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The Intergenerational Effects of Economic Sanctions

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Abstract

While economic sanctions are successful in achieving political goals, can hurt the civilian population. These negative effects could be even more detrimental and long-lasting for future generations. This study estimates the effects of economic sanctions on children's education by exploiting the United Nations sanctions imposed on Iran in 2006. Using the variation in the strength of sanctions across industries and difference-in-differences with synthetic control analyses, this study finds that the sanctions decreased children's total years of schooling by 0.1 years and the probability of attending college by 4.8 percentage points. Moreover, households reduced education spending by 58 percent—particularly on school tuition. These effects are larger for children who were exposed longer to the sanctions. 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.

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The Intergenerational Effects of Economic Sanctions*

Safoura Moeeni†

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 (Ahn and Ludema (2020); Draca et al. (2019); Hufbauer et al. (2010)). More recent literature investigates the adverse consequences of sanctions on the civilian population while sanctions are in place (Petrescu (2016)). However, as the effects of sanctions may last in the subsequent period, effects on the 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 sanctions. This paper studies these negative externalities of economic sanctions, in particular, it evaluates 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 reduce household income, which is the major source of education funding in Iran.[2] How household income matters for children's

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[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 1966. Since that date, the Security Council has imposed 25 sanctions regimes, in South Africa, the former Yugoslavia, Haiti, Al-Qaida and the Taliban, Iraq, Iran, etc. There are 14 ongoing sanctions which focus on conflicts, nuclear programs, and terrorism.

[2]Household expenditure on education as a percentage of GDP is 5% and government expenditure on education is 4% of GDP in 2006.

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 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 increases 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 the 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.

[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 (e.g., vehicles and furniture) and services (e.g., insurance and utilities) may also be difficult to adjust (Chetty and Szeidl (2007)).

[4] Although there are a few studies that 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. In addition to the overall mixed evidence (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)).

This paper investigates the effects of the 2006 UN sanctions against Iran to identify the impacts of sanctions on children's education. For estimation of the sanctions effects on children's education, the Iranian setting is well suited for two reasons. First, other factors that 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 Surveys (HIES), that roughly span the four decades from the 1980s to 2010s (before, during, and after the sanctions). These surveys collected detailed information on the children's years of schooling and their family income and expenditures including spending on education.

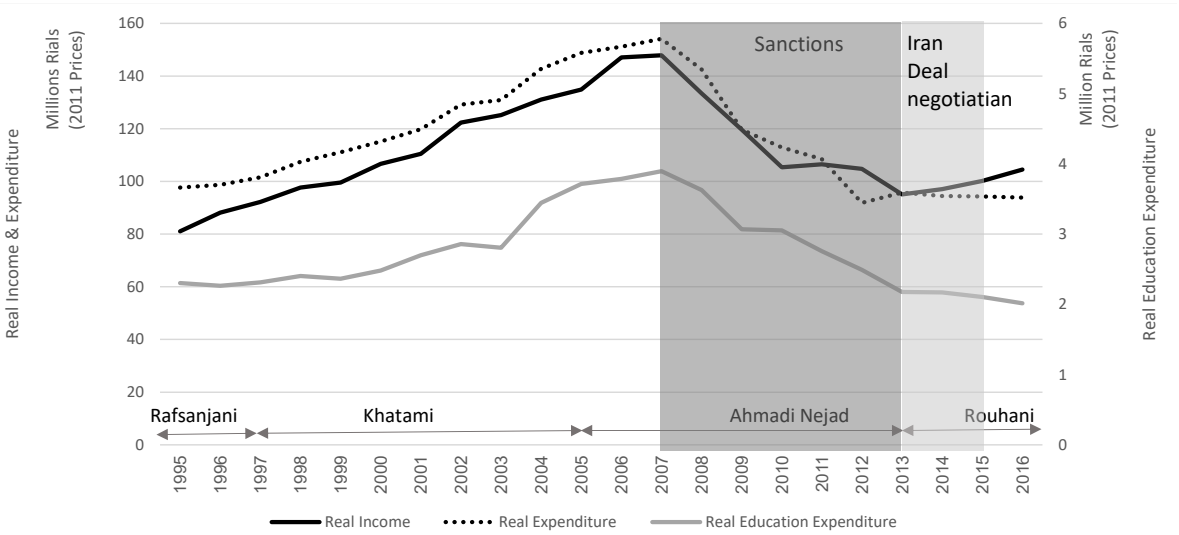
On 23 December 2006, the UN Security Council passed Resolution 1737 and imposed economic sanctions after Iran declined to suspend its uranium enrichment program. The UN sanctions include trade and financial restrictions. Trade restrictions targeted specific firms and individuals including oil and gas production and shipping companies, nuclear research and production companies, and military and security services companies owned, controlled, or performing on behalf of the Islamic Revolutionary Guard Corps (IRGC) or Setad Ejraie Foundation. Overall, these sanctions mostly targeted investments in and export of oil and gas. Financial restrictions entail any transactions with the Central Bank of Iran, disconnecting Iranian banks from the SWIFT, and freezing assets of specific firms and individuals. As a consequence, crude oil exports declined to less than one million barrels per day and the growth rate sank to -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%, resulting in cutting off 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.

The identification strategy of this paper uses variation in the impact of sanctions across industries. This study thus compares the educational outcomes of children in the most affected industries, before and after the sanctions, with a control group of industries not significantly exposed to the sanctions (a difference-in-difference approach).[5] I define households in which the head works in either the oil and gas industry or energy supply as the treated group. The oil and gas industry is directly affected by the sanctions and the energy supply industry is highly dependent on oil exports. Following Abadie and Gardeazabal (2003) and Abadie et al. (2010), this paper uses a synthetic control method (SCM) and weight industries in the control group to construct a synthetic control that matches treated households for a 12-year pre-sanctions period. The synthetic control group includes information, education, and health industries with weights 0.148, 0.169, and 0.683, respectively. These industries are heavily regulated by the government. Therefore, the sanctions have little effect on wages and employment levels of these industries.[6] Moreover, these industries are not dependent on trade, thus making them unaffected by the changes in the exchange rate. The raw data confirm that households in these industries experienced the lowest incidence of family income changes after the sanctions.

[5] My methodology, which relies on difference-in-differences, can only capture differential impacts on the most affected households relative to the less affected households and does not capture the general effects of the sanctions.

[6] Those working in the information, education, and health industries are mostly public sector employees for whom the minimum wage and the minimum percentage change in wage rates in the public sector are determined by the Supreme Labor Council (in the Ministry of Labor and Social Welfare). The Supreme Labor Council adjusts the wage rates in the public sector based on the expected inflation rate.

Figure 1: Average Real Income, Total Expenditures, and Education Spendings for Iranian Households



Source: Author's calculations from HEIS data.
Note: Figure displays the decreases in average real annual income, total expenditures, and education spendings for Iranian households over the years of economic sanctions.

The analysis of this paper reveals two main findings. First, among children who ever been between the ages 6 to 24 during years 2006-2013, sanctions decreased the years of schooling significantly by 0.1 years (0.3 years among children ages 15-24 years) and the probability of attending college (any post-secondary programs) by 4.8 percentage points. This effect on children's education is more than two 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.[7] Also, these negative effects are larger for children who were exposed longer to the sanctions and children at crucial ages (high school dropout age and matriculation at a university). In particular, the years of schooling and probability of attending college significantly decreased by 0.4 years and 6.1 percentage points, respectively among cohorts whose more years of schooling career overlapped with the sanctions. Moreover, the economic sanctions decreased the enrollment rate at the high school by 4.3 percentage points among children at high school dropout age (16 years old) with a larger effect among girls and decreased the probability of attending college at age 18 (the average age of matriculation) by 15.4 percentage points. These effects worsen when children reach the age of 16 and 18 at a later stage of the sanctions period. A simple back of the envelope calculation shows that 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%. Also, 45% of the costs to the society associated with the reduction in earnings comes from decreased earnings for the current workers, and 55% comes from decreased earnings for the next generation. It suggests that the cost estimates using only earnings of the current generation may only capture less than half of the overall cost.

Second, this paper examines the effects of the sanctions on investment in children's education by looking at household spending on education. This paper results show that after the implementation of the sanctions,

[7] 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.

households reduced expenditure on education by 58% - 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.[8] This negative effect on education expenditure is larger than implied by the income elasticity estimates from the previous literature (Acar et al. (2016); Huy (2012); Qian and Smyth (2011)). Most of these studies find that the income elasticity of education spending is significantly less than one.[9] Alternatively, this paper finds an income elasticity of 3.3, indicating households allocate a smaller share of their budgets to education spending after the sanctions.

Overall, after the sanctions, both the educational attainment (measured by the enrollment rates and years of schooling) and investment in children's education (measured by the family education spending) decreased. Although the effects of sanctions depend on the context and severity of the sanctions and how the 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, this study 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 which lasted seven years. As explained above, persistent changes in family income can 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. (2021); 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. (2021)). Despite differences, all these papers look at the cases in which the exogenous shock in family income is temporary, in accordance they find small effects compared to my findings. Even when the shock is large e.g., lotteries, as Bulman et al. (2021) 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, which 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); Hilger (2016); Pan and Ost (2014)). There are a few studies that examine the effect of parental job loss on children's schooling in cases that other financial resources are not available to children. For example, 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, respectively. 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 (2019) show parental loss job caused by a conflict in the Occupied Palestinian Territories increases child school dropout. This 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.

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

[9] Previous studies find that even for those groups of households that education spending is a luxury good, income elasticity is less than 2.

Second, this paper adds to the distributional debate about the burden of family income effects. As explained above, households' response to an income shock could vary across different income quantiles.[10] 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.[11] On the contrary, the sanctions affect treated households at any level of income. Therefore, this paper estimates the effects for high-income households as well as low-income households. Moreover, these studies look at positive shocks in family income. Household responses to upward versus downward shocks could be asymmetric. This paper complements this literature by studying the effects of a negative persistence shock in the family income.

Finally, this paper complements the literature on trade and child schooling. On the one hand, positive income shock and higher wages caused by trade liberalization (e.g., changes in the relative price of an exported commodity) could increase human capital investment and decrease child labor (Edmonds and Pavcnik (2005a); Edmonds and Pavcnik (2005b); Ambler et al. (2015)). On the other hand, trade reforms that increase the opportunity cost of education by creating more employment opportunities for children (e.g., less skilled export-manufacturing jobs) could lead to a reduction in children's education (Atkin (2016); Blanchard and Olney (2017)). Findings of this paper are consistent with Edmonds et al. (2010) and Edmonds et al. (2009) that show the importance of household income on child time allocation. Using Indian tariff reform, they show that after tariff reductions, families that experienced a negative shock in their income cut investment in their children's education (to save schooling cost), even the opportunity cost of schooling also decreased. This paper also finds that a negative shock in household income caused by trade restrictions imposed by economic sanctions leads to a reduction in investment in children's education.

This paper proceeds as follows. Section 2 provides the institutional setting and discuss mechanisms behind the impacts of the 2006 UN economic sanctions on children's education. Sections 3 and 4 describe the data set and the identification strategy. Section 5 and 6 present the main empirical results and report some robustness checks. Section 7 explores heterogeneous effects by exposure to the sanctions. Section 8 concludes the paper. All appendix material can be found in the Online Appendix.

[10] 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.

[11] 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.

2 Institutional Setting and Mechanisms

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 the nuclear program if any sanctions were imposed. Unexpectedly, in December 2006, the Council imposed trade and financial sanctions on Iran targeting the oil and gas industry (by imposing restrictions on investments in and export of oil, gas, and refined petroleum products) and the Iranian Revolutionary Guard Corps (IRGC) and Setad Ejraie Foundation (by banning any business dealings with them). 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, controlled, or acting on behalf of IRGC or Setad. Financial restrictions encompass banking and insurance transactions (including any transactions with the Central Bank of Iran, disconnecting Iranian banks from the SWIFT, and freezing assets of specific firms and individuals).[12] The 2006 sanctions were effective to pressure Iran to negotiate on its nuclear program. In 2013, Iran accepted negotiation for a framework deal with permanent members of the UN Security Council (China, France, Russia, the U.K., and the U.S.) and Germany (P5+1). On 2 April 2015, they finalized an agreement (Joint Comprehensive Plan of Action (JCPOA)) known as the Iran deal. Thus, the UN Security Council, the E.U., and the U.S. have terminated all nuclear-related resolutions and sanctions in January 2016.[13]

The 2006 sanctions are the most severe sanctions ever put on Iran because most countries including the E.U. stopped buying oil from Iran. Moreover, the U.S. 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 withdrew from the Iran oil industry, and thus 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. In consequence, Iran's economy got 15-20% smaller than it would have been in the absence of 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 sank to 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, over the 2007-2013 period, households' real income and their spending on education on average decreased by 35% and 43%, respectively.

[12]The sanctions had been gradually more intense by UN Resolutions 1737 (in 2006), 1747 (in 2007), 1803 (in 2008), and 1929 (in 2010) and EU's oil embargo (in 2012). This paper finds the aggregate effects during the years of the sanctions, though as figures 1 and 2 show, the major part of these effects is related to the first years of the sanctions.

[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 UN Security Council. However, the U.S. withdrew from the deal in May 2018 and reimposed the sanctions in November 2018.

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.[15] Enrollment rates exceed 90% at the primary and secondary levels, comparable to that of Western countries. Thus, the youth literacy rate increased from 56% in 1976 to 97% in 2006 (Source: World Bank).[16]

The rapid growth in education 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% 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 levels of education.[17] However, like most Middle Eastern countries, a large share of Iranian government spending on education is allocated to post-secondary education in large urban areas.[18] 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.[19] The competition to succeed in school and the public universities entrance examination have encouraged parents to spend on their children's education such as spending on private schools and private tutoring to help their children in this competition (Salehi-Isfahani (2012)).[20] As Figure 1 shows, Iranian households' spending on education, which is the major source of education funding in Iran, increased by 67% over the 1995-2006 period. Spending on primary and secondary schools tuition is a significant share of total household expenditure on education in Iran (21%). Many of the best overall primary and secondary schools in Iran are privately funded (Source: Ministry of Education of Iran).[21] Also, 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 test scores than their public school counterparts. For example, children who attend private schools perform better in school final exams and the universities entrance exam and have better academic outcomes than those in public schools (Salehi-Isfahani et al. (2014); 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, attendance at the undergraduate programs

[14] Over these years, Iran's economy has been under various economic sanctions. The first economic sanctions on Iran were imposed by the U.S 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] Education has expanded in Middle Eastern and North African (MENA) countries faster than in any other region of the world (Source: World Bank). Some countries such as Iran, Turkey, Egypt, and Jordan experienced more growth in education.

[16] 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.

[17] 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)).

