. Thus, these two variables (and their components) are not affected by sanctions. The sample is households with children aged 6 to 24. Standard deviations are in parenthesis. *Significant at 10% level; **significant at 5% level; ***significant at 1% level. Treatment group: Oil and Gas industry, control group: Water Supply and Information industries.

F Appendix: Fixed Effects

Table 24 presents the estimated effect of the sanctions under two model specifications. Both models includes time fixed effects and group fixed effects (province and industry fixed effects). Moreover, all observations are clustered at province and industry level (87 clusters from 29 provinces and 3 industries) to account for correlation within observations. Model 2 also includes province by year fixed-effects to account any time-variant differences at the province level. The signs of the coefficients and significance are all the same. These results provide evidence that the source of differences across provinces are time invariant.

Table 24: DID Estimates with different Fixed Effects

	Family Income (log)		Attending college (HSG,≤24 yr old)		Years of Schooling (6-24 yr old)		Education Expenditure (log)	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Post2007 × Oil	-0.102***	-0.099***	-0.087**	-0.098**	-0.186***	-0.224***	-0.610***	-0.706**
	(0.032)	(0.036)	(0.039)	(0.047)	(0.088)	(0.089)	(0.246)	(0.316)
time-fixed effect	X	X	X	X	X	X	X	X
group-fixed effect:								
province	X	X	X	X	X	X	X	X
industry	X	X	X	X	X	X	X	X
province × year	-	X	-	X	-	X	-	X

Notes: This table presents estimated coefficients of Eq(1) including different fixed effects. The time period is 1995-2013. *Significant at 10% level; **significant at 5% level; ***significant at 1% level. Treatment group: Oil and Gas industry, synthetic control group: Water Supply, Information, and health industries.