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The Effect of Parental Job Loss on Child School Dropout: Evidence from the Occupied Palestinian Territories

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ABSTRACT

The Effect of Parental Job Loss on Child School Dropout: Evidence from the Occupied Palestinian Territories*

We study the effect of parental job loss on child school dropout in developing countries. We focus on Palestinian households living in the Occupied Palestinian Territories and having the household head employed in Israel during the Second Intifada (2000-2006). We exploit quarterly variation in conflict intensity across districts in the OPT to instrument for Palestinian workers' job loss in Israel. Our 2SLS results show that parental job loss increases child school dropout probability by 9 percentage points. The effect varies with child and household characteristics. We provide evidence that the effect operates through the job loss-induced reduction in household income.

JEL Classification: H56, I20, J63

Keywords: job loss, school dropout, conflict, Second Intifada, Occupied Palestinian Territories, Israel

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1 Introduction

How do negative economic shocks affect household schooling decisions? To answer this question, we focus on parental job loss, one of the most traumatic economic shocks a household can experience, and explore its effect on child school dropout. This is a particularly relevant issue in developing countries. Where markets are not perfect, even temporary negative income shocks may have an impact on household education choices. Moreover, in these contexts, decisions as that of withdrawing the child from school are more difficult to revert and, therefore, more likely to have effects on the human capital accumulation process, with possibly large consequences on the child's future welfare.

Job loss has negative consequences on adults in terms of health and earnings (Farre et al., 2018; Schaller and Stevens, 2015; Sullivan and von Wachter, 2009). The effects can be dramatic also for children. Evidence from developed countries indicates that parental job loss has long-run adverse effects on children's level of human capital and future income (Hilger, 2016; Oreopoulos et al., 2008). Moreover, it may affect children educational outcomes also in the short-run. Children exposed to parental job loss have higher probability of grade repetition (Stevens and Shaller, 2011), lower grades (Rege et al., 2011; Ruiz-Valenzuela, 2015), and lower likelihood of enrolling at the university (Coelli, 2011).

While these outcomes are well documented for advanced economies, there is a lack of evidence on the effects of parental job loss on children educational outcomes in developing countries. The reason for this lack of evidence is twofold. First, panel data are rarely available for developing countries. Second, identifying a credibly exogenous source of variation for job loss is very challenging in these contexts. For instance, plant closures and mass layoffs, often used as instrumental variables in this literature, are relatively uncommon in developing countries.

In this paper we investigate the causal effect of parental job loss on child school dropout in the context of a developing country, namely the Occupied Palestinian Territories (hereafter OPT). The OPT provides a unique setting for our analysis for three main reasons. First, the rotating panel structure of the Palestinian Labor Force Survey

makes it possible to look at the immediate change (i.e. between two consecutive quarters) in both the employment status of the household head and the enrollment status of the child. Second, it provides a convenient sample to study the effect of parental job loss on children school dropout. This is the sample of Palestinian children attending school in the OPT and having the household head employed in Israel during the Second Intifada.¹ By looking at Palestinian workers employed in Israel, we focus on a group of workers for whom job loss is arguably involuntary, having always been largely determined by changes in the level of violence of the Israeli-Palestinian conflict (World Bank, 2006).² Finally, education is highly valued in the OPT and the Palestinian education system proved to be very resilient to adverse conditions. Despite the conflict situation and the difficulties of the Palestinian economy (including physical disruptions of various sorts and low levels of public investment), the education system continued to operate during the whole Second Intifada (Di Maio and Nandi, 2013; Brueck et al., 2019). The fact that schooling in OPT is robust to severe local disturbances makes it an interesting setting where to study parental employment effects on education choices.

By taking advantage of the characteristics of our sample, our identification strategy exploits the over-time variation in conflict intensity across districts in the OPT as an instrumental variable for a Palestinian worker’s job loss in Israel. We measure conflict intensity as the quarterly per-capita number of Palestinians killed by the Israeli Defense Forces (IDF) in a worker’s district of residence. Higher conflict intensity increases Palestinian workers’ probability of losing their job in Israel for at least two different reasons. First, it may make it more difficult to reach the workplace in Israel, possibly increasing absenteeism.³ Second, it may induce Israeli employers to fire Palestinian workers in retal-

¹The Second Intifada was a period of intensified violence between the Israeli Defense Forces (IDF) and the Palestinians which took place between 2000 and 2006. Section 2 provides background information on the history of the Israeli-Palestinian conflict and on the Second Intifada.

²The involuntarily nature of job loss for these workers is also suggested by the fact that Palestinian employed in Israel earn a substantial wage premium which has increased during the Second Intifada. PLFS data indicate that during the Second Intifada wages of Palestinian employed in Israel were on average 15% higher than wages of Palestinian employed in the OPT, controlling for age, education level, employment sector, and type of occupation. Moreover, data indicate that - during the conflict - real wages of Palestinians workers employed in Israel for two consecutive quarters have increased by 3% on average, significantly more than those of workers employed in the OPT.

³Palestinian workers are not allowed stay overnight in Israel and have to commute daily by crossing the check points at the border between Israel and the OPT (see Section 2.1).

iation for conflict events. Our first-stage regression results indicate that conflict intensity is in fact a strong predictor of job loss for Palestinian workers employed in Israel.