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

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

[20] 58% of pre-university students receive private tutoring, which is a significant item in households' education expenditure (52%), to increase their probability of success at the university entrance examination (source: calculation by the author using HIES).

[21] The Ministry of Education's school ranking is based on teachers' quality standard, level of social and cognitive activities, school facilities and environment, school management, etc.

dropped after the implementation of the sanction. Over 2007-2013, the enrollment rates in primary and secondary schools were always around 97% and 89%, respectively (Source: Statistical Centre of Iran, otherwise noted). At the same time, the population of first-year college students decreased by 11.5%. Moreover, over the years of sanctions, the investment in children's education measured by household spending on education has decreased on average by 43% (Source: calculation by the author using HIES). 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.

2.3 Mechanisms behind Sanctions

In this section, I explore the mechanisms by which economic sanctions may decrease investment in children's education. The sanctions affect children's education through changes in the demand side (labor income and relative prices) and the supply side of schooling (government budget).

One mechanism by which the sanctions affect children's education is through labor income. As explained above, as a result of the sanctions, labor earnings decreased in the treated industries. 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 constraints. An 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. Reduction in family income after the sanctions may have made it harder for children to attend school. However, as explained above, 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 (Browning and Lusardi (1996); Deaton (1992)). However, if sanctions increased uncertainty about future income, households may consume less and save more (Sandmo (1970)).[22]

Second, labor income shocks may affect children's 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 foregone 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 levels of education. In consequence, the income of some low education levels that used 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, the overall effect is ambiguous and depends on the relative strength of different effects. In

[22] I find no significant effect of sanctions on family savings and debt (Table **??**, panels C and D, in the supplementary online appendix available with this article at The World Bank Economic Review website)

the supplementary online appendix (section S4.), I outline a simple model that identifies these two channels and describes conditions by which the incentive to invest in education may increase or decrease after the sanctions. Therefore, I empirically test the overall impact of sanctions in this paper.

Another mechanism by which economic sanctions may affect children's education is through changes in the government's budget. Based on the government budget documents, the 2006 UN sanctions did not affect public spending on education. The sanctions affected Iranian government revenue because on average 60% of Iranian government revenues come from oil and gas which was affected by the sanctions. However, as the government budget documents show, the real value of Iranian government's spending on social programs, including health and education, did not show a significant response to this shock. In particular, public spending on education and each of its components (spending on primary to tertiary education) did not change after the sanctions (source: Government Budget documents and World Bank). As government budget documents show during the years of sanctions the Iranian government financed other expenditure by reducing the military spending, which is the major component of government spending. This state of budgetary management is not limited to the shock caused by the sanctions. Habibi (2001) and Farzanegan (2011) show that pre-sanctions oil revenue fluctuations in Iran and other Middle Eastern oil-exporting countries do not affect the basic social spending on education, health, and social welfare. 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 possible reason for this allocation is that governments are sensitive to the demands of the urban middle class, and college education is important for this group (Richards and Waterbury (1996)).

Economic sanctions may also affect children's education through changes in relative prices. In addition to the reduction in household income, rising prices decreased households' spending capacity. Over the years of sanctions, prices of many commodities spiraled upwards and inflation reached 35%. However, the magnitude of this change is different across goods and services. In particular, the prices of tradables (typically goods) have risen significantly relative to non-tradables (typically services). Thus, the relative prices and so the budget shares of the different commodities have changed.[23] Although education prices doubled, the changes are not as much as other commodities. The Education Price Index (EPI) increased on average 8% less than the overall rate of inflation.

3 Data

The main data source is the Iranian Households Income and Expenditures Surveys (HIES).[24] These surveys, which are conducted yearly by the Statistics Centre of Iran (SCI), are designed to be representative of the population of Iran and cover near 40,000 households every year. HIES include extensive data on expenditures of households including education spending according to the Classification of Individual Consumption According to Purpose

[23]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 patterns. 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%).

[24]HIES are publicly available on www.amar.org.ir

(COICOP).[25] Moreover, these data contain rich information at the individual level including age, gender, years of education, income, marital status, and relation with the head of family.[26]

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, and family expenditure). Second, the HIES contain children's years of schooling, enrollment at different levels of education, and family education spending. Information on the education spending includes payments for books, tuition, private tutoring, and donation to the school for different levels of education (pre-primary, 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.[27] Children aged six who were 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 include all observations from years 1995 to 2013 (1374 to 1392 in Persian Calendar), 12 years before and 7 years after the implementation of 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 a robustness check in section 6. I consider only those children who live with their parents as the data do not report parental characteristics for those who do not live with their parents. 87% of individuals age 18-24 live with their parents.[28] The sanctions could have had an impact on the composition of households. In particular, if older children are more likely to be in the household or more likely to married young as a result of the sanctions, this would bias the estimates. In section 4, I conduct an analysis of cohort size and find that the sanctions did not affect household composition. In particular, the sanctions had no effect on the probability with which young adults (18-24) live with their parents.

Although only 6% of children age 6-24 in treated and control households live in rural areas,[29] for the main analysis, I exclude them from the sample. I study households who live in urban regions of the country because there are differences between rural and urban areas in factors affecting education spending. Estimated elasticities suggest that rural households' spending on education is more sensitive to changes in income relative to urban households' (see, for example, Mussa (2013)). Moreover, education opportunities are different between cities and rural communities, in particular, all rural schools are public. Also, private supplementary tutoring is not available to students in rural areas. One important concern is that endogenous migration could bias the results, although the intensity of the sanctions does not vary across regions. In particular, if families of highly educated children move to rural areas, the effect of sanctions will be overestimated by considering only children in urban areas. To explore this possibility, I first looked at the migration patterns before and after the sanction. 94% of children age 6-24 in treated and control households live in urban areas (77% of them did not change their location at all; 17% moved from a city to another city); 1% moved from a village to another village; and only 5% moved between urban

[25] Since 2010, HIES is collected as a rotating panel based on a 3-in-then-out method, however, the rotating nature of the panel can not be used in this study because households' id is changed for confidentiality purpose in the version available to researchers. About 90% of children in the main sample of this paper are distinct observations.

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

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

[28] Almost all young adults who do not live with their parents are married.

[29] Most workers in rural areas work in agriculture and construction industries

and rural areas. Most importantly, the sanctions had no effect on the migration pattern.[30] Second, as a robustness check (section 6), I include children from rural areas and find similar results mostly because of few observations of rural households in treated and control industries.

4 Identification Strategy

I exploit variation in the impact of 2006 economic sanctions across industries in a difference-in-differences framework using the synthetic control method (SCM) (Abadie and Gardeazabal (2003); Abadie et al. (2010); Abadie et al. (2015)). The first difference is over time. The second difference is across groups of households. The difference-in-difference comparison is implemented by estimating regressions of the following type:

$$Y_{ispt} = \alpha + \gamma \ (Treat_i \times Post2007_t) + \beta \ Treat_i + \lambda_t + X_{ispt}^{'}\delta + \phi_p + \psi_s + \varepsilon_{ispt} \tag{1}$$

where Y_{ispt} is the outcome variable of interest (family income, family education spending, and children's education outcomes) of individual (or household) i in province p and industry s at time t. The variable $Treat_i$ is a dummy for treatment households 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 outcome variables but are not observable to me. The vector X_{ispt} is a set of individual or household-specific characteristics to control for any observable differences that might confound the analysis (e.g., age for estimation the effect on years of schooling). Since outcome variables e.g., income are likely to be correlated within local labor markets and industry level, all observations are clustered at the province and industry levels to account for correlation within observations, which may result in an underestimation of standard errors. 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 also examine gender differences in the impact of sanctions by estimating the following equation:

$$\begin{aligned} Y_{ispt} = \alpha + & \gamma_1 \ (Treat_i \times Post2007_t) + \gamma_2 \ (Female \times Treat_i \times Post2007_t) \\ & + \beta_1 \ Treat_i + \beta_2 \ (Female \times Treat_i) + Female + \lambda_t + X_{ispt}^{'}\delta + \phi_p + \psi_s + \varepsilon_{ispt} \end{aligned} \tag{2}$$

To explore the effect of the sanctions over time, I generalize Eq (1) by replacing $Treat_i \times Post2007_t$ with a full set of treatment times year interaction terms:

$$Y_{ispt} \quad = \alpha + \sum_{l=1995}^{2013} \gamma_l \ (Treat_i \times year_l) + \beta \ Treat_i + \lambda_t + X_{ispt}^{'}\delta + \phi_p + \psi_s + \varepsilon_{ispt} \tag{3}$$

where $year_l$ is a dummy that is 1 in year l and 0 otherwise. The pre-2007 interaction terms provide evidence for pretreatment common trend assumption, although they may capture possible anticipation effects.

[30] These facts are not limited to the main sample. Every year around 15% of the population migrates; 75% of migration is a movement from one city to another city. Most households migrating between regions (from rural to urban areas and visa versa) are employed in manufacturing, construction, and trade industries.

I follow the approach of Abadie and Gardeazabal (2003) and Abadie et al. (2010), weighting industries to construct a synthetic counterfactual that replicates the characteristic of treatment group before exposure to the sanctions.

4.1 Synthetic Control Group Strategy.

The SCM is based on the idea that when an intervention affects a small number of units, a combination of unaffected unites provides a better comparison group, especially when no single unit alone is comparable to the affected units (Abadie (2021)).

In the ideal case, sanctions would be an independent random event for targeted industries that had no spillover effect to other industries. Although the 2006 sanctions targeted specific firms and individuals mostly in the oil and gas industry, the present analysis is not such an ideal case because Iran's economy is dependent on oil exports. Thus, sanctions indirectly impacted some other industries through the government budget and exchange rates.

I define households in which the head works for either oil and gas industry or energy supply (electricity, gas, steam and air conditioning supply) as the treated group.[31] Although the sanctions affected many sections of Iran's economy, the severity is different across industries: (1) based on detailed policy documents on the 2006 sanctions, people who work in the oil and gas industry were directly affected by the sanctions. Also, some firms in other industries (including some firms in rubber, plastic, and mineral products industry, financial sector, motor vehicles industry, and basic metals industry) that owned or control by IRGC or Setad were directly affected by the sanctions (Source: the sanctions documents and Draca et al. (2019)).[32] Since the name of these firms are not observable in the main data used in this paper, I discard the entire financial sector and those subcategories of industries that include one of these targeted firms; (2) the industries that need to import raw materials and industries that are dependent on oil exports suffered from trade restrictions and the increase in the exchange rate, for example the energy supply industry was hit hard. After the oil and gas industry, the energy supply industry experienced the largest negative growth rate of value-added during the sanctions. Although this effect is indirect, since it is large and immediate, I include households in which the head works for the energy supply industry in the treated group. Other industries that were indirectly affected experience relatively small changes with a lag.[33] (3) there are some industries that are regulated by the government and/or are not dependent on trade, thus these industries were hardly affected by the sanctions (e.g., information industry); (4) the export-oriented industries and the industries that have foreign rivals benefited from the increase in the exchange rate as a result of sanctions (e.g., agriculture and food industries).[34]

The definition of the comparison group is crucial, as it should capture the counterfactual outcomes trend in the absence of the sanctions. One potential comparison group would be households in which the head works for non-oil/energy industries. This group is not a good comparison group because these households differ from households

[31] I define the household head as the person earning the highest monetary income, mostly the same as the person reported as the head of the household. Some families reported the eldest person (e.g., a grandparent) as the head.

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

[33] The growth rate of the real estate and administrative and support services are also largely decreased after the sanctions. However, I do not consider households whose head works in these industries in the treated group for two reasons: first, these households have a large difference in their characteristics relative to other treated households in the oil and gas industry and energy supply; second, the negative effect of the sanctions in these industries happened with a lag.

[34] Overall, 10% of household heads work in these industries that benefited.

in oil and energy industries in characteristics that are thought to be related to the potential for children's education. In fact, the pre-treatment trends of outcome variables (family income and education outcomes) are not parallel for these two groups. Also, there is no single unaffected industry that provides a comparison for the affected industries. Hence, I use the SCM to find a combination of industries not (or less) affected by the sanctions as a synthetic control group and estimate the counterfactual for treated group. I consider all other industries but financial, real estate, and administrative and support service industries in the donor pool (11 industries),[35] though I check the sensitivity of results using different selected donor industries in section 6 (Table S4).

Weights are determined to maximize the similarity between the synthetic control and the treated households in terms of matching variables. For the main analysis, following Botosaru and Ferman (2019),[36] I consider only pre-treatment family income as the matching variable. As a robustness check, I include observed covariates (parent's education, employment status, age, etc.) and find similar results (Table S4). In particular, I find that algorithms that minimize the distance between the treated units and the synthetic control put small and ignorable variable weights for covariates if more pre-treatment outcomes are included. I also use a modified SC estimator by Ferman and Pinto (2021) and demean the data using information from the pre-treatment period because the values of pre-treatment family income for the affected industries do not fall inside the convex hull of the corresponding values for the donor pool and can yield an imperfect fit. Thus, I construct the synthetic control using the demeaned data. Since there is more than one treated unit, based on Abadie et al. (2010), I aggregate the treated units into a single unit (pooled SCM). Pooled SCM can yield poor unit-specific fits. Thus, I check the fit for each treated unit and find a good fit mostly because the characteristics of households in affected units are similar (Figure S3). Moreover, following Ben-Michael et al. (2021), I use the partially pooled SCM as a robustness check and find similar results.

The optimal weights are positive for three industries information, education, and health with values 0.148, 0.169, and 0.683, respectively and take value zero for the other potential controls in the donor pool. 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.[37] Second, these industries are not dependent on trade, thus making them unaffected by the changes in the exchange rate due to trade

[35]Since the synthetic control is supposed to reproduce the outcome variables for treated industries in the absence of the sanctions, I discard financial, real estate, and administrative and support service industries from the donor pool. As explained above, some firms in the financial sector that owned or control by either IRGC or Setad are targeted by the sanctions, but in HIES only the job sector is observable not the firm. Thus, I exclude the financial industry. I also exclude households in real estate and administrative and support service industries from the donor pool because, as explained above, these households have a large difference in their characteristics relative to treated households which makes them unsuitable controls. Based on Abadie (2021), while the SCM puts small weights for dissimilar units, it is still important to limit the donor pool to units with similar characteristics to the affected unit to avoid interpolation biases. Also, these households were indirectly affected by the sanctions, though with a lag. Moreover, I exclude industries for which data are not consistently available in the HIES: arts, entertainment and recreation; activities of households as employers; undifferentiated goods- and services-producing activities of households for own use; professional, scientific and technical activities; activities of extraterritorial organizations and bodies.

[36]Botosaru and Ferman (2019) shows as long as there is a perfect match on pre-treatment outcomes, a perfect match on covariates is not required. Ben-Michael et al. (2021) and Doudchenko and Imbens (2016) also use only the lagged outcomes as matching variables and show including covariates has no effects on synthetic control.

[37]Those working in the information, Education, and health (the control groups) are mostly public sector employees for whom the minimum wage and the minimum percentage change in wage rates are determined by the Supreme Labor Council (in the Ministry of Labor and Social Welfare). The Supreme Labor Council adjusts wage rates in the public sector based on the expected inflation rate. Since the 2006 inflation rate was 12%, the Supreme Labor Council announced a minimum 10% increase in the wage rate of public workers assuming a 10% expected inflation rate in 2007. The actual inflation rate in 2007 was 18%. Figure 2 show a reduction in the real income of the control group in 2007. Thus, the Supreme Labor Council increased the wage rates at higher rates the following years. Overall, the real income of workers in the control group remains constant. The government financed this increase in the wage rate of workers in the public sector by reducing military spending (Source: Government Budget documents). For treated groups (oil & gas, and energy industries) there is no such inflation adjustment.

restrictions after the sanctions. The raw data confirm that households in these industries experienced the lowest incidence of family income changes after the sanctions. Figure 2 panel (a) displays the real median family income for treated households and their synthetic counterpart in the period 1995-2015. The synthetic control almost exactly reproduces the family income for the treated households during the entire pre-sanctions period (1995-2006). As Figure 2 panle (b) shows, the gap between the actual and counterfactual family income widens from around zero over the pre-sanctions period, to 45% in 2013.

Table 1 reports households' and children's characteristics of the synthetic group comparing to treated households in the absence of the sanctions. The variables overall are well balanced between these groups. It is important to emphasize that many economic sectors were affected by the sanction. This paper compare the most affected households with the least affected comparison group. In section 6, I check the validity of the synthetic control for counterfactual by checking the sensitivity of results to the choice of different matching methods (different matching variables, different matching year range, and different methods for selecting weights) and different selected donor industries. Overall, the sensitivity tests verify the robustness on the original results (Table S4).

Table 1: Descriptive Statistics (before the 2006 UN Economic Sanctions)

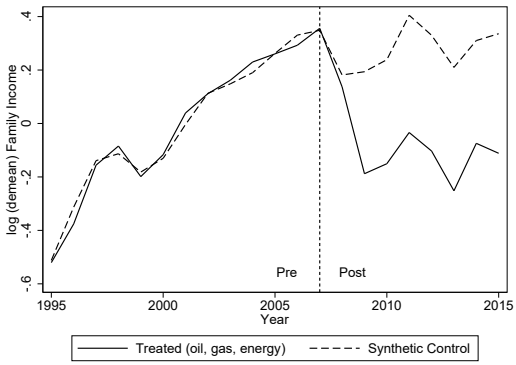
	Treatment	Synthetic Control
Household-level variables		
Ln(Total Family income)	18.59	18.50
	(0.60)	(0.74)
demean	-1.23e-07	-7.66e-07
	(0.59)	(0.73)
Ln(Labor income)	18.15	18.02
	(0.61)	(0.78)
demean	-1.63e-07	4.86e-07
	(0.61)	(0.76)
Ln(Education Expenditure)	12.21	12.19
	(4.78)	(4.82)
Observations	2,282	10,405
Child-level variables ($6 \leq age \leq 24$)		
Age (female)	14.47	14.55
	(4.96)	(5.01)
Age (male)	14.41	14.28
	(4.88)	(4.86)
Years of schooling: girls	7.26	7.68
	(3.84)	(4.10)
Years of schooling: boys	7.24	7.38
	(3.75)	(3.87)
% In school: girls	77.51	78.63
% In school: boys	79.28	81.30
% In school: girls 6-18	93.01	93.36
% In school: boys 6-18	92.84	93.60
Observations	6,295	25,076

Source: Iranian Households Income and Expenditures Surveys (HIES).

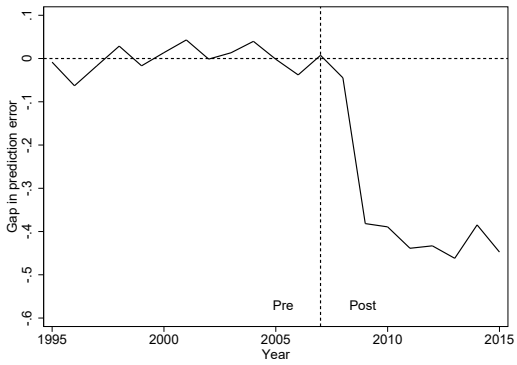
Note: The table reports summary statistics of household and child-level data by treatment status. Standard deviations are in parenthesis. Family incomes and expenditures are deflated by CPI. Treated: Oil and Gas, Energy Supply industries. Synthetic control: Information (0.148), Education (0.169), and Health (0.683) industries.

Figure 2: Real Median Family Income and Gap between the Treated and Synthetic Control

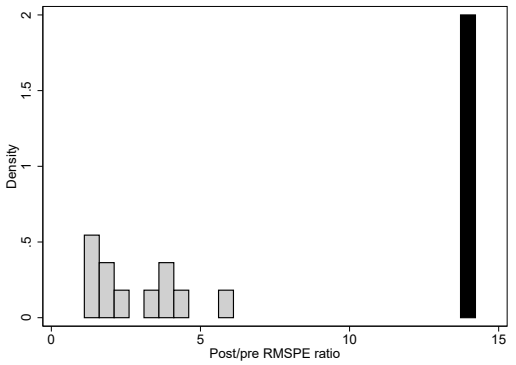
(a) Real Median Family Income



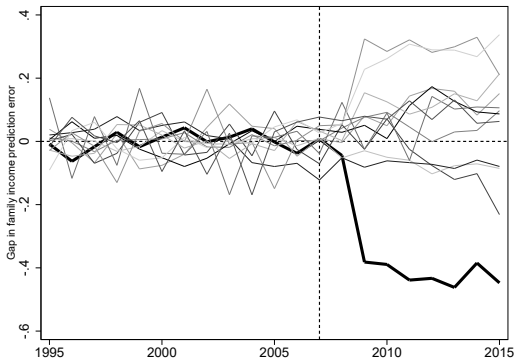
(b) Gap between Treated & Synthetic



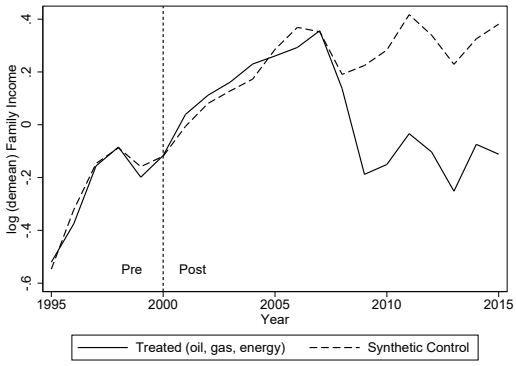
(c) post/pre RMSPE ratio



(d) Gap between Treated & Synthetic



(e) Placebo Sanctions in the Year 2000



Source: Iranian Households Income and Expenditure Surveys (HIES).
Note: The figure reports synthetic control method analyses as well as placebos at industry level in the 1995â2015 period. Panel (a) displays the average real family income for treated households in the oil and gas industry and energy supply (solid line) and the synthetic control (dashed line). Panel (b) shows the gap between actual treated and synthetic control. Panel (c) shows the post/pre-treatment ratios of root mean squared prediction errors (RMSPE) for placebo estimates. The black ones indicate the post/pre-RMSPE ratio using the actual treated industries. Panel (d) shows the gap between treated and synthetic control for placebo estimates, with actual treated industries shown with a black solid line. Treated: oil and gas, energy supply industries. Synthetic control: information (0.148), education (0.169), and health (0.683) industries. Synthetic control for placebo sanctions in the year 2000: education (0.483) and health (0.517) industries.

4.2 Identification Assumptions.

The key identifying assumption for this estimation method is that the composition of the sample is not changed between periods. In particular, if workers in affected industries move to other industries, the effect of sanctions will be overestimated. In addition to several robustness checks (section 6), for observed characteristics, I check labor and household compositions (section S1. in the supplementary online appendix provides more details). First, I check the impact of the sanctions on labor composition in terms of both quantity (employment rate, employment share, job separation rate, and job tenure) and quality (measured by years of schooling and job experience) of labor across industries. Figure S1 shows a stable employment rate over time in the treated and control industries despite fluctuations in the total employment rate. Also, as Table S1 and Figure S2 show, the sanctions had no significant effects on job separation rates and job tenure. Yet, treated and control industries might have lost different types of workers. For example, if highly educated workers moved from the oil and gas industry to other industries and low educated workers moved to the oil and gas industry, a reduction in children's education might be observed in oil and gas industry as a result of this movement, not the sanctions. Thus, I also examine the effects of sanctions on years of schooling and skill index (the aggregate of years of schooling and work experience based on a principal component analysis) of employees and unemployed individuals who used to work in each industry, and find no significant effect (Table S1).

These pieces of evidence indicate that workers in the oil and energy industries took significant pay decreases, but they neither were laid off nor quit their job.[38] Rahmati and Chobdaran (2020) show that the high labor mobility cost led to the little labor movement across industries in Iran. Based on their estimation labor mobility cost is between 3-11 times of per capita income in 2011. The cost is even higher for educated workers.[39] There are at least three possible reasons why workers did not move. First, during the years of sanctions, the unemployment rate was high and increasing, and the duration of unemployment after losing a job was one year on average. Under these conditions, workers are less likely to quit. In fact, the Iranian labor markets were sticky even before the sanctions. Second, different skills needed among industries is another obstacle for labor movement; for example, oil engineers and technicians have little chance of obtaining employment in other industries. Third, although the real wage rate of the treated industries had been decreasing over the years of 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, the workers who had the experience or qualifications to work in other industries, would not switch to another industry.

I also check the effect of sanction on the households composition. The result of a balancing test in Table **??** shows the sanctions had no significant effect on children and households' characteristics including children's gender, children's age, family size, head's education, and parent's employment status. Although the sanctions did not affect the family size (the number of observed people in the household), if older children are more likely to live with their parents after the sanctions, this would bias the estimates. Thus, I also conduct an analysis of cohort size. As

[38] According to Iranian labor law, termination of the employment contract is allowed only under the following instances: death/total disability of the employee, retirement of the employee, expiration of the duration of the employment contract conclusion of work in task specific contracts, workplace closures, and the resignation of the employee (Source: Ministry of Labor and Social Welfare).

[39] The labor mobility cost is 2.76 and 3.71 times the annual wage in the developed and developing countries, respectively (Artuc et al. (2015)). Labor mobility costs include moving cost, firing-hiring costs, sector-specific skills, etc.

Table S3 shows, the sanctions had no significant effect on the household composition in terms of age and relation to the head. In particular, the sanctions had no effect on the probability with which young adults (18-24) live with their parents.

For unobserved characteristics, as explained above, observations are clustered (150 clusters) at the province (30 provinces) and industry levels (5 industries: 2 treated industries and 3 control industries). However, since there are a few clusters at the industry level, t-tests based on cluster-robust variance estimator (CRVE) tend to be over-rejected. Moreover, different variants of the wild cluster bootstrap can over-reject or under-reject (MacKinnon and Webb (2019)).[40] To solve this problem and calculate p-values, following MacKinnon and Webb (2019), I use wild bootstrap randomization inference (WBRI).

5 Results

I analyze the direct impact of the 2006 economic sanctions on family income and the indirect effects on children's education.

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 rates, and employment. The 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 and energy supply through unemployment, inflation, and falling wages. Indeed, the raw data shows the reduction in income of households that the head works in either the oil and gas industry or energy supply industry. The real median annual income of households that the head works in the oil and gas industry decreased from 133 to 65 million Rials (-51%).[41] The reduction in household income can be related to a decline in working hours or wage rate (or both). The working hours have not changed over the years of sanctions. However, the average real wage per hour in this industry decreased from 44 in 2006 to 23 thousand Rials in 2013 (-48%). Also, households in the energy supply experienced a reduction in their income by 38% (21% reduction in 2008).

Table 2 lists the estimated effect of the sanctions on family income under various model specifications. All specifications include dummy variables for year, industry, and province. Models 2 and 3 include covariates including head's education, age, and age square, with the latter using a province by year set of fixed effects. The results are similar between all specifications. Referring to the specification of model 2,[42] total income and labor income of families that the head works in treated industries decreased by 15% and 16% relative to families in control industries, respectively (panel A). Panel B shows that the real wage rates in the treated industries relative to control industries decreased by 12% after the sanctions. In fact, the nominal wage rates increased, but it had not been synchronized with the rate of inflation. There is no significant effect on working hours. In particular, the

[40] 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 (2019)).

[41] 46% reduction in the real average family income

[42] All estimates in the rest of the paper are based on the model 2 specification, though I show the results are not sensitive to different specifications using model 1 and 3 (Table **??**).

sanctions did not affect full/part-time employment. This reduction in income is independent of worker's abilities since it is due to a shock in the economy whose effects do not depend on skills and abilities.

Table 2: Effect on Family Income

Panel A: Real Family Income	Total Income			Labor Income		
	(1)	(2)	(3)	(1)	(2)	(3)
Treat × Post2007	-0.154***	-0.149***	-0.144***	-0.160***	-0.155***	-0.154***
	(0.036)	(0.026)	(0.020)	(0.035)	(0.025)	(0.022)
R-squared	0.197	0.429	0.460	0.173	0.348	0.372
Observations	20,731	20,731	20,731	20,731	20,731	20,731
Panel B:	Real Wage Rate			Weekly Working Hours		
	(1)	(2)	(3)	(1)	(2)	(3)
Treat × Post2007	-0.111***	-0.115***	-0.097***	-0.731	-0.654	-0.734
	(0.037)	(0.032)	(0.030)	(0.782)	(0.804)	(0.745)
R-squared	0.204	0.403	0.429	0.218	0.230	0.271
Observations	9,951	9,951	9,951	9,951	9,951	9,951
Demographic controls	No	Yes	Yes	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Province & Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Province × Year	No	No	Yes	No	No	Yes

Source: Iranian Households Income and Expenditures Surveys (HIES).

Note: The table presents estimated coefficients of $Treat \times Post2007$ (γ in equation (1)) for household income, wage rates, and weekly working hours for the respective model specification. Dependent variables (total income, labor income, and wage rates) are log transformed and deflated by consumer price index (CPI) which equals 100 in year 2011. The time period for household income (total and labor income) is 1995â2013. The time period for wage rates and weekly working hours is 2006â2013 because weekly working hours are not observable 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). Treated: oil and gas, energy supply industries. Synthetic control: information (0.148), education (0.169), and health (0.683) industries. *Significant at 10% level; **significant at 5% level; ***significant at 1% level.