The validity of our instrument relies on the assumption that conflict has no direct effect on child school dropout other than through parental job loss (i.e. the exclusion restriction). To provide support to this assumption, we perform a placebo test looking at the effect of conflict intensity on school dropout for children with household head employed in the OPT and compare it with that for children in our sample (i.e. those who have the household head employed in Israel). If conflict had a direct effect on school dropout, we would find that observationally identical children going to school in the same district in the OPT - thus exposed to the same conflict intensity - should be affected in the same way, regardless of their household head being employed in Israel or in the OPT.⁴ This is not the case. Conflict increases school dropout only for children with the household head employed in Israel while there is no effect for children with the household head employed in the OPT. Moreover, we find that the effect of conflict on school dropout mirrors the effect of conflict on job loss. Conflict increases job loss for Palestinian employed in Israel (our first stage result), whereas it does not increase job loss for those employed in the OPT. All these results are consistent with conflict intensity affecting school dropout through parental job loss.

While we interpret this as convincing evidence supporting the exclusion restriction, we cannot rule out that there may be other ways through which conflict may affect school dropout. However, based on the results of our test, we can exclude all channels related to conflict-induced changes in the supply of education, such as a worsening in the quality of school infrastructures. Moreover, our results suggest that we could also exclude all mechanisms related to changes in the demand for education due to conflict-induced changes in the local-level conditions, e.g., an increase in the severity of mobility restrictions imposed by the IDF (possibly increasing the difficulty of reaching schools).

Next, we address two additional potential threats to our identification strategy. First, children may dropout from school to fight against the IDF rather than as a consequence

⁴Note that Palestinians living in the OPT cannot send their children to school in Israel.

of parental job loss or, similarly, conflict may be fueled by dropout students. Second, households may decide to relocate due to conflict, with households of high-skilled workers moving to districts exposed to lower number of fatalities. We provide suggestive evidence ruling out both a feedback mechanism linking school dropout to conflict intensity and any compositional effect at the geographical level associated with conflict intensity, hence boosting confidence in the results of the present analysis.

Our 2SLS estimates indicate a positive and significant effect of parental job loss on child school dropout. As for the magnitude of the effect, we find that parental job loss increases child school dropout probability by 9 percentage points. This result is robust to a number of checks. These are the inclusion of: another proxy for conflict intensity, which may confound the effect of the number of fatalities; a large set of additional household head and household controls; non-linearities in both the control variables and the instrument; the full set of district-specific time trends to account for time-varying unobservable factors at the local level. The result also holds when using an alternative definition of job loss, alternative samples, an alternative instrument, while it vanishes when randomly generated fatalities are used to proxy for conflict intensity in a falsification exercise.

Next, we explore possible heterogeneity in the effect of parental job loss on child school dropout. Our analysis shows that the effect is larger for male children and students with lower academic ability. This suggests that parental job loss may lead to child labour as a household coping strategy and that households are more likely to withdraw the child from school if the expected returns to schooling are lower. Moreover, we find that the effect is larger for children with low educated household heads and children in larger households. This indicates that a household's response to negative economic shocks may depend on how parents value education and on the per-capita resources available for education investment in the household. Importantly, these heterogeneous effects of parental job loss on child school dropout are consistent with the 2SLS estimates being relatively larger in magnitude than the OLS ones, in line with the Local Average Treatment Effect (LATE) interpretation of our 2SLS result.

Finally, we examine the potential mechanisms whereby parental job loss can affect

child school dropout. We provide evidence suggesting that the effect operates through the job loss-induced reduction in household income. This is not surprising considering that Palestinians working in Israel are mostly low-skilled workers employed in the construction, agricultural, and manufacturing sectors and therefore are not from wealthy households. Consistent with this explanation, we also present some evidence that children dropping out of school are more likely to start working. Instead, we do not find evidence of alternative mechanisms, such as household head becoming a fighter, family disruption (i.e. parental divorce), or household residential relocation.

Our paper is related to three strands of literature. The first is the literature on the effects of parental job loss on children education outcomes. Most of these studies focus on job loss in developed countries and look at outcomes such as grade repetition, graduation points, and likelihood of enrolling at the university (Stevens and Shaller, 2011; Rege et al. 2011; Coelli, 2009). Only two papers examine the effect of job loss on children schooling in developing countries.⁵ Skoufias and Parker (2006) find a negative association between parental job loss and attendance for female students during the Mexico peso crisis. Duryea et al. (2007) show that during economic crises in Brazil father job loss is correlated with a higher child school dropout probability. Differently from these studies, our analysis explicitly addresses the endogeneity issue by adopting an instrumental variable approach.

The second strand of literature relevant for our paper is the one on the economic

⁵There are various reasons why parental job loss is expected to have a larger effect on child school dropout in a developing country context with respect to a developed one. First, the effect of job loss is likely to be more binding on the household budget constraint. In developing countries, inefficient credit markets severely limit the possibility for the household to smooth even temporary negative income shocks. At the same time, job losers in developing countries usually do not have access to forms of government support such as unemployment benefits. This implies that job loss may significantly reduce the households ability to cover education costs. In fact, while primary education is formally free in most of developing countries, there are still not negligible costs to attend school (e.g. transportation, school uniform, stationery, etc.). In these contexts, these costs may simply become prohibitive after household head job loss. Second, it is more likely that - to cope with the household income drop - the child needs to abandon school to start working (Jacoby and Skoufias, 1997; Dehejia and Gatti 2005; Edmonds, 2008). While this is unlikely in developed countries, it is not so in developing ones where mandatory education and child labor laws are often not enforced vigilantly. Finally, the effect of job loss on school dropout is expected to be larger in developing countries because the decision to abandon school is more difficult to revert. Even short-term interruptions of schooling may easily become permanent in contexts where programs to support students with learning gaps are almost completely lacking. Moreover, since improvements in intergenerational education attainment is a predictor of intergenerational socioeconomic mobility (Narayan et al., 2018), this implies that the consequent slowdown in the human capital accumulation process is likely to have a larger negative impact on future welfare in contexts where insurance mechanisms are not available or do not work properly.

determinants of child schooling. Previous research has shown the importance of child characteristics (age, gender, health), parental characteristics, and school quality (Alderman et al. 2001; Handa, 2002). In particular, household income has been shown to be a key determinant of different schooling outcomes, including enrolment, test score, and attainment (Dostie and Jayaraman, 2006). Our study contributes to this line of research by investigating how education investment decisions respond to negative (even if possibly temporary) economic shocks affecting households in a developing country.