5.1.1 Placebo Studies

To assess the credibility of my results, following Abadie et al. (2015), I examine in-space placebos. To do so, I apply the synthetic control method to every 11 industries in the donor pool, assuming it was treated in the year 2007. If the placebo effects are as large as the main estimate, then it is likely that the estimated effect on family income was observed by chance. Figure 2 panel (c) shows the distribution of the post/pre-treatment ratios of root mean squared prediction errors (RMSPE) for the actual treated group (the black one) and all the industries in the donor pool.[43] The actual treated unit (oil & gas and energy supply) clearly stands out with the highest RMSPE ratio. Also, Figure 2 panel (d) displays the average family income gap between the actual treated unit and its synthetic (bold line) as well as the respective gaps for placebo industries. Whereas there is no significant difference in the family income between the actual treated households and the synthetic control in the pre-sanction period, it experienced large negative effects over the years of sanctions. No other placebo industry experiences a similar change. Thus, the placebo tests suggest that these results are not due to chance. Section 6 provides further robustness checks by considering in-time placebos, different periods, and different model specifications (Table **??**).

[43] RMSPE measures the magnitude of the difference between each industry and its synthetic control in the outcome variable. A small preintervention RMSPE and a large postintervention RMSPE can be indicative of a large effect of the intervention (Abadie et al. (2015)).

Table 3: Effect on Education Expenditure of Households

Dependent Variable	Share (2006)	log	share	per child
A. Total Expenditure		-0.111***	-	-
		(0.016)		
B. Categories				
Education	2.14%	-0.575***	-0.051**	-0.565***
		(0.137)	(0.021)	(0.134)
School Tuition	21.02%	-0.393***	-0.033***	-0.384***
		(0.139)	(0.009)	(0.131)
University Tuition	51.02%	-0.713***	-0.010	-0.739***
		(0.381)	(0.026)	(0.378)
Books	39.54%	-0.344***	0.001	-0.337***
		(0.082)	(0.008)	(0.078)
Private Tutoring	6.21%	-0.537***	-0.020**	-0.337***
		(0.186)	(0.008)	(0.078)
non-Education	97.86%	-0.111***	0.002**	-
		(0.022)	(0.001)	
Observations		20,731		

Source: Iranian Households Income and Expenditures Surveys (HIES).
Note: Table presents estimated coefficients of Treat × Post2007 (γ in Eq (1)). Dependent variables are total family expenditure, spending on education (by item), and spending on non-education goods and services according to COICOP classification. Dependent variables are log transformed and deflated by CPI which equals 100 in year 2011 (education spending is deflated by the Education Price Index (EPI)). The sample includes households with children aged 6-24. For school, tuition the sample consists of all households with children aged 6-24 who have not graduated from high school. For university tuition, the sample consists of households with children aged 6-24 who have graduated from high school. For spending on books and private tutoring, I consider all households with children aged 6-24. The time period is 1995-2013. I control for covariates, as well as time, province, and industry fixed effects (model 2). 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). Treated: Oil and Gas, Energy Supply industries. Synthetic control: Information (0.148), Education (0.169), and Health (0.683) industries. *Significant at 10% level; **significant at 5% level; ***significant at 1% level.

5.2 Effect on Children's Education

In consequence of a reduction in the family income (-15%), affected households reduced their total expenditure by 11% (Table 3). Although spending decreased for most components, it did not decrease by the same rate. As Table 3 shows, households cut spending on education by 58% (the share decreased by 5%). 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 schools instead of private schools for their children. In this section, I evaluate the effects of the sanction on quantity of children's education (enrollment rates and completed years of education) and household spending on education.

5.2.1 Effect on Enrollment and Years of Schooling

First, I find the impact of the sanctions on the educational attainment measured by enrollment rates and years of schooling. Table 4 presents the effects on school enrollment, college attendance (any post-secondary programs), and years of schooling.[44]

[44]The sample for grades 1-9 and high school two are children of the age group who are officially eligible for enrollment in these grades (6-14 and 15-18 years old for grades 1-9 and high school two, respectively). The sample for college attendance is high school graduates who are under the typical college graduation age (≤24 years old).

Table 4: Effect on Enrollment Rates and Years of Education

	Enrollment in grade 1-9 (6-14 yr old)	Enrollment in High School two (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					
Treat × Post2007	0.001	-0.001	-0.048**	-0.262***	-0.117***
	(0.005)	(0.005)	(0.024)	(0.077)	(0.047)
R-squared	0.013	0.014	0.143	0.948	0.708
B. Allowing differences across gender					
Female × Treat × Post2007	0.001	-0.014*	0.007	0.127	-0.063
	(0.006)	(0.008)	(0.054)	(0.238)	(0.155)
Female × Treat	-0.003	0.010*	-0.009	-0.458***	-0.062
	(0.004)	(0.006)	(0.028)	(0.071)	(0.041)
Female	0.001	-0.002	-0.026*	0.358***	0.059**
	(0.002)	(0.002)	(0.016)	(0.044)	(0.026)
R-squared	0.010	0.012	0.145	0.956	0.739
Mean	98.95%	81.76%	59.59%	10.49	7.70
Observations	22,560	8,231	6,217	20,450	43,011

Source: Iranian Households Income and Expenditures Surveys (HIES).
Note: The table presents estimated coefficients of $Treat \times Post2007$ (γ in equation (1)) in panel A, and estimated coefficients of $Female \times Treat \times Post2007$ ($\gamma 1$ in equation (2))) in panel B for enrollment rates and years of schooling. The sample for this analysis is children of the age group that officially corresponds to each level. In panel B, I examine gender differences by estimating equation (2). For college enrollment, I limit the sample to high school graduates (HSG) who aged below 24. The time period is 1995â2013. I control for age, age-squared, and parentsâ education effects, as well as time, province, and industry fixed effects (model 2). Heteroskedasticityconsistent standard errors accounting for clustering at the province and industry level in parentheses. p-values are calculated using wild bootstrap randomization inference (WBRI). Treated: oil and gas, energy supply industries. Synthetic control: information (0.148), education (0.169), and health (0.683) industries. *Significant at 10% level; **significant at 5% level; ***significant at 1% level.

As, the third column of Table 4 shows, the probability of attending college significantly decreased by 4.8 percentage points after the sanctions. Also, years of schooling significantly decreased by 0.1 years for the whole sample (column 5), and decreased by 0.3 years for children aged 15-24 years who completed grade 9 (column 4).[45] In Iran, education is compulsory until the end of high school one (grade 9). Therefore, nonsignificant effect on enrollment in these grades (column 1) can interpret as the falsification test, because this group of children attends school anyway. Also, the sanctions had no significant effect on enrollment in high school two (column 2). 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. Panel B of Table 4 shows that the effects are not different across gender.

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); Akee et al. (2010); Bastian and

[45]The overall effect is to reduce schooling by 0.1 and 0.3 years among children age 6-24 and children age 15-24, respectively. Thus, it likely implies a positive effect for children age 6-14. It is correct that the effect for those age 6-14 is positive (0.005, but not significantly different from zero) . Since children 6-14 form 53% of total sample of children 6-24, the small positive effect on the subsample of children age 6-14, would increase the schooling by only 0.003 for total sample of children age 6-24 ($0.53 \times 0.005 = 0.003$). Similarly, since children 15-24 form 47% of total sample of children 6-24, the negative effect on the subsample of children age 15-24, would decrease the schooling by 0.123 for total sample of children age 6-24 ($0.47 \times -0.262 = -0.123$). 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 the percentage of children age 6-24 at different levels of education) confirms that at the prior rates of college attendance and enrollment at different education levels, years of education on average decreased by about 0.1 years after the sanctions. Also. the same calculations confirm a 0.3 reduction in years of education for children age 15-24, and a 0.005 increase in years of education for children age 6-14 (which is consistent with the estimated coefficients). Thus, the overall effect on years of schooling equals to ($0.53 \times 0.005 + 0.47 \times -0.262 = -0.1$)

Michelmore (2018); Blanden and Gregg (2004); Bleakley and Ferrie (2016); Coelli (2011); Hilger (2016); Løken (2010); Lovenheim (2011); Lovenheim and Reynolds (2013); Manoli and Turner (2018); Pan and Ost (2014)). My result is large compared to other studies. I find that a 15% decrease in family income is predicted to decrease college enrollments by 4.8 percentage points. The large effects estimated in this paper are expected because of the persistent shock and lack of adjustment possibilities to the shock. Acemoglu and Pischke (2001) find that a 10% increase in family income increases college enrollments by 1-1.4 percentage points. 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 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.

Back-of-the-envelope Calculation 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 education levels and lower wage rates. To find the children's earnings loss due to the sanctions, I compare the present value of future lifetime earnings of children with (I_s) and without the sanctions (I_{ns}).

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

where w_j^H and w_j^C are the percentage of children with a high school or less and the percentage of children with a university degree, respectively. T is the number of working years and β is the discount rate (0.95). For this calculation, I focus on the primary working ages (30-54).[46] I do not observe $Income^H$ and $Income^C$ (real annual income at different ages for high school graduates and college graduates) because 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 scenarios for changes in their education and income.[47] Similarly, I calculate the present value of lifetime earnings of the current generation using the annual income before and after the implementation of the sanctions to find parents' earnings loss.

The first exercise is to calculate what the expected magnitude of the children's income would be if the sanctions had not been imposed. The more reasonable scenario is that after the sanctions were lifted children would not go back to school and the wage rates do not recover. The results of cohort analysis in section 7 (Table 6, Panel B) provide evidence for this assumption that affected children did not return to school. Also, this assumption that the wage rates do not recover after the sanctions are lifted, is reasonable considering the fact that average real income has increased by only 5% between 2013 and 2019. In this scenario, I assume without the sanctions, the next

[46] Since HIES is a cross-sectional survey, I observe single-year measures of the earnings. Such short-run measures of workers' earnings may 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). $t = 0$ is related to age 30 and $T = 24$ is related to age 54.

[47] For example for future income I consider (i) median/average of (all/treated) workers' income in the last year of sanctions (year 2013), and (ii) median/average of (all/treated) workers' income before the sanctions (year 2006).

generation in the treated group could have enrolled in college at the same rate as the college enrollment in year 2006 (last year before the sanction) and had the average wage rates of workers in the treated industry observed in year 2006 (assuming they could have similar wage rates to their parents at corresponding ages). I compare these two cases using the population share of workers with high school diploma and college degree and their average real income at different ages in years 2006 and 2013 (for example, real $\text{Income}^{C}_{age30,2006} = 93.6$ million Rials and real $\text{Income}^{C}_{age30,2013} = 61.6$ million Rials), I find that without the sanction children could have had a 1.4 trillion Rials real lifetime income that will be only 824 million Rials as a result of sanction. Thus, this back of the envelope calculation shows a 41% reduction in children's lifetime earnings.[48]

It is also interesting to ask how large is the children's income loss in economic terms? To assess the size of this loss, I compare it with earnings loss of the current workers due to the sanctions. My calculations suggest that a one-dollar reduction in parents' permanent earnings leads to a subsequent reduction in children's earnings of 1.2 dollars.[49] 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 Iranian GDP over the years of sanctions. 45% of this reduction comes from decreased earnings for the current workers, and 55% comes from decreased earnings for the next generation. It suggests that the cost estimates using only the earnings of the 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 earnings.

5.2.2 Effect on Education Spending

So far, I have looked at the educational attainment measured by the enrollment rates and years of education. Now, I examine the effect of the sanctions on investment in children's education measured by household spending on education.[50] The education spending is the explicit costs associated with payments in cash such as (primary/secondary) school tuition, university tuition, books, private tutoring, donations, and other education expenditures (for instance extra classes). The average percentage of family educational spending was about 2% over the pre-sanction period (Source: Calculation by the author using HIES).[51] The primary/secondary school tuition fee constituted a significant proportion of total education costs (21%).

Table 3 presents the effect of the sanctions on education spending by items (they include zero for non-enrolled

[48] I also decompose the total effect of the sanctions on the children's 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 under different scenarios the effect of reduction in education on the lifetime earnings is relatively smaller than that of the wage reduction.

[49] This effect is larger than 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.

[50] 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.

[51] For Canada and the UK, the percentages were about 1.1 and 1.2%, respectively in 2009. Moreover, the average household expenditure on education as a percentage of total household income was 1.95 percent in the Unites States over 1990-1991 (Huston (1995)). For the 25 EU countries, the average private expenditure on education as a percentage of total household consumption during 1995-2004 is about 1% (range from 0.1 to 2.9%) (Lin and Lin (2012)). The share of education expenditure in household expenditure is 4.3% in India (Azam and Kingdon (2013)).

children. I add one to the values of these variables and then log-transformed).[52] As this table shows, households cut spending on education by 58%. In particular, households spent less on primary/secondary school tuition by 40% (its share decreased by 3%.). Knowing that the sanctions did not affect enrollment in primary and high schools, 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. Although, I do not observe the type of schools (private vs public) in the data, I use family spending on school tuition as a proxy for choosing private school for at least one child in the household and find a 5 percentage points reduction in choosing private schools among treated households compared to control households (Table S6).[53]

Moreover, households spent 71% less on university tuition, which is consistent with a reduction in college enrollment found in the previous section. Although the number of seats at public universities is limited, a shift from private universities to public universities may explain some of this reduction in spending on university tuition. For example, before the sanctions, some students in large cities would choose to remain in their cities and enroll in private universities instead of enrolling in public universities in small cities. The reduction in family income could force these students to move to fee-free public universities. The immigration data does not support this hypothesis. Although the type of university is not observable in the data, the cross-country migration for education decreased from 9% before the sanction to only 4% in 2012. As is explained above, the sanctions had no significant effect on total immigration and distribution of different migrant groups.

Also, spending on books and private tutoring decreased by 34% and 54%, respectively. A large share of spending on private tutoring is for pre-university students to increase their probability of success at the public universities entrance examination. Thus, children from treated households faced a decline in the financial resource available to enroll in private universities, as well as a reduction in receiving private tutoring, which may have increased their chance of getting placed in a public university.

An important concern is that the decline of fertility in Iran over the past decades could explain the reduction in household education spending. The average number of students in households decreased from 2.2 in 1995-2006 (pre-sanctions) to 1.5 in the 2007-2013 period (post-sanctions).[54] To check for this possibility, I evaluate the effect of the sanctions on education spending per child (Table 3, the last column). The results show reductions in education spending for each child, in particular per child spending on school and university tuition significantly decreased by 38% and 74%, respectively.

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 of being a child in a treated household after the sanctions conditional on being in a treated household and the time effects. As explained above, since HIES is a cross-sectional survey, I observe single-year measures of

[52] For school tuition, the sample consists of all children aged 6-24 who have not graduated from high school. For university tuition, the sample consists of high school graduates aged below 24 years. For spending on books and private tutoring, I consider all children aged 6-24.

[53] Also, as Panel B of Table **??** shows, treated households with more girls are less likely to choose a private school for their children after the sanctions than households with more boys.

[54] The average number of children in households who are enrolled in schools (primary and high schools) and universities decreased from 2 to 1.4 and from 0.2 to 0.1, respectively.

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:

$$ln(Edu\_exp_{ispt}) = \alpha + \xi ln(\widehat{Total\_exp_{ispt}}) + \beta\ Treat_i + \lambda_t + X'_{ispt}\delta + \phi_p + \psi_s + \varepsilon_{ispt} \quad (4)$$

where Edu_exp_{ispt} is household education spending of household i in industry s and province p at time t. $ln(\widehat{Total\_exp_{ipt}})$ (as a proxy for the family income) is the fitted value of total household expenditure derived from the the first stage equation given by:

$$ln(Total\_exp_{ispt}) = \upsilon + \gamma\ (Treat_i \times Post2007_t) + \iota\ Treat_i + \kappa_t + X'_{ispt}\nu + \varphi_p + \Psi_s + \varsigma_{ispt} \quad (5)$$

The vector X'_{ispt} is a set of family specific characteristics that are correlated with both educational spending and income e.g., parents' education. Since education spending and total household expenditure are both in logarithmic form, ξ denotes elasticity.

Table 5 presents the maximum likelihood estimation results of Eq (4) and Eq (5) (the unconditional marginal effects). I find that income elasticity is significantly greater than one (3.284). Thus, as total expenditure decreases, education spending decreases more rapidly. The F-statistic in the corresponding first-stage regression is far above the critical value, indicating that the used instrument is relevant.

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	3.284**	2.049**
	(1.374)	(0.850)
First stage		
IV: Treat × Post2007	-0.111***	-0.149***
F-stat	353.74	553.24
R-squared	0.316	0.419
Observations	20,731	20,731

Source: Iranian Households Income and Expenditures Surveys (HIES).

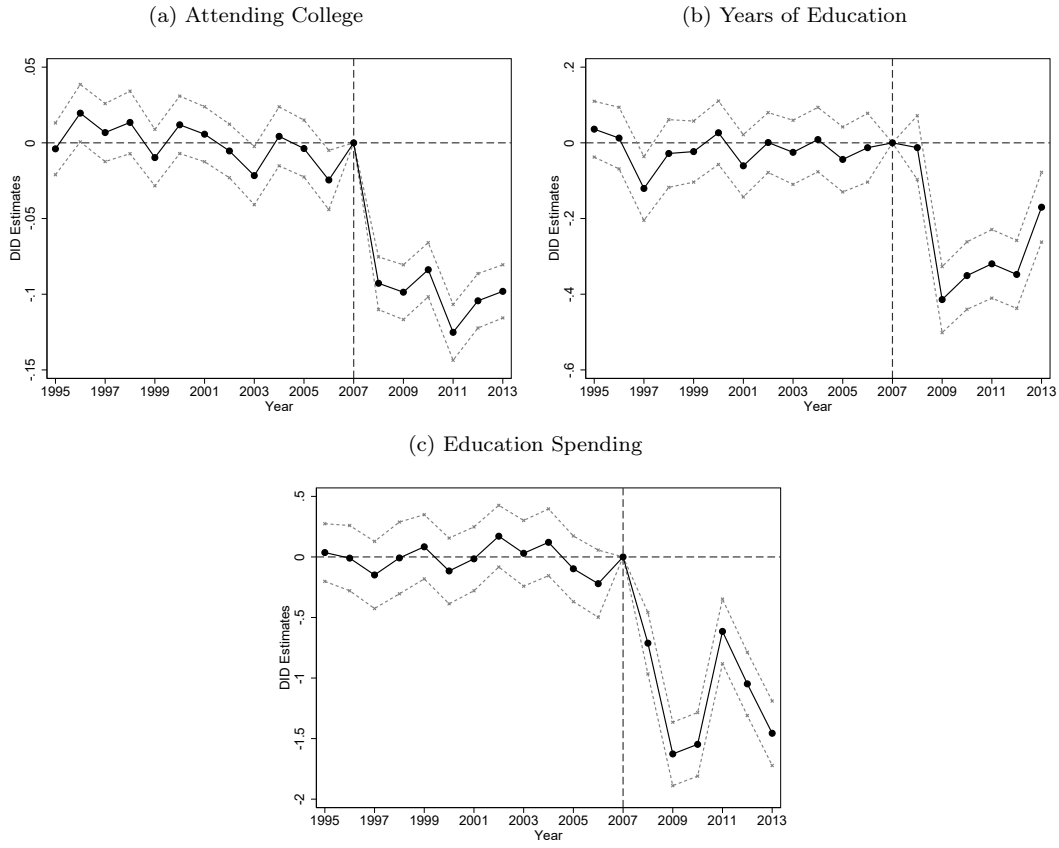
Note: Table presents estimated coefficients of Eq (4) and (5). Dependent variable is Ln(Household Spending on Education). Since education spending and total household expenditure are both in logarithmic form, the estimated coefficient (ξ) denotes elasticity. I also use family income itself as a robustness check. Additional controls include household size, head's age, and head's education. 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). Treated: Oil and Gas, Energy Supply industries. Synthetic control: Information (0.148), Education (0.169), and Health (0.683) industries. *Significant at 10% level; **significant at 5% level; ***significant at 1% level.

This negative effect on education spending is large compared to studies that find the income elasticity of education spending (Acar et al. (2016); Huy (2012); Qian and Smyth (2011)). While the results of these studies are different across countries, levels of family income, and other household characteristics such as parents' occupation, 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 groups of households that education is a luxury good, income elasticity is less than two. I find an income elasticity of more than three. Using family income, the estimated elasticity of

education spending is smaller (2.049), but still large compared to existing studies (the last column of Table 5).

5.2.4 Event Study

Figure 3: Event Study (Dynamic Effects)



Source: Iranian Households Income and Expenditure Surveys (HIES).
Note: Figure reports the DID estimates of dynamic effects on outcome variables (coefficients of the interaction Treat × year in Eq (3), 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. Treated: Oil and Gas, Energy Supply industries. Synthetic control: Information (0.148), Education (0.169), and Health (0.683) industries.

Figure 3 shows the difference-in-difference (DID) estimates of dynamic effects on outcome variables: college attendance, years of schooling, and education spending per child (coefficients of the year treatment interaction terms in Eq (3), with 95-percent confidence interval). The estimated coefficients fluctuate around 0 before 2007 thereby providing evidence that there is no significant difference between children of households in treated and control groups in terms of these outcome variables before the sanctions. As Figure 3 panel (a) shows, coefficients for college attendance turn significantly negative immediately after the sanction imposed in 2007. The decline in college attendance led to a decrease in the years of education of affected children. As Figure 3 panel (b) shows, this reduction happened with one-year lag. Moreover, Figure 3 panel (c) shows a decline in education spending of households working in treatment industries relative to households in control industries. This negative effect temporarily decreased in 2011. That decrease may have been the result of a targeted subsidy plan institute in December 2010. The goal of the subsidy reform was to replace energy subsidies with targeted social assistance. As

part of this reform, the government distributed $40 per person/month (i.e., 455,000 Rials/month) to all Iranians starting in December 2010.[55] These payments provided households with more income that could have been invested in education. Treated households' spending on education temporarily increased in 2011 as they spent this extra income on their children's education. However, the fixed payment amount and high inflation rates in the following years effectively decreased the purchasing power of this subsidy, likely reducing its impact on education spending. This trend can be interpreted as additional evidence of the high income elasticity of education spending.

6 Robustness Checks

I provide two groups of robustness checks. First, I examine the sensitivity of the composition of the synthetic control group to alternative implementations of the SCM (different matching variables, different matching year range, different methods for selecting weights, and different selected donor industries). The results in Table **??** show that the original synthetic control and the main results are not sensitive to these choices. Second, I consider several robustness checks of the main results including in-time placebos, considering different periods (1995-2015, excluding the years 2007 and 2009), and using various model specifications. I also use SCM at the industry-sector level because the effects of the sanctions are different across the public and private sectors. My results pass these robustness tests. Finally, I discuss whether the estimated effects are related to the sanctions or other changes in economic and political factors.

As an in-time placebo test, following Abadie et al. (2015), I estimate the effects by reassigning the sanctions to occur during the pre-sanctions period. Although my results show a substantial effect of sanctions on outcome variables, such findings would not be valid if the SCM also estimated significant effects for fake treatment years. I rerun the model for the case when the sanctions are reassigned in the year 2000. Figure 2 panel (e) displays the results of this in-time placebo study. The synthetic control almost exactly reproduces the family income for the treated industries over the 1995-2000 period. Most importantly, the family income trajectories of treated industries and its synthetic control do not diverge considerably during the 2000-2006 period. Table **??** panel B lists the insignificant effects on family income and children's education outcomes using this placebo synthetic control.

Table S5 also reports the results of other robustness checks (Panels C-I). For the main analysis, I restrict the data to the 1995-2013 period 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 in 2013. I re-conduct the analysis using a different period including 2014 and 2015 (Panel C). 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 were imposed unexpectedly (Panel D). Second, I exclude 2009 because the 2009 presidential elections in Iran and the U.S. could affect the Iranian economy (Panel E). For all cases, the results are close to the original results. In particular, the 2009 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. 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

[55] The average transfer received by an Iranian household in 2011 is about 13% of average family income.

spending. I re-conduct the analysis by including children from rural areas and find similar results (Panel F) mostly because only 6% of households in treated and control industries live in rural areas (most workers in rural areas work in agriculture and construction industries). Even most teachers and physicians who work at rural schools and hospitals live in a neighboring city and the travel to their workplace. Since there are not enough observations of treated and control households in rural areas, estimating the heterogeneous effects of sanctions across urban-rural areas is not possible. Finally, I estimate the effects of the sanctions under two different model specifications (Panel G and H). Model 1 excludes all 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. Model 3, includes covariates and a province-by-year set of fixed effects. Overall, these sensitivity tests verify the robustness of the original results.

I also apply SCM at the industry-sector level. In addition to the different effects of the sanction across industries, these effects are different across the public and private sectors. In particular, workers in the private sector of many industries experience more reduction in their income. For the treated group, households in the oil and gas industry and energy supply in both public and private sectors are affected by the sanctions in the same way. Using industry-sector level synthetic control analysis, the optimal weights are positive for information (public: 0.071), education (public: 0.180), health (public: 0.640, private: 0.055), and other service activities (public: 0.053) industries. Table **??** (Panel I) reports the effects on family income and children's education outcomes using the synthetic control at the industry-sector level. As Table shows, the total income and education spending of treated households decreased by 17% and 70%, respectively. Also, the sanctions decreased college enrollment and years of schooling by 5.7 percentage points and 0.2 years (0.3 years for children age 15-24 who completed grade 9), respectively. Overall, the results are larger than those using synthetic control at the industry level because households in the private sector of several industries that indirectly affected by the sanctions are removed from the synthetic control group.

6.1 Other Factors

To make sure the estimated effects are solely due to the sanctions, I check whether there were other changes in economic or political factors that affected the treated and control groups differently. First, I discuss two events (the 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. Moreover, the reduction in households' income started immediately after imposing sanctions in 2007 before the recession started in 2008. 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.

Moreover, as Borszik (2016) shows no major political changes took place during the years of sanctions, and 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's strategic preferences. In particular, 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)).[56]

7 Heterogeneous Effects of the Economic Sanctions

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

In this section, I present the impact of the sanctions on children's education by exposure and age.[57] Children from different cohorts were exposed differently depending on how their schooling years overlapped with the sanctions. Early exposure and greater exposure to the sanctions may result in different effects on education. For example, a six years old child in 2007 would be fully exposed in the sense potentially affected by all years of sanctions. But for a toddler age two in 2007 who has not started school yet or someone age 24 in 2007 who already completed their education, the effect can be different. Also, the effects can be different among those children who are fully exposed to the sanctions. For example, both six years old children in 2007 and 14 years old children in 2007 are fully exposed to the sanction for seven years, former are affected by sanctions during their compulsory education, and later experienced the sanctions at crucial ages: high school dropout age (16 years old) and matriculation at a university (18 years old). The date of birth and parents' industry workplace jointly determine a child's exposure to the sanctions. Thus, I use differential age-of-first-exposure and exposure-duration to find heterogeneous effects by exposure to the sanctions. Given the small sample by age, I define only three cohorts based on age in 2007 for each outcome variable: early-exposed treated cohort, fully-exposed treated cohort, and not-exposed cohort. For example, children aged 25 or older in 2007 are not exposed to the sanctions because they had completed their education; children aged 16-20 were exposed the entire time they were in college; college education of children aged 12-15 was exposed early but not fully to the sanctions as they turn age 18 (the average age of matriculation) during the years of sanctions.[58] The cohort classifications are different for other outcome variables. For example, for attending and completing high school two, children aged 21 or older in 2007 are not exposed to the sanctions because the maximum age eligible for completing high school education is 20;[59] children aged 13-16 were exposed the entire time they were in high school; high school education of children aged 10-12 was exposed early but not fully to the sanctions as they turn age 16 (high school dropout age) during the years of sanctions and completed

[56] Although there were no major changes in Iran's policies over 2005-2013, 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)). In June 2013, the moderate Hassan Rouhani won the presidential election. President Rouhani's campaign promised to improve the economic growth and unemployment. He also emphasized the need to negotiate with the Security Council over the nuclear program by highlighting the negative effects of the UN sanctions on Iran's economy. President Rouhani and his team were successful in finalizing the nuclear deal and terminating the sanctions.

[57] Section **??** in the supplementary online appendix provides more heterogeneous estimates.

[58] Also, the college education of children aged 21-24 is partially affected by the sanctions as these individuals may drop out from college or postpone graduation.

[59] There are age restrictions for enrollment at each grade. Detail information is available on the website of the Ministry of Education (in Persian).

high school after lifting the sanctions. Following Duflo (2001), I also use a cohort DID strategy and estimate the effects on each cohort using the following equation and observations from the last year of the sanctions (2013):

$$Y_{ispc} = \alpha + \sum_{l=early,fully} \gamma_l \left(Treat_i \times cohort_{il}\right) + \rho_c + X'_{ispt}\delta + \phi_p + \psi_s + \varepsilon_{isp} \quad (6)$$

where $cohort_{il}$ is a dummy that indicates whether individual i belongs to cohort l (early-exposed or fully-exposed) and ρ_c is a vector of cohort fixed effects. As in the main analysis, $Treat_i$ is a dummy for households in treated industries. Y_{ispc} is the quantity of education (enrollment, completing different levels of education, and years of schooling). Each γ_l can be interpreted as an estimate of the effect on a given cohort (the not-exposed cohort serves as the baseline).