Finally, our paper is related to the literature on the effect of the Palestinian-Israeli conflict on education outcomes in the OPT. Two previous studies have looked at the impact of the Second Intifada on education focusing on the West Bank. Di Maio and Nandi (2013) find that IDF-imposed mobility restrictions (i.e. the closure of the OPT borders) while increasing child labour - have no significant effect on school attendance for children 10-14 in the West Bank. While close in spirit, this paper differs from ours under important aspects including the outcomes considered and the sample of analysis. Moreover, because of the finding of the null effect of conflict on school attendance, Di Maio and Nandi (2013) do not discuss the possible mechanisms through which conflict may affect child schooling decisions. Brueck et al. (2019) instead look at the effect of conflict intensity on academic achievement in the West Bank. They show that the locality-level number of fatalities reduces the test score at the final exam for high-school students (17-18 years old) in the West Bank. At the same time, they document that conflict intensity has instead no effect on enrollment, attendance, or school dropout. Interestingly, the findings from both these studies can be interpreted as providing further empirical support for our exclusion restrictions.

The paper proceeds as follows. Section 2 provides some background on the Israeli-Palestinian conflict and on the Palestinian school system. Section 3 describes the data. Section 4 presents the econometric model and discusses the identification strategy. In section 5, we present our main results, the robustness checks, the heterogeneity analysis, and the possible mechanisms explaining our main result. Section 6 concludes.

2 Background

2.1 Palestinian workers in Israel and the Second Intifada

As a consequence of the dependence of the OPT economy from the Israeli one, the dynamic of the Palestinian labor market has always been influenced by the amount of job opportunities in Israel (Angrist, 1996; Kadri and MacMillen, 1998; Mansour, 2010). During the years, the number of Palestinian workers employed in Israel has responded to major political events, such as the First Palestinian Uprising (1987-1993), but also to changes in Israeli regulations of work permits and security policies. The possibility to be employed in Israel has always been subject to holding a work permit and Palestinian workers have to commute daily because regulations prohibit them from staying overnight in Israel.⁶ Nonetheless, by the late 1990s, more than 25% of the Palestinian labour force was employed in Israel, accounting for one sixth of Palestinian national income (Ruppert Bulmer, 2003). Palestinian workers have traditionally mainly supplied labor services in the construction, agriculture, and tourism sectors. Yet, being employed in Israel is very attractive for Palestinian workers: *ceteris paribus*, wages in Israel are significantly higher, between 10% and 25% (IMF, 2003; World Bank, 2004; Mansour, 2010). In September 2000, after some years of relative stability, the security situation rapidly deteriorated and the Second Intifada (the so-called al-Aqsa Intifada) started.⁷ In the following months, there was a rapid increase in violent events from both side of the conflict. The Israeli Government adopted a number of security measures, including the limitation of the movement of Palestinians within and outside the OPT.⁸ As a result, the number of Palestinian

⁶To obtain work permits for Palestinian workers, Israeli employers submit petitions to the Israeli Ministry of Interior. The request is typically approved if the employer has not violated relevant Israeli labor regulations and if the relevant quota is not yet filled. Quota are set by the Israeli government for each sector (construction, agriculture, etc.). As described in Etkes (2012), work permits are granted to Palestinians who pass a security check and meet certain age and personal status criteria, which are imposed to reduce the likelihood of their participation in terrorist attacks against Israelis. Among these conditions, there is to be married and have at least one child (Berda, 2012).

⁷There is no established end date for the Second Intifada. However, violence between IDF and Palestinians decreased substantially after 2006. Jaeger and Paserman (2008) provide detailed description of the different periods of violence during the Second Intifada.

⁸These limitations include: a reduction in the number of work permits issued to Palestinians, a change of rules to get the permit (from a system based on age and marital status to individual permits), curfews imposed on Palestinian cities, internal and external borders closures, and the building (started in 2002) of a separation wall between Israel and the West Bank. (World Bank, 2004; United Nations, 2005).

workers commuting to Israel sharply dropped and Palestinian employment in Israel decreased. In the West Bank, wages decreased and employment increased. The conflict situation also led to an increase in job separation for Arab-Israeli in Israel (Abrahams, 2015; Adnan, 2015; Cali and Miaari, 2013; Di Maio and Nandi, 2013; Mansour, 2010; Miaari et al. 2012). Between 2000 and 2006, Palestinians killed 234 Israeli civilians and 226 IDF personnel in the OPT while the IDF caused more than 4,000 Palestinian fatalities, the majority of them non-combatants (B'TSELEM, 2007).