Table 6: Heterogeneous Effect on Enrollment rates and Years of Education by Exposure

	Enrollment in	High School two		College		Years of
	grade 1-9	Ever Attending	Completing	Ever Attending	Completing	Education
Panel A						
Sample:						
Early Exposed Treated: Age in 2007	1-5	10-12	12-15	12-15	12-15	10-15
Fully Exposed Treated: Age in 2007	6-15	13-16	16-20	16-20	16-20	16-20
Early-Exposed	0.058	-0.007	-0.008	-0.017***	-0.003	-0.265***
	(0.063)	(0.007)	(0.012)	(0.005)	(0.002)	(0.052)
Fully-Exposed	0.019	-0.007	-0.037	-0.061***	-0.033***	-0.353***
	(0.015)	(0.007)	(0.033)	(0.010)	(0.007)	(0.078)
Prob > chi2	0.235	0.988	0.414	0.073	0.039	0.609
Panel B: Cohort analysis (2013, 2019)						
2013: Early-Exposed (γ_1)	0.069	-0.021	-0.151	-0.013	-0.072	-0.229**
	(0.046)	(0.021)	(0.103)	(0.057)	(0.058)	(0.095)
Fully-Exposed (γ_2)	0.045	-0.022	-0.152*	-0.171***	-0.244***	-0.543***
	(0.047)	(0.018)	(0.077)	(0.048)	(0.079)	(0.120)
Prob > F	0.230	0.899	0.996	0.009	0.031	0.020
2019: Early-Exposed (γ_1)	0.063	-0.003	-0.010	-0.024	-0.171***	-0.583*
	(0.041)	(0.064)	(0.048)	(0.047)	(0.061)	(0.396)
Fully-Exposed (γ_2)	0.042	-0.066	-0.029	-0.242***	-0.216***	-0.672**
	(0.050)	(0.071)	(0.040)	(0.050)	(0.070)	(0.327)
Prob > F	0.558	0.248	0.747	0.000	0.500	0.784
Panel C: Placebo Test						
Sample:						
Fake Treated: Age in 2007	16-22	21-24	21-24	25-30	25-30	25-30
Not Exposed: Age in 2007	23-26	25-30	25-30	31-35	31-35	31-35
Fake Treated × Treated Industries	0.003	0.002	0.015	0.012	0.016	0.121
	(0.006)	(0.005)	(0.042)	(0.018)	(0.019)	(0.078)

Source: Iranian Households Income and Expenditures Surveys (HIES).

Note: The table presents the heterogeneous effects by exposure to the sanctions. Panel A presents estimated coefficients of $Treat \times cohort$ ($\gamma 1$ in equation (6)) using observations from years 2013 and 2019. The dependent variable for attending any level of education is an indicator for whether an individual ever enrolls in a specified education level (including students and graduates). The dependent variable for completing high school and college is an indicator for whether an individual graduated. Panel B presents estimated coefficients of $Treat \times cohort$ ($\gamma 1$ in equation (6)). Panel C shows the results of a placebo test considering fake treatment cohorts. 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). Treated: oil and gas, energy supply industries. Synthetic control: information (0.148), education (0.169), and health (0.683) industries. *Significant at 10% level; **significant at 5% level; ***significant at 1% level.

Table 6 Panel A and B present the effects on the quantity of education (enrollment, completing different levels

of education, and years of schooling) by exposure to the sanctions.[60] Overall, these results show that children who were more exposed to the sanction when they were of schooling age obtained fewer years of education and were less likely to enroll and complete a college program. As Panel A shows, negative consequences of the sanction are larger for children who were exposed longer to the sanctions. The years of schooling significantly decreased by 0.4 and 0.3 years among fully-exposed and early-exposed cohorts, respectively. Also, the probability of attending college significantly decreased among fully-exposed cohort by 6.1 percentage points and this effect is significantly larger than that of early-exposed cohort (1.7 percentage points). Another important concern is that affected children may just postpone completing their education and enter into college later. In this case, children would take more time to complete their degree, but still would complete it. To check for this possibility, I conduct a similar analysis for college completion and I find that longer exposure to the sanctions decreases the probability of completing college by 3.3 pp. The fully-exposed cohort (aged 16-20 in 2007) are affected by the sanctions during their high school and university education. A lower quality of schooling, lack of access to private tutoring for the university entrance exam preparation, and lack of financial resources for college can explain this larger effect. There is no significant effect on enrollment in grades 1-9 and enrollment in high school two. Although attending high school two is not mandatory, the sanctions had no effect on it perhaps because fee-free public high schools are available and/or the outside option (e.g., employment opportunities for low educated people) is not high. Also, I did not find any significant effects on completing high school two perhaps because Iranian students follow the entry age cutoff rule set by the Iranian Ministry of Education. For high school two, the minimum and maximum entry ages are 15 and 18, respectively. Also, the maximum graduation age is 20.[61] Also, cohort analysis results in Panel B are consistent with earlier results in Panel A.[62] These results are robust to the use of different bandwidths of treated cohorts. Another important concern is that affected children may return to school after the sanctions are lifted. To check for this possibility, I use the data in 2019 (the most recent available data) and find similar results (Panel B) that indicate affected children did not return to school after the sanctions were lifted. Delay entry into college is rare among boys because non-student men older than 18 years old must go for two-year military service. While there is no such restriction for girls, I did not find any evidence of postponing going to college among affected girls. Also I did not find any significant effects on returning to high school in 2019 perhaps because of the age restriction for entering and completing high school. Panel C shows that the effects are small and not significantly different from zero for fake treatment cohorts

It is also helpful to understand the heterogeneity of the sanctions effects at different ages. Age plays an important role in the school enrollment decision. 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 years old). As Table 7 shows, the economic sanctions increased the probability of dropping out of high school. The

[60] Using HIES this analysis cannot be done for family education spending because in this data the share of total education spending on each child is not observable. Also, I cannot do this analysis for each grade because in the data education levels are reported based on ISCED (less than primary, primary, lower secondary, upper secondary, 2-year tertiary vocational education, Bachelor's, Master, and Ph.D.), thus the grades are not observable.

[61] Older people are not allowed to enroll or continue their education in high school two. Instead, they can enroll in the adult education system which is not available in all cities due to low demand. The data shows very few persons return to school as adults, and I did not find any significant change in enrollment in the adult education system.

[62] The results in Panel B are less precise because for cohort analysis a single year data is used, thus the sample size is smaller compared to that of Panel A. For example, the population of fully-exposed children (aged 16-20 in 2007) in Panel A is ten times larger than that in Panel B.

enrollment rate of children at high school dropout age (16 years old) decreased by 4.3 percentage points. 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. As Panel B shows, this effect is larger for girls. Moreover, the economic sanctions decrease the probability of attending college at age 18 by 15 percentage points, which is consistent with earlier results in section 5. Panel C presents estimates of the effects by exposure duration (measured by age at 2007). As Panel C shows, the negative effects on high school and college enrollemnt at crucial ages (ages 15 and 18) are bigger when children reach these ages at a later stage of the sanctions period. For example, children who reached age 16 in 2013 are more likely to dropout from high school than do children who reached age 16 in 2007.

Table 7: Heterogeneous Effect on Enrollment Rates by crucial ages

	Age		
	6	16	18 (HSG)
Sample: Whole sample			
A. No differences across gender			
Treat × Post2007	0.041	-0.043**	-0.154**
	(0.032)	(0.023)	(0.062)
R-squared	0.279	0.075	0.157
B. Allowing differences across gender			
Female × Treat × Post2007	0.043	-0.122***	-0.072
	(0.005)	(0.036)	(0.104)
Mean Control	91%	92%	47%
R-squared	0.029	0.076	0.169
Observations	1,019	3,011	1,437
Sample: Early & Fully Exposed: Age in 2007	1-15	10-20	12-24
C. Allowing differences in exposure			
Age at 2007 × Treat	-0.002	0.009**	0.018**
	(0.004)	(0.004)	(0.009)
Treat	-0.001	-0.133*	-0.294*
	(0.038)	(0.070)	(0.171)
R-squared	0.393	0.098	0.166
Observations	735	1,395	969

Source: Iranian Households Income and Expenditures Surveys (HIES).

Note: The table presents estimates of the average and gender differences effects of the sanctions on enrollment rates by crucial ages (estimated coefficients of $Treat \times Post2007$ (γ in equation (1)) in panel A, and estimated coefficients of $Female \times Treat \times Post2007$ ($\gamma 1$ in equation (2)) in panel B): enrollment in the first grade at age 6, high school at age 16 (high-school dropout age), and matriculation at a university at age 18 (among high school graduates (HSG)). The time period is 1995â2013. Heteroskedasticity-consistent standard errors accounting for clustering at the province and industry level in parentheses. Treated: oil and gas, energy supply industries. Synthetic control: information (0.148), education (0.169), and health (0.683) industries. *Significant at 10% level; **significant at 5% level; ***significant at 1% level. .

8 Conclusion

In this paper, I analyze the negative externalities of economic sanctions on the next generation through changes in children's education. 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 the 2006 UN economic sanctions against Iran on children's education. These targeted

sanctions were associated with large, sudden reductions in households' income that lasted 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, gas, and energy supply industries as the treated group, and the weighted average of information, education, and health industries as the synthetic control), the empirical analysis suggests that the sanctions had significant negative impacts on both educational attainment (quantity of education) and investment in education (quality of education). First, the sanctions decreased children's probability of attending college by 4.8 percentage points and years of schooling by 0.1 years. Second, households reduced spending on children's education by 58% - 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 had persistent effects on parent income. 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.

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 who were exposed longer to the sanctions and children at crucial ages. First, children who were more exposed to the sanction when they were of schooling age obtained fewer years of education and were less likely to go to college. Affected children did not go back to school after the sanctions were lifted. Second, the enrollment rate of children at high school dropout age (16 years old) and matriculation at a university (18 years old) decreased by 4.3 percentage points and 15.4 percentage points, respectively. These effects worsen when children reach the age of 16 and 18 at a later stage of the sanctions period.

This paper complements the literature documenting the negative effects of economic sanctions. Current studies show the negative effects of sanctions on economic growth and living standards and the humanitarian situation of the civilian population during the years of sanctions. In the case of Iran, Iran's economy got 15-20% smaller than it would have been absent the sanctions (U.S. Treasury Secretary Jacob Lew report, 2015) that led to a reduction in the total welfare of 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 cost estimates using only the earnings of the 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 over the years of sanctions.

The estimates presented in this paper suggest that although economic sanctions against Iran were successful in terms 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 the 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.

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Supplementary Online Appendix

The Intergenerational Effects of Economic Sanctions

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Online Appendix (not for publication)

In this online appendix, I present the detailed analysis of the validity of identification assumption and robustness checks which I left out of the main text. I also provide more heterogeneous effects and outline a simple model of investment in schooling to identify the channels through which sanctions affect children's education.

1 Identification Assumptions

In this appendix, I provide the detailed analysis of the validity of identification assumptions. Since the data is a repeated cross-sectional survey, I need to make sure the composition of the sample is 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 labor and household composition. For unobserved characteristics, I cluster observations at province and industry levels.

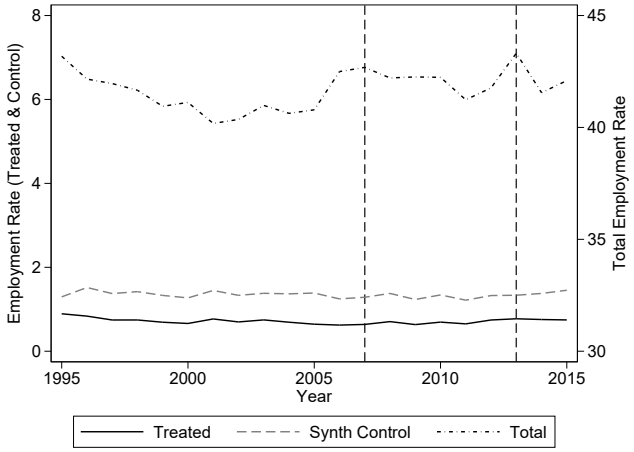
Labor Movement. First, I check whether the sanctions significantly affect moving workers across industries. Workers' movement across industries could bias estimates of sanctions effects obtained by comparing outcomes according to the family's head economic activity (Rosenzweig and Wolpin (1988)). As mentioned before, the 2006 UN sanctions mostly affected the oil and gas industry and energy supply. Since real wage decreased in these industries, it is possible that the workers in the affected industries 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 and job experience) of labor across industries in the main sample and a bigger sample from Iranian Labor Force Surveys (ILFS).

Figure 1 shows a stable employment rate over time in treated 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.

I also examine the effects of sanctions on job separation rates, job tenure, years of schooling, and skill index (the aggregate of years of schooling and work experience based on a principal component analysis) of employees and unemployed individuals who used to work in each industry using Iranian Labor Force Surveys (ILFS). Although the main data is a rotating panel, this feature of the panel can not be used in this study because households' id is changed for confidentiality purpose in the version available to researchers. Thus, I use another data Iranian Labor Force Surveys (ILFS). The advantage of ILFS data is that it

provides information on the former job of unemployed individuals, work experience, and job tenure.[1] Therefore, I can check quality and quantity of unemployed individuals who used to work in treated and control industries. As Table 1 shows, the sanctions had no significant effects on job separation rates and skills. In fact, only 7% of unemployed individuals who used to work in treated industries have left their job because their income was low and this percentage is constant over time.[2] Moreover, the sanctions had no significant effects on job tenure of employed people. If a significant share of workers in treated industries were to move to control industries, we would observe a reduction in job tenure among workers in control industries. Also, Figure 2 shows the DID estimates of dynamic effects on job separation and job tenure (coefficients of the interaction Treat $\times$ year in Eq (**??**), with 95-percent confidence interval). The estimated coefficients are not significantly different from zero before and after 2007.

Figure 1: Employment Rates (Total/Treated/ Synthetic Control)



Note: Figure displays a stable employment rates over time for the treated and synthetic control industries despite fluctuations in the total employment rate. *Source: Author's calculations from HEIS data.*

[1] 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.

[2] The reason of the leaving job for unemployed individuals (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) is reported in ILFS.

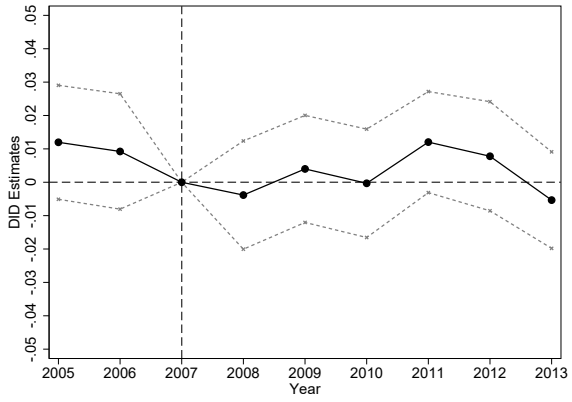
Table 1: Effect on Job Separation and Skills

			Years of Education		Skill Index	
	Job Separation	Job Tenure	Employees	Unemployed	Employees	Unemployed
Treat × Post	-0.005	-0.446	0.007	0.220	-0.266	-0.071
	(0.006)	(0.342)	(0.282)	(0.316)	(0.222)	(0.995)
R-squared	0.018	0.490	0.214	0.321	0.758	0.676
Observations	162,836	156,922	156,922	5,914	156,922	5,914

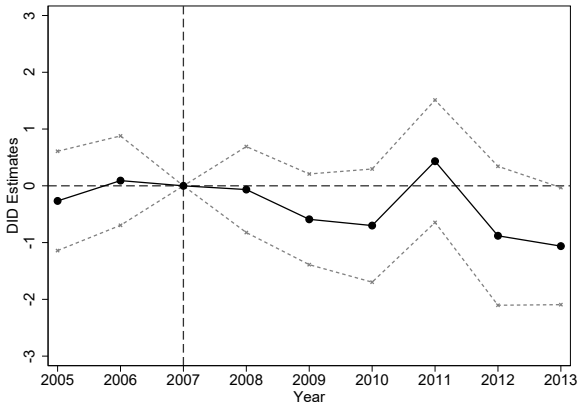
Notes: Table presents the effect of sanction on the separation rate, years of schooling, and a skill index for employees and unemployed individuals who use to work in treated or synthetic control. The sample is from Iranian Labor Force Surveys and time period is 2005-2013. Heteroskedasticity-consistent standard errors accounting for clustering at the province and industry level in parentheses. P-values are calculated using wild bootstrap randomization inference (WBRI). *Significant at 10% level; **significant at 5% level; ***significant at 1% level. Treated: Oil and Gas, Energy Supply industries; Synthetic control: Information (0.148), Education (0.169), and Health (0.683) industries, *Source: Author's calculations from ILFS data.*

Figure 2: The Dynamic Effects on the Job Separation and Job Tenure

(a) Job Separation



(b) Job Tenure



Note: Figure shows the DID estimates of dynamic effects on job separation (left) and job tenure (right) (coefficients of the interaction Treat × year in Eq (**??**), with 95-percent confidence interval). The estimated coefficients are not significantly different from zero before and after 2007.

Household Composition. I also check the effect of sanctions on the household composition. First, I check the balance of control variables. As Pei et al. (2019) show, a powerful test of the identifying this assumption is to put the control variable on the left-hand side of the regression (Eq (**??**)) instead of the outcome variable (balancing test). A zero coefficient on Treat × Post2007 confirms no change in children's and households' characteristics as a result of the sanctions. As Table 2 reports, the sanctions had no significant effect on gender, age, family size, head's education, and parent's employment status, thus the selection does not change in terms of these covariates.

Table 2: Balancing Test and Selection on Observables

	Children		Household			
	Female	Age	Family Size	Head's Education	Employed Mother	Employed Father
Treat × Post2007	0.018	0.750	-0.011	0.331	-0.022	-0.008
	(0.015)	(0.470)	(0.083)	(0.240)	(0.017)	(0.009)
R-squared	0.002	0.031	0.313	0.150	0.084	0.019
Observations	43,011	43,011	20,731	20,731	20,731	20,731

Notes: Table presents the coefficient of Treat × Post2007 from OLS regressions (Eq (??)) for children's and households' characteristics. The sample is households with 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. P-values are calculated using wild bootstrap randomization inference (WBRI). *Significant at 10% level; **significant at 5% level; ***significant at 1% level. Treated: Oil and Gas, Energy Supply industries; Synthetic control: Information (0.148), Education (0.169), and Health (0.683) industries.

Although the sanctions did not affect the family size (the number of observed people in the household), they could have an impact on the composition of households. 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 the household composition. As Table 3 shows, the sanctions had no significant effect on the household composition in terms of age and relation to the head. In particular, the sanctions had no effect on the probability with which young adults (18-24) live with their parents.

Table 3: The Effect of Sanctions on Household composition

	children		living with parents	relation to the head			
	0-5 yr old	6-17 yr old	(18-24 yr old)	child	parent	sibling	other
Treat × Post2007	0.011	0.001	-0.033	0.019	-0.000	0.001	0.001
	(0.021)	(0.021)	(0.024)	(0.010)	(0.000)	(0.001)	(0.001)
R-squared	0.129	0.107	0.310	0.016	0.003	0.004	0.003
Observations	20,731	20,731	10,037	20,731	20,731	20,731	20,731

Notes: Table presents the coefficient of Treat × Post2007 from OLS regressions (Eq (??)) for household composition in terms of age and relation to the head. The sample is households with 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. P-values are calculated using wild bootstrap randomization inference (WBRI). *Significant at 10% level; **significant at 5% level; ***significant at 1% level. Treated: Oil and Gas, Energy Supply industries; Synthetic control: Information (0.148), Education (0.169), and Health (0.683) industries.

2 Robustness Check

In this online appendix, I provide the detailed of robustness checks which I left out of the main text. As in the main text, I provide two groups of robustness checks. First, I assess the sensitivity of the synthetic control to alternative implementations of the SCM. Second, I present the results of several robustness checks of the main results.

2.1 Robustness Check for the Synthetic Control

In this section, I assess the sensitivity of the synthetic control to alternative implementations of the SCM. For the main analysis, I use pre-treatment family income as the matching variable in the 1995-2006 period and a nested optimization procedure. First, I evaluate the effects of the choice of matching variables, matching year range, and methods for selecting weights. Then, I check the sensitivity of results using different selected donor industries. Overall, these sensitivity tests verify the robustness of the original synthetic.

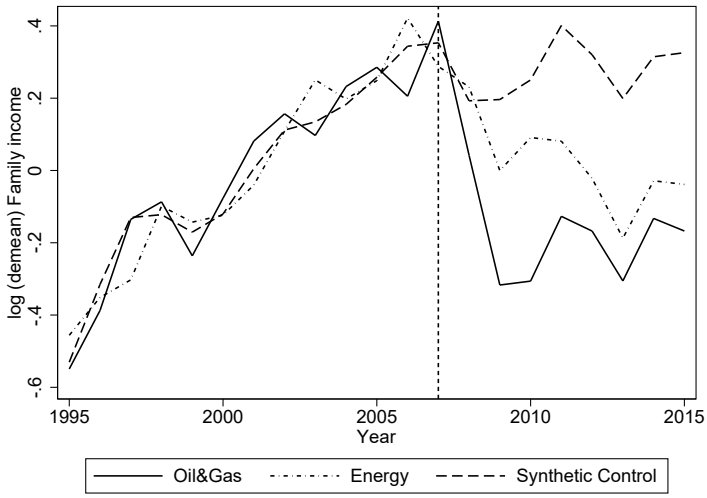
First, I test whether the composition of the synthetic control group is sensitive to the matching method. Following Cavallo et al. (2013), I check the validity of synthetic control for counterfactual by checking the sensitivity of results to the choice of matching variables. For the main analysis, following Botosaru and Ferman (2019), I consider only pre-treatment family income as the matching variable. As a robustness check, I include observed covariates (parent's education, employment status, age, etc.). I also include some (but not all: 2000-2006; 1995, 2000, 2006; 2000, 2006; 1995, 2000; 1995, 2006) lags of family income in the list of matching variables and check whether the synthetic control matches well the treated households. I also apply the synthetic control method limiting the years range to the 2000-2006 period. Moreover, I rerun the model using a data-driven regression based method (standard method) for selecting weights. This method is faster compared to the nested method and often yields satisfactory results in terms of minimizing the RMSPE. Table 4 (the first column) reports RMSPEs as a measure of the pretreatment fit for the different model choices. As the Table shows, changing the matching method has no large effect on matching results ($0.026 \leq \text{RMSPE} \leq 0.047$). In particular, I find that if more pre-treatment family income is included, the variable weights for covariates are small and ignorable. Overall, using different matchings, the synthetic controls closely match the treated industries in the pretreatment period. However, the choice of matchings could affect the results if it influences the selected industries for the synthetic control. Table 4 (panel A) lists the industries weights for different matchings. For all cases, the health industry receives the largest weight ($0.647 \leq w \leq 0.831$). In some cases, the weight of the information and education industries is zero and the weight of the water supply is positive. Although, the composition of the synthetic control group is not the same using different matching methods, as the

panel B shows, the main results are similar.

Second, I test whether the composition of the synthetic control group is sensitive to the selected donor industries. As explained above, I discard the financial, real estate, and administrative and support service industries from the donor pool. As a robustness check, I include these industries. The new synthetic control includes the real estate as well as information, education, and health industries (last row of Table 4). The RMSPE is 0.030, very close to the RMSPE of the original synthetic control (0.034). Although the results are not significantly different from the main results, they are biased because these households are unsuitable controls due to a large difference in their characteristics relative to treated households (Abadie (2019)). Moreover, since households in this industry experienced a reduction in their income (with a lag) over the years of sanction, using this synthetic control may result in an underestimation of the effects of the sanction. I also 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 (leave-one-out test) (Abadie et al. (2015)). The leave-one-out synthetics closely match the original synthetic control.

As explained above, I aggregate the treated units into a single unit (pooled SCM). Pooled SCM can yield poor unit-specific fits. Thus, I check the fit for each treated unit and find a good fit mostly because the characteristics of households in affected units are similar (Figure 3).

Figure 3: Real Median Income for Treated (by industry) and Synthetic Control



Note: Figure displays the real median family income for treated households (separated by industry) and synthetic control in the 1995-2013 period. Treated: Oil and Gas, Energy Supply industries. Synthetic control: Information (0.148), Education (0.169), and Health (0.683) industries

Table 4: Synthetic Control and Main Results using Various Matchings

		A. Weights					B. Main Results			
	RMSPE	Information	Education	Health	Water Supply	Real Estate	Family Income	Education Spending	Years of Schooling	Attending College
Main Model	0.034	0.148	0.169	0.683	-		-0.149***	-0.575***	-0.262***	-0.048**
(1) Different Matches										
Matching variables										
family income lags (some)	0.035	0.169	-	0.831	-		-0.130***	-0.537***	-0.329***	-0.031**
predictors, no lags	0.047	-	-	0.647	0.353		-0.125***	-0.535***	-0.367***	-0.028**
predictors + lags	0.035	0.148	0.169	0.683	-		-0.149***	-0.575***	-0.262***	-0.048**
Matching year range										
2000-2006	0.026	0.215	-	0.703	0.082		-0.125***	-0.524***	-0.347***	-0.031**
Method for selecting weights										
Standard	0.035	0.187	-	0.804	0.010		-0.129***	-0.534***	-0.332***	-0.031**
(2) Different Donor Pool	0.030	0.042	0.119	0.637	-	0.202	-0.137***	-0.567***	-0.278***	-0.050**

Notes: Table reports RMSPEs, synthetic control weights, and the main results using different model choices (different matching variables, matching year range, and methods for selecting weights) and different selected donor industries. The main model uses all pre-treatment family income in the 1995-2006 period and a nested optimization procedure over a donor industries excluding the financial, real estate, and administrative and support services industries. 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.

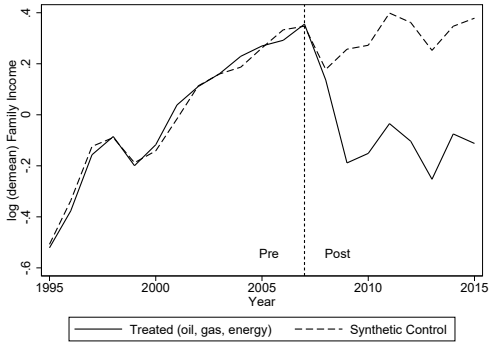
Treated: Oil and Gas, Energy Supply industries

2.2 Robustness Check for the Main Results

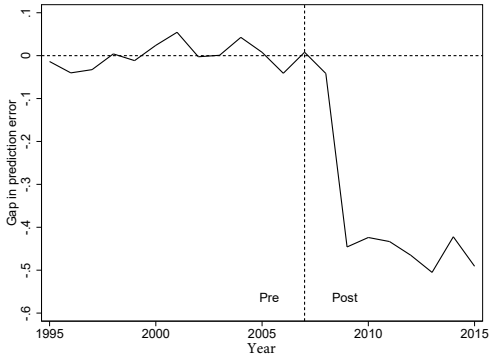
In this section, I present the results of several robustness checks of the main results (as discussed in section **??**) including in-time placebos, considering different periods (1995-2015, excluding the years 2007 and 2009), and using various model specifications (excluding all covariates (model 1), including a province by year set of fixed effects (model 3)). My main results pass these robustness tests. As an alternative approach, I apply the synthetic control analysis at the industry-sector level (explained in section **??**). Table 5 (Panel I) reports the effects on family income and children's education outcomes. Overall, the results are larger than those using synthetic control at the industry level because households in the private sector of several industries that indirectly affected by the sanctions are removed from the synthetic control group. In-space placebo tests for the other 22 industry-sectors suggest that these results are not due to chance (Figure 4(c) and 4(d)).

Figure 4: Real Median Family Income and Gaps at industry-sector level

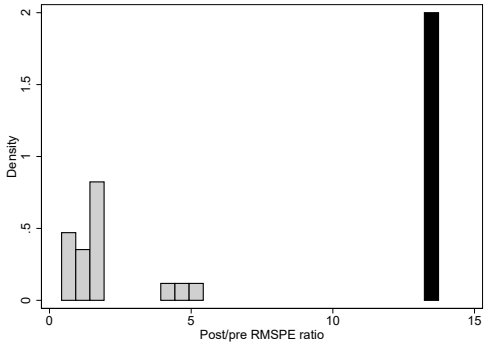
(a) Real Median Family Income



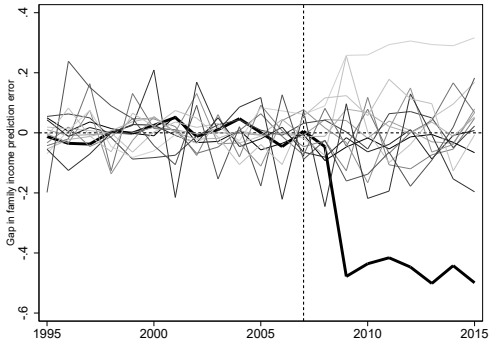
(b) Gap between Treated & Synthetic



(c) post/pre RMSPE ratio



(d) Gap between Treated & Synthetic



Note: Figure reports synthetic control method analyses as well as placeboes at industry-sector level in the 1995-2015 period. Figures (a) displays the average real family income for treated households in the oil and gas industry and energy supply (solid line) and the synthetic control (dashed line). Figures (b) shows the gap between actual treated and synthetic control. Figures (c) shows the post/pre RMSPE ratio for placebo estimates. The black ones indicate the post/pre RMSPE ratio using the actual treated industries. Figures (d) shows the gap between treated and synthetic control for placebo estimates, with actual treated industries in black solid line.