2.2 The Palestinian school system during the Second Intifada

Since the 1994 Oslo Accords, the education system in the OPT is managed by the Palestinian Ministry of Education and Higher Education (MoEHE). The academic year begins in September and ends in June. The education system is divided into two levels: primary - including elementary school and middle school - (grades 1 to 10) and secondary (grades 11 to 12). Grades 1 to 10 are compulsory, implying that all Palestinian children between 6 and 15 years old are expected to be in school. Instead, grades 11 and 12 are not mandatory. At the end of the secondary school, students take a final exam (Tawjihi General Examination) which is required to access the university (UNESCO, 2007)

The Palestinian society places a high value on education. This is reflected in enrolment and attendance rates which are high by regional and global standards reaching 98% for primary education and 85% for secondary education (PCBS, 2006; Sharek Youth Foundation, 2009). The high value placed on schooling extends also to girl education. Gender participation rates show perfect equality in education access for both primary and secondary school (50.4% and 51.7%, respectively). Gender-based participation levels are broadly equivalent in the West Bank and the Gaza Strip, where 54.5% and 52.3% of the students are female, respectively.

Differently from what usually happens in conflict-affected countries, the education system in the OPT continued to fully operate during the Second Intifada (Nicolai, 2007). For instance, some school construction took place and a new Palestinian curriculum was implemented in those years (PCBS, 2006). However, as any other aspect of the Palestinian

economy, also the education system has faced numerous difficulties. Schools have been damaged and occupied by the IDF. Teachers and students have been victim of violent events (World Bank, 2004; United Nations, 2005; MoEHE and UNESCO, 2005). Military-imposed mobility restrictions - such as checkpoints and physical barriers - made it more difficult to reach the schools. School days have been lost or shortened because of security reasons. At the same time, explicit and implicit costs of schooling have increased. Due to the difficult general economic situation, school fees - while far from being prohibitive - have become to be hardship for an increasing number of families (Nicolai, 2007).⁹ Indirect costs of attending school - such as food, books, transportation prices - have risen as well (IMF, 2003; WFP and FAO, 2007). Yet, the aggregate number of students enrolled has not declined during the conflict period. In fact, between 2000 and 2006, the total number of Palestinian students in basic education in the OPT increased from 830,765 to 944,713. Education attendance increased from 95% to 98% for 6-11 years old, from 96% to 97% for 12-14 years old, and from 74% to 85% for 15-17 years old.¹⁰

3 Data

Labour force survey and children data Our main source of data is the Palestinian Labour Force Survey (PLFS) administered by the Palestinian Central Bureau of Statistics (PCBS). The PLFS is a quarterly representative household survey of Palestinians living in the OPT (West Bank and Gaza Strip) collecting data on individuals aged at least 16, which is the minimum working age in the OPT. The PLFS is a quarterly rotating panel in which households are surveyed four times over six quarters: they are surveyed for two consecutive quarters, dropped in the next two quarters, and then surveyed again for two consecutive quarters. Although the survey is not designed for longitudinal analysis, the rotating design makes it possible to match individuals across waves.

To construct our dataset, we combine the PLFS data for the Second Intifada period

⁹Although education is officially free in the OPT, students traditionally contribute with a donation - effectively a school fee. These fees form some 50 per cent of schools' budgets and are needed to make the schools continue to operate (Nicolai, 2007)

¹⁰Data taken from the PCBS website http://www.pcbs.gov.ps/Portals/_Rainbow/Documents/Education-1994-2013-09E.htm.

(2000-2006) with additional confidential information - not provided in the publicly available PLFS - on children aged 10-15 for the same period. We consider both students in compulsory and non compulsory school grades (before university), thus our final sample includes Palestinian children aged 10-17 who, at the time of the first interview, are attending school and whose household head is working in Israel.¹¹ The rotating panel structure of the PLFS (and of the confidential children data) allows us define our two main variables of interest (*Household head job loss* and *Child school dropout*) as the employment and the education status change between two consecutive quarters for the household head and the child, respectively (see below for the formal definitions). Finally, to complement our analysis, we use information from the Child Labour Survey administered by the PCBS in 2004. While data are available only for one year, the survey provides detailed information on parental and household characteristics, including household income and the marital status of parents.

Conflict events data Data on conflict-related Palestinian fatalities during the Second Intifada are provided by the Israeli NGO B'Tselem (B'Tselem, 2007). The B'Tselem dataset provides a rich set of information, such as age, gender, and place of residence of the killed, the date, place, and a description of the circumstances of the event. Data are based on a number of sources and validated by several cross-checks. For this reason, they are considered to be accurate and reliable by both the Israelis and the Palestinians and have been previously used by other scholars studying the Israeli-Palestinian conflict (see for instance Amodio and Di Maio, 2018; Mansour and Reis, 2012; Jaeger and Paserman, 2008). In our analysis, we measure conflict intensity using the district-level number of Palestinians fatalities caused by the IDF per 10,000 inhabitants by quarter. We also use additional data on other military-motivated security measures implemented by the IDF, namely the external border closures. During closure days movements of workers and goods between the OPT and Israel, as well as between the West Bank and the Gaza Strip, are completely banned since all permits previously issued to residents of the OPT

¹¹The publicly distributed version of the PLFS dataset does not distinguish between being employed in Israel or in Israeli settlements in the OPT. However, our arguments on the effect of conflict on job loss for those employed in Israel do apply also to those employed in Israeli settlements.

for purposes of work, trade, or medical treatment are invalid. Data on the yearly number of closure days of the border between Israel and the OPT are also provided by B'Tselem.

Table 1 reports the descriptive statistics for the main variables included in the analysis. Our sample includes 9,539 Palestinian children who are attending school and have the household head employed in Israel at the time of the first interview. *Child school dropout* is a dummy taking value 1 if the child attends school in quarter t (the time of the first interview) but not in quarter $t + 1$ (the time of the second interview). Dropout students are 1.3% of our sample (4% if we consider those in secondary school). This is not surprising since - as discussed in Section 2 - the education system in the OPT is characterized by an extremely high education attendance rate (as high as 95% for primary education). As for the children characteristics, the sample is balanced with respect to the gender of the child. The average age of child is 12 and the mean years of schooling 6.3. *Household head job loss* is a dummy taking value 1 if the household head is employed in Israel in quarter t (the time of the first interview) but not in quarter $t + 1$ (the time of the second interview). The data show that job loss is a frequent event during the Second Intifada for Palestinian employed in Israel: 34% of workers loses the job in Israel between two quarters. As for household head education, 62% have completed at least primary education, 20% secondary education, and 7% tertiary education. More than 85% of Palestinian workers in Israel are employed in the private sector, 11% are self-employed and 2% employed in the public sector. As for the household characteristics, the average household size is of 8 members, the average number of children in the household is 3, and the number of people employed in the household other than the household head is 2.

Fatalities - our proxy for conflict intensity - is the per-capita number of Palestinian fatalities per 10,000 inhabitants by district and quarter: the mean is 0.36 and the variance 0.58. Conflict intensity is characterized by large variation across district and time as shown in Figure A.1 where OPT districts are classified according to the quantile they belong to in the distribution of the quarterly level number of Palestinian fatalities.

————— [Table 1 here] —————

4 Empirical strategy

4.1 Identification strategy

Our objective is to estimate the causal effect of parental job loss on child school dropout. To this end, we look at a representative sample of Palestinian children who are attending school in the OPT and have the household head employed in Israel during the Second Intifada. Our main regression model is the following:

$$Dropout_{ihjt} = \beta_0 + \beta_1 JobLoss_{hjt} + X'_{ihjt}\delta + W'_{hjt}\gamma + \theta_j + \lambda_t + \epsilon_{ihjt} \quad (1)$$

where $Dropout_{ihjt}$ is a dummy variable which takes value 1 if child i in household h from district j attends school in quarter t (the time of the first interview) and does not attend school in quarter $t + 1$ (the time of the second interview). $JobLoss_{hjt}$ is a dummy variable which takes value 1 if head of household h is employed in Israel in quarter t (the time of the first interview) and is not employed in Israel in quarter $t + 1$ (the time of the second interview). The set of controls includes: (i) child characteristics (gender, age, and years of schooling) grouped in vector X'_{ihjt} ; (ii) household head characteristics (age, age squared, a set of dummy variables for the education level, and a set of dummies for the employment status, i.e. regular employee, irregular employee, self-employed); and household characteristics (size, number of children, and number of people employed other than the household head) grouped in vector W'_{hjt} ; (iii) district fixed-effects grouped in vector θ_j ; (iv) quarter fixed-effects grouped in vector λ_t . Finally, ϵ_{ihjt} is the error term.

To identify the casual effect of job loss on child dropout, we adopt an instrumental variable strategy. As a plausibly exogenous source of variation in the job loss probability, we use the worker's exposure to conflict events occurred in his/her place of residence in the OPT. In particular, we exploit the variation across districts and over time in the quarterly per-capita number of Palestinian killed by the IDF in the worker's district of residence. The first-stage regression is:

$$JobLoss_{hjt} = \alpha_0 + \alpha_1 Fatalities_{jt} + X'_{ihjt}\zeta + W'_{hjt}\eta + \theta_j + \lambda_t + \mu_{ihjt} \quad (2)$$

where $Fatalities_{jt}$ is defined as the per-capita number of Palestinians killed by the Israeli Defense Forces (IDF) per 10,000 inhabitants in the household head’s district of residence in quarter t (the time of the first interview). All other variables are defined as in equation 1. The coefficient α_1 thus measures the conflict-induced increase in the likelihood that a worker who is employed in Israel in quarter t is no longer employed in Israel in quarter $t+1$. As in model 1, the vectors X'_{ihjt} and W'_{hjt} include child characteristics, and household head and household characteristics, respectively. θ_j includes the set of district fixed-effects, and λ_t the set of quarter fixed-effects. Lastly, μ_{ihjt} is the error term. In our robustness checks, we also include district-specific time trends to account for any district-level time varying characteristic. In all regressions, standard errors are clustered at district and type of residential location (i.e. rural, urban, refugee camp) level. This ensures a sufficiently large number of clusters (16 districts \times 3 types of residential location) so that the cluster-robust estimates of the variance covariance matrix of residuals are reliable.¹²

4.1.1 Discussion of the instrument

Relevance of the instrument Our estimation strategy is based on the hypothesis that higher exposure to conflict (as proxied by the per-capita number of Palestinian killed by the IDF in the worker’s district of residence) increases the job loss probability of a Palestinian worker living in the OPT (i.e. West Bank or Gaza Strip) and employed in Israel.¹³

There are at least two reasons for this effect. First, higher conflict intensity makes it more difficult for Palestinian employed in Israel to reach their workplace.¹⁴ For instance, the higher conflict intensity the more likely is that the IDF puts in place security measures to control the roads to the Israeli borders. These security measures make the travel time

¹²As a robustness, we also report the results when standard errors are clustered at the district level (the level at which conflict intensity is measured) and when we compute the p-values using wild bootstrapping (see Section 5).

¹³The West Bank and the Gaza Strip were both administered by the Palestinian Authority and had very similar economic trends during the Second Intifada. The two regions started to diverge in political and economic terms only after 2007, when Israel imposed a complete blockade on Gaza (Etkes and Zimring, 2015). The percentage of Palestinian workers from the West Bank and Gaza employed in Israel during the Second Intifada period is also not very different (20% and 14%, respectively.)

¹⁴Palestinian workers employed in Israel commute daily because they are not authorized to spend the night in Israel (see Section 2.1).

to the job place in Israel highly uncertain, increasing the likelihood of episodes of days late or absenteeism (Abrahams, 2015; Cali and Miaari, 2018; World Bank, 2004). Second, a higher conflict intensity in the district of residence of the Palestinian worker may be interpreted by the Israeli employer as a proxy for the probability of the worker’s involvement in violent actions against the IDF. Under this scenario, higher conflict intensity may increase the likelihood that Israeli employers fire Palestinian workers in retaliation for violence against Israeli military in the OPT (Miaari et al., 2012).

These arguments suggest that conflict is expected to have a differential effect on Palestinian workers depending on where they are employed. To provide empirical support to this conclusion, we consider the full sample of Palestinian workers living in the OPT and we estimate the effect of conflict intensity on the job loss probability of those employed in Israel versus in the OPT. Results reported in Panel A of Table 2 show that the number of fatalities positively affects the probability of losing the job for Palestinian workers employed in Israel but it has no effect for workers employed in the OPT - with the difference between the two coefficients being statistically significant at 1%.¹⁵

To further corroborate our conclusion, we estimate the effect of conflict intensity on job loss for the full sample of Palestinian workers by skill, occupation, and type of employment. We use these tests to exclude that conflict increases the worker’s probability of job loss for reasons other than being employed in Israel. Results reported in Table A.1 in the Appendix show that there are no differential effects of fatalities on job loss by any of these dimensions. This result confirms the relevance of our instrument in explaining job loss for Palestinian employed in Israel.

————— [Table 2 here] —————

¹⁵Previous research has shown that conflict intensity (as proxied by number of closure days or number of Palestinian fatalities) has a significant and positive effect on unemployment when considering only Palestinian residing in the West Bank (Mansour, 2010; Di Maio and Nandi, 2013; Miaari et al., 2014; Cali and Miaari, 2018). However, these studies do not distinguish workers by place of employment (Israel versus the West Bank) and thus it is not possible to disentangle whether this effect is driven by those employed in the West Bank, in Israel, or both. Interestingly, our results confirm previous studies (i.e. conflict increases unemployment in the West Bank) when we restrict the analysis to the same sample, i.e. only the West Bank. Yet, when distinguishing workers employed in the West Bank vs Israel, we find that the effect of conflict intensity on unemployment is statistically significant only for the latter group (results available upon request). This suggests that the negative effect of conflict on employment in the West Bank is driven by the job loss of those employed in Israel.

Validity of the instrument The validity of $Fatalities_{jt}$ as an instrumental variable for $Jobloss_{ihjt}$ in equation (2) relies on the assumption that conflict intensity has no direct effects on child school dropout other than through the household head job loss (i.e. exclusion restriction). For instance, this assumption would be violated if higher conflict intensity increases school dropout by destroying school infrastructure or by making more difficult to reach the school thus inducing the household to keep the child at home.

To provide empirical support to the exclusion restriction, we run a sort of placebo test using individual-level data from the full sample of children in the PLFS, independently from where the household head is employed at the time of the first interview. To begin, we note that children whose household head is employed in Israel (i.e. our main sample) are not different - in terms of gender distribution (0.5), age (12.7), and household size (6.7) - to the group of children whose household head is employed in the OPT (i.e. the placebo group). Next, we separately estimate the effect of conflict intensity on school dropout for these two groups of children controlling for individual, household, and locality characteristics along with quarter and district fixed effects. This allows us to compare the effect of the district-level number of fatalities on observationally identical children who are attending school in the same district (and thus who are exposed to the same conflict intensity) and only differ for the place of employment of the household head (Israel vs. OPT).¹⁶ This test is based on the idea that if conflict has a direct impact on child school dropout, it should affect children living in the same district independently from where the child's household head is employed.¹⁷ Our results indicate that this is not the case. As shown by Table 2 panel B, a higher number of fatalities increases the probability of school dropout for children with household head employed in Israel (column 1), while it does not affect children with household head employed in the OPT (column 2), with the

¹⁶Students allocation in schools in the OPT is decided by the MoEHE according to the place of residence of the household. The supply of schools is not an issue in the West Bank. In fact, access to education is considered to be highly equitable with respect to location (World Bank, 2007). Moreover, it is very unlikely that a student attends a school which is located far from home because mobility is extremely limited (due to military-security measures imposed by the IDF). This implies that children living in the same locality are very much likely to go to the same school.

¹⁷Intuitively, unobservables correlated with both the number of fatalities and the child experiencing parental job loss should affect observationally identical children regardless of whether their household head is employed in Israel or in the OPT.

difference between the coefficients being statistically significant at 5%.

The null effect of conflict on school dropout for children with the household head working in the OPT confirms the findings from previous studies documenting that enrolment and attendance of Palestinian students in both primary and secondary grades have not declined during the Second Intifada (see Section 2.2). In particular, Di Maio and Nandi (2013) show that primary school attendance in the West Bank has not been affected by conflict intensity as proxied by the number of border closure days. At the same time, Brueck et al. (2019) document that conflict intensity - measured as the number of Palestinian fatalities at the locality level - had no effect on enrollment, attendance, or dropout at the locality and school level for high school students in the West Bank during the Second Intifada.¹⁸

Reading together the results from the two panels of Table 2 indicates that the effect of conflict on child school dropout mirrors its effect on parental job loss. Conflict intensity increases school dropout only for children with the household head employed in Israel and it increases job loss only for Palestinian workers employed in Israel. We interpret these results as providing suggesting evidence ruling out direct effects of conflict intensity on child school dropout, therefore boosting the confidence in our identification strategy.

The validity of the test reported in Table 2 relies on the two types of households (and the two types of children) having the same exposure to conflict intensity. One possible concern is that the spatial distribution of the two types of households might be different within districts. This might lead to the two types of households being differentially exposed to conflict intensity. Unfortunately, we cannot directly test for this because households in the PLFS are not geo-localised. In any case, we argue that this should not be a serious concern for our results because the 16 OPT districts are quite small territorial units, which makes somehow unlikely the possibility of being exposed to very different levels of conflict intensity within the same district.¹⁹ Moreover, in our regression we

¹⁸Despite facing several difficulties, the education system in the OPT continued to fully operate during the Second Intifada: classes and final exams have taken place regularly, new school constructions have been completed, and even a new school curriculum has been implemented. The high resilience of the education system to conflict and the persistently high enrollment rates even during the conflict period can be explained by the high value given to education by the Palestinian society (Nicolai, 2007).

¹⁹The OPT are a very small territory. The West Bank, which comprises 11 districts, is 110 kilometers

control for type of residential location (rural, urban, or refugee camp) of the household. This should reduce even more the possibility that we are comparing households which are located in different areas within the same district. Finally, we anticipate that in Section 5.2 we perform a set of robustness checks to exclude the possibility that our results are driven by some geographical features of our data.

Although we interpret these results as convincing evidence supporting the exclusion restriction, we cannot rule out that there may be other ways through which conflict can affect school dropout. However, based on results in Table 2, we can exclude all channels related to conflict-induced changes in the supply of education, e.g., a worsening in the quality of school infrastructures. Moreover, our results suggest that we could also exclude all mechanisms related to changes in the demand for education which have to do with conflict-induced variation in the characteristics of the household's place of residence, e.g., an increase in the severity of mobility restriction imposed by the IDF (possibly increasing the difficulty of reaching schools).²⁰

One potential concern with our identification strategy is that children may drop out of school to fight against the IDF rather than as a consequence of parental job loss. To put it differently, if conflict is fueled by dropout students our identification strategy would be invalidated.²¹ This feedback mechanism seems unlikely in the context of the OPT. For instance, Di Maio and Nandi (2013) show that the district-level rate of school attendance is not correlated with the intensity of military measures (as captured by the number of closure days) implemented by the IDF. Brueck et al. (2019) document the same finding considering high-school students in the West Bank. To provide additional support to this claim, we regress the district-level number of fatalities on the district-level percentage of child school dropout separately for those with the household head employed in Israel and for those with the household head employed in the OPT, using a panel regression with

long and from 20 to 50 kilometers wide. The Gaza Strip, which comprises 5 districts, is 40 kilometers long and from 6 to 12 kilometers wide.

²⁰The inclusion of district-specific time trend actually controls for the possible role of district-level time-varying characteristics, such as conflict-induced changes in local economic conditions.

²¹As suggested by Rodriguez and Sanchez (2012) being a dropout may increase rebellion in adolescents. In turn, this may contribute to increase the number of demonstrations, the level of violence, and, possibly, the number of the consequent conflict-related fatalities.

district and time fixed effects. We find that these relations are never significant (see Table A.2). This indicates that it is unlikely that a higher conflict intensity is the consequence of children’s decision to abandon school to fight. Importantly, this conclusion applies also to children with the household head employed in Israel. These are children living in families possibly experiencing more directly the military and security consequences of the conflict (e.g. long waiting time at the border crossings and discrimination) and thus potentially developing a stronger opposition attitude towards Israel. While these results are to be taken cautiously because of data limitations, we argue that the available evidence suggests that it is unlikely that the potential threat of a feedback mechanism linking dropout to conflict intensity would invalidate our identification strategy.

Another possible threat to our identification is that workers may decide to relocate in districts less exposed to the conflict. This geographical sorting might lead to biased estimates of the effect of parental job loss. If, for instance, high-skilled workers move towards districts with lower number of fatalities, then our estimates would be biased upward. Ideally, one would like to check if the household changes district of residence between t and $t + 1$. The PLFS does not allow for a direct test of this possibility because it does not track households changing residential location. Yet, it should be noted that the possibility of relocation was extremely limited during the Second Intifada (see for instance, World Bank, 2007).²² In fact, after the outbreak of the Second Intifada, internal migration has been “very negligible” (PCBS, 2009) and the percentage of Palestinians who migrated declined for all age groups. Interestingly, the main motivation for internal migration during the period under analysis is marriage (PCBS, 2009). These results suggest that household relocation should not undermine our identification strategy. To provide additional evidence to this argument, we use our data to test whether the number of fatalities is associated with compositional effects at the district level. To this end, we regress the household head’s level of education, averaged by district, on the district-level number of fatalities, using a panel regression with district and time fixed effects.

²²World Bank (2007, p.1) reports that “administrative restrictions, rooted in military orders associated with the occupation of the West Bank [...] are used to restrict Palestinian access to large segments of the territories [...]. Permit policies limit the freedom of Palestinians to move home, obtain work, invest, [...] move about outside of their municipal jurisdiction.”

Results (reported in Appendix Table A.3) indicate that there are no compositional effects associated with conflict intensity, thus strengthening our confidence on the validity of the number of Palestinian fatalities as an instrumental variable for job loss.

5 Results

5.1 OLS results

Table 3 reports the OLS results. The baseline specification in column 1 shows that household head job loss is significantly positively correlated with child school dropout. From column 2 to 4, we progressively add to the baseline specification the set of controls for child, household head, and household characteristics, respectively. The magnitude of the coefficient remains stable across specifications and significant at least at the 5% level.

————— [Table 3 here] —————

5.2 2SLS results

First-stage results Table 4 column 1 reports the results for the first-stage regression (equation 2). The estimated effect of $Fatalities_{jt}$ on $JobLoss_{iht}$ is positive and highly significant.²³ This implies that a Palestinian worker employed in Israel who is exposed to a higher conflict intensity - as measured by the per-capita number of fatalities occurred in the district of residence - faces a higher job loss probability. As for the magnitude, one additional fatality in 10,000 inhabitants leads to an increase in the worker's probability of job loss by 5.3 percentage points.²⁴

As a robustness check, we estimate the first-stage regression (equation 2) including as an additional control the (per capita district-level) number of fatalities occurred in the

²³Note that the coefficient for the number of fatalities is larger than that reported in Table 2 for Palestinian workers employed in Israel (column 1). The reason is the more restrictive definition of the job loss status used there: to allow for the comparison between the two groups in Table 2 (those employed in Israel and those employed in the OPT at the time of the first interview), the job loss status does not apply to workers who lose the job in Israel but get re-employed in the OPT in the next quarter.

²⁴Given that the mean of our dependent variable is 0.341, one additional fatality in 10,000 inhabitants leads to an average increase in the probability of job separation by 15.5 percent $[(0.053/0.341)=0.155]$.

quarter after the first interview and those occurred in the quarter before that. Results shown in Table 4 column 2 indicate that the effect of fatalities occurred in the quarter after the interview is small and not significant while the effect of the fatalities at the time of the first interview (i.e. our proxy for conflict intensity) is only slightly reduced and remains significant at 5%. These results are reassuring as for our main finding since including the fatalities in the next quarter after the first interview (when the status change for both the household head and the child - if any - has already occurred) makes this specification a sort of placebo test. Column 3 shows that the effect of current quarter fatalities is also robust to controlling for previous quarter fatalities. Interestingly, this latter result together with the fact that the errors are clustered at the district level suggest that the possibility of serial correlation is not a concern for our analysis.

Next, we implement a falsification exercise to test whether ‘randomly generated’ values for the number of fatalities produce point-estimates close to the ‘true’ one. If this was the case, the null hypothesis that the coefficient of $Fatalities_{jt}$ is equal to zero would be erroneously rejected. As shown in Figure A.2, the point-estimates generated in the falsification test are normally distributed with mean zero.²⁵ This indicates that there is no correlation between the number of fatalities and household head job loss when the former are randomly assigned.

Finally, as an additional robustness check, we estimate the first-stage regression computing the standard errors via bootstrapping (Cameron et al., 2008). The level of significance of the estimates is unaffected (results available upon request).

————— [Table 4 here] —————

Second-stage results Table 5 presents our main results. The coefficients reported in the first row are the second-stage estimates of the effect of household head job loss on

²⁵Figure A.2 depicts the probability density function of the coefficients of $Fatalities_{jt}$ obtained by estimating the first-stage regression with the ‘random’ fatalities as independent variable and iterating 10,000 times. The vertical line indicates our ‘true’ point-estimate (0.053), which is reported in column (1) of Table 4. ‘Random’ fatalities are generated in the following way. For each iteration, we take the ‘true’ number of quarterly fatalities occurred during the Second Intifada and we randomly re-assign them to the district-quarter pairs. This implies that in each artificially (randomly) generated Second Intifada the total number of fatalities is equal to the real one but its district-quarter distribution is instead random.

child’s school dropout probability. Column 1 reports the specification which includes only the fixed effects. In columns 2-4, we progressively add a number of additional control variables for child, household head, and household characteristics. These estimates document a positive and significant effect of household head job loss on child school dropout, which remains fairly stable across different model specifications. In particular, looking at the more demanding specification (column 4), we find that parental job loss increases a child’s probability of dropping out of school by 9 percentage points.²⁶

It is worth noting that the 2SLS estimates are larger than the OLS estimates reported in Table 3. In our analysis, the compliers are the children whose household head gets separated from the job in Israel because of the conflict. The always-takers are those children who would have been exposed to parental job loss even in the absence of conflict (some household head may have decided to voluntarily leave the job, some others may have incurred in job separation for reasons other than the conflict). The estimated coefficient of β_1 is higher for the compliers: this is the group for which job loss can be particularly traumatic as it comes as unanticipated and involuntary, and it is more likely to be permanent. It follows that the coefficient we identify with our instrument can be interpreted as a Local Average Treatment Effect (LATE).

————— [Table 5 here] —————

Our results indicate that, among the children having the household head employed in Israel, the probability to dropout from school increases if the household head loses the job. Given that education is highly valued among Palestinians and the outside options are low, it is not obvious that this would be the case. To understand this result, it is worth noting that - *ceteris paribus* - children with the household head employed in

²⁶Note that, while the effect of household head job loss on child school dropout is highly significant and the magnitude large, the absolute number of affected children is small due to the low dropout rate (1.3% for the whole sample, 4% for students in secondary school, see Table 1). This may suggest a limited economic relevance for our results. Yet, two elements suggest that this is not the case. First, our analysis employs the most restrictive