Table 5: Robustness Checks

	Family Income (log)	Education Expenditure (log)	Attending college (HSG,≤24 yr)	Years of Schooling (15-24 yr)	Years of Schooling (6-24 yr old)
A. Main Model					
Treat × Post2007	-0.149***	-0.575***	-0.048**	-0.262***	-0.117***
	(0.026)	(0.137)	(0.024)	(0.077)	(0.047)
B. Placebo Sanctions in the Year 2000					
Treat × Post2000	0.006	0.193	0.004	0.009	0.069
	(0.025)	(0.112)	(0.034)	(0.087)	(0.051)
C. Including 2014, 2015					
Treat × Post2007	-0.158***	-0.473***	-0.051**	-0.266***	-0.109***
	(0.026)	(0.124)	(0.022)	(0.069)	(0.042)
D. Excluding 2007 (the first year of sanction)					
Treat × Post2007	-0.149***	-0.541***	-0.044**	-0.259***	-0.108***
	(0.027)	(0.138)	(0.063)	(0.077)	(0.047)
E. Excluding 2009 (election year)					
Treat × Post2007	-0.146***	-0.457***	-0.033**	-0.208**	-0.083*
	(0.027)	(0.145)	(0.017)	(0.082)	(0.050)
F. Including Children in Rural Areas					
Treat × Post2007	-0.156***	-0.648***	-0.052**	-0.291***	-0.126***
	(0.026)	(0.133)	(0.024)	(0.075)	(0.046)
G. Model 1: Excluding Covariates					
Treat × Post2007	-0.154***	-0.851***	-0.030**	-0.169**	-0.437***
	(0.036)	(0.145)	(0.016)	(0.081)	(0.085)
H. Model 3: Including Province × Year FEs					
Treat × Post2007	-0.144***	-0.533***	-0.045**	-0.183**	-0.123**
	(0.020)	(0.143)	(0.017)	(0.087)	(0.049)
I. Industry-Sector Level Analysis					
Treat × Post2007	-0.170***	-0.702***	-0.057**	-0.338***	-0.161***
	(0.027)	(0.137)	(0.025)	(0.078)	(0.047)

Notes: Table presents the results of robustness tests including in-time placebos, considering different periods (1995-2015, excluding the years 2007 and 2009), and using various model specifications. The main model estimates the effect of the actual sanctions in 2007 over the 1995-2013 period by controlling various covariates and dummy variables for year, industry, and province. Panel H presents estimated coefficients of Eq(**??**) using synthetic control method (SCM) at the industry-sector level. Family income and education expenditure are log transformed and deflated by CPI which equals 100 in year 2011. The sample is households with children aged 6 to 24. 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.

Treated: Oil and Gas, Energy Supply industries

Synthetic control: Information (0.148), Education (0.169), and Health (0.683) industries

Synthetic control (for the placebo sanctions in the year 2000): Education (0.483) and Health (0.517) industries

Synthetic control at industry-sector level: Information (public: 0.071), Education (public: 0.180), Health (public: 0.640, private: 0.055), and Other Service Activities (public: 0.053) industries

The main results show households cut spending on school tuition by 40% (Table **??**). Knowing that the sanctions did not affect enrollment in primary and high schools, 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. I do not observe the type of schools (private vs public) in the data. However, since the public schools are fee-free, if a household's spending on school tuition in positive, I can say that at least one child in the household goes to a private school. Thus, I use the whole sample and family spending on school tuition as a proxy for choosing private school for at least one child in a family. I define a dummy variable which is 0 if all children in a family go to public school (family spending on school tuition is zero) and 1 if at least one child in a family go to a private school. As Table 6 shows, the probability of parents choosing a private school for their children decreases by 5 percentage points (3.7 percentage points among single-child families) following the imposition of sanctions. Also, as Panel B shows, treated households with more girls are less likely to choose a private school for their children after the sanctions than households with more boys.[3]

Table 6: School Choice: Private vs Public

	whole sample	single-child families
A. No differences across gender		
Treat × Post2007	-0.050***	-0.037*
	(0.015)	(0.021)
R-squared	0.050	0.050
B. Allowing differences across gender		
Female × Treat × Post2007	-0.352**	-0.072**
	(0.151)	(0.033)
R-squared	0.051	0.053
Observations	16,934	8,041

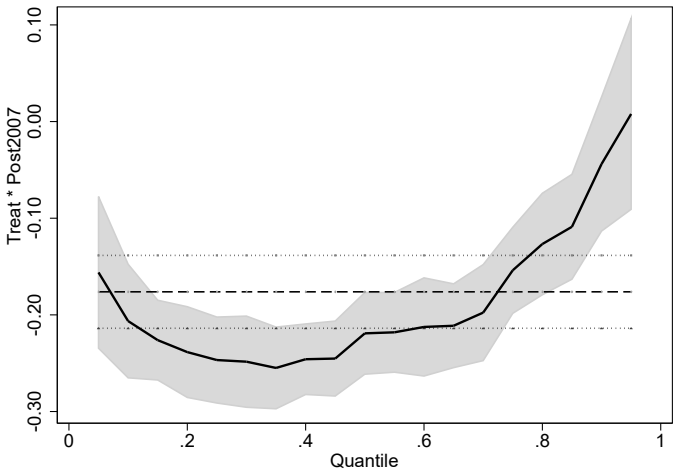
Notes: Table presents the estimated effects on choosing school type (private vs public). Dependent variable is an indicator for whether the family spending on school tuition is positive, thus at least one child in the family goes to a private school. Heteroskedasticity-consistent standard errors accounting 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.
Treated: Oil and Gas, Energy Supply industries
Synthetic control: Information (0.148), Education (0.169), and Health (0.683) industries

[3] An alternative approach is identifying the school type for a sub-sample of children. For single-child families, household's spending on school tuition must be spending their only child to private school. For multiple-child families, it is still, in some cases, possible to identify whether child is attending private or public schools, e.g., if there are two children in a household, one attending primary school and another high school, and this household reports spending no money on primary school and a positive amount on high school tuition, I can say that the younger child goes to a public primary school and the older child goes to a private high school. In such a way, I can identify school type for 47% of households in the main sample, most of whom are single-child family. Using this subsample, I find a reduction in the probability of treated parents choosing a private school for their children after the imposition of sanctions. Since the number of children is a parents' choice and single-child families spend more on their child education, this subsample is most likely not representative.

3 Heterogeneous Effects

In this appendix, I provide more heterogeneous Effects of the sanctions (by other family financial resources). I first examine how the sanctions affected family income across different quantiles. Table 7 (and Figure 5) presents estimated coefficients from OLS and quantile regression for family income. As this Table shows, the effect of sanctions on the income of low and middle-income households is larger (24% and 20%, respectively) and significantly different from the average effect (15%).

Figure 5: Heterogeneous Effects on Family Income



Note: Figure displays the heterogeneous effects of the sanctions on family income (coefficients of $Treat \times Post2007$ in Eq (??), with 95-percent confidence interval). The solid line shows quantile coefficients. The horizontal dash line shows the OLS coefficient (the average effects of sanctions on family income). The Dependent variable (total family income) is log transformed and deflated by CPI which equals 100 in year 2011. Treated: Oil and Gas, Energy Supply industries. Synthetic control: Information (0.148), Education (0.169), and Health (0.683) industries

Table 7: Heterogeneous Effect on Family Income

	Average Effect	Quantile Regression			
	(OLS)	0.25	0.50	0.75	0.90
Treat × Post2007	-0.149***	-0.237^{+***}	-0.200^{+***}	-0.129***	-0.025^{+}
	(0.026)	(0.022)	(0.023)	(0.028)	(0.038)
R-squared	0.429	0.117	0.117	0.120	0.110
Observations	20,731	5,183	10,366	5,182	2,073

Notes: Table presents estimated coefficients from OLS and quantile regression for family income. Dependent variable (total family income) 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. *Significant at 10% level; **significant at 5% level; ***significant at 1% level. $^{+}$Significantly different from OLS coefficient at the 5% significant level. Treated: Oil and Gas, Energy Supply industries. Synthetic control: Information (0.148), Education (0.169), and Health (0.683) industries.

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 non-labor income. I approximate wealth using an asset index based on Filmer and Pritchett (1999) which aggregates various assets of a household including durable goods (car, bicycle, TV, radio, etc.) and housing ownership and characteristics (size, number of rooms, appliances, etc.).[4] I also group individuals based on their family non-labor income which is summation of the non-labor income of each family member including financial transferred aids, real estate incomes, subsidies, interests on bank deposits, bonds yield and share dividends, scholarships and cash gifts from others. Table 9 shows that the wealth index and non-labor income (and their components) are not affected by the sanctions.

Table 8 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 (aged 6-24) from the 25th percentile (in total family wealth and non-labor income) experienced 0.2 years reduction 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 54%-61%. 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.[5] Overall, children from low-wealth families are more affected in terms of educational attainment, and children from middle-wealth families are more affected in terms of investment in education.

[4] I use principal component analysis (PCA) for driving weights.

[5] 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).

Table 8: Heterogeneous Effect on Education by percentiles of Family Resources

	Family Wealth			Family non-labor Income		
	<25th	25-95	>95th	<25th	25-95	>95th
A. Years of Schooling						
Treat × Post2007	-0.235**	-0.105	0.066	-0.225**	-0.038	0.323
	(0.113)	(0.097)	(0.308)	(-0.110)	(0.096)	(0.221)
R-squared	0.676	0.732	0.792	0.683	0.721	0.834
Observations	11,701	31,699	2,347	11,496	31,963	2,288
B. Education Spending						
Treat × Post2007	-0.399	-0.613***	1.551	-0.021	-0.539***	0.365
	(0.350)	(0.150)	(1.309)	(0.362)	(0.153)	(1.029)
R-squared	0.139	0.089	0.064	0.157	0.099	0.048
Observations	5,478	14,237	1,014	5,067	14,662	1,000

Notes: Table presents the effects on years of schooling and household education spending over the wealth and non-labor income distributions. The sample for this analysis is all households with 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. P-values are calculated using wild bootstrap randomization inference (WBRI). *Significant at 10% level; **significant at 5% level; ***significant at 1% level. Treated: Oil and Gas, Energy Supply industries. Synthetic control: Information (0.148), Education (0.169), and Health (0.683) industries

Table 9: Sanction Effects on Wealth Index, non-Labor Income, and Savings

Dependent Variable		Treat × Post2007	s.e.
A. Wealth Index		-1.641	(3.284)
components:	durable goods	0.042	(0.045)
	housing ownership	-0.013	(0.021)
	housing characteristics	-1.641	(3.284)
B. non-Labor Income	(*log*)	-0.100	(0.112)
	(*share*)	-0.001	(0.006)
components (log):	scholarships and cash gifts	0.018	(0.106)
	transferred aids	-0.035	(0.118)
	interest on bank deposits, bonds yield, and share dividends	-0.082	(0.519)
	real estate incomes	0.227	(0.196)
C. Savings	(*log*)	0.535	0.364)
	(*share*)	0.007	(0.019)
D. Debt	(*log*)	-0.569	(0.374)
	(*share*)	-0.056	(0.242)

Notes: Table presents the coefficient of Treat × Post2007 from OLS regressions (Eq (??)) for a wealth index, non-labor income, savings, and debt. Non-labor incomes, savings, and debt are log transformed and deflated by CPI which equals 100 in year 2011. The share values are share of total family income. I calculate the saving by subtracting total consumption from total family income.The sample is households with children aged 6 to 24 and 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. Treated: Oil and Gas, Energy Supply industries. Synthetic control: Information (0.148), Education (0.169), and Health (0.683) industries

4 Model

In this appendix, I outline a simple model of investment in schooling based on Acemoglu and Pischke (2001) to identify the channels (illustrated in the text) through which sanctions affect children's education. People live for two periods. In the first period, 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:

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

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 payoffs (in terms of higher income later for their children) from their investment in children's education. Parents may value children's education for several 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. 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 the first period ($s \geq 0$). Therefore, the cost of investment in children's education is lower consumption in the first period. If parents' income and/or return to education are high enough, parents would like to spend on their children's education. First order conditions of the households

optimization problem are:

$$
\begin{aligned}
&u_c = \lambda \\
&u_{c'} = \mu \\
&h_E = \lambda exp_E - \mu w^e - \mu w^e w^q QEdu \\
&h_Q = \lambda exp_Q - \mu w^e w^q Edu \\
&c + exp + s = y \\
&c' = w^u + w^e Edu + w^e w^q Edu\ QEdu + s
\end{aligned}
$$

Thus, total derivatives are:

$$
\begin{aligned}
&u_{cc} dc + u_{cc'} dc' = d\lambda \\
&u_{cc'} dc + u_{c'c'} dc' = d\mu \\
&h_{EE} dE + h_{EQ} dQ = \lambda dexp_E + exp_E d\lambda - w^e d\mu - \mu w^e w^q dQ - \mu w^e\ QEdu\ dw^q \\
&\qquad\qquad - \mu(1 + w^q\ QEdu) dw^e - w^e w^q\ QEdu\ d\mu \\
&h_{EQ} dE + h_{QQ} dQ = \lambda dexp_Q + exp_Q d\lambda - \mu w^e w^q dE - \mu w^e\ Edu\ dw^q - \mu w^q\ Edu dw^e - w^e w^q\ Edu\ d\mu \\
&dc + dexp_E + dexp_Q + ds = dy \\
&dc' = dw^u + w^e dE + Edu dw^e + w^e w^q Edu\ dQ + w^e w^q\ QEdu\ dE + w^e Edu\ QEdu\ dw^q \\
&\qquad + w^e Edu\ QEdu\ dw^q + ds
\end{aligned}
$$

Or:

$$
A \begin{bmatrix} dc \\ dc' \\ d\lambda \\ d\mu \\ dEdu \\ dQEdu \\ ds \end{bmatrix} = \begin{bmatrix} dy \\ dw^u \\ dw^e \\ dw^q \end{bmatrix}
$$

where A is a the coefficient matrix. Thus, from this comparative static analysis:

$$
\begin{aligned}
dEdu &= constant + f_y(.)dy + f_e(.)dw^e + f_q(.)dw^q + f_u(.)dw^u \\
dQEdu &= constant + g_y(.)dy + g_e(.)dw^e + g_q(.)dw^q + g_u(.)dw^u
\end{aligned}
$$

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.

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. Therefore, in this paper, I empirically tested the overall impact of sanctions on children's education.

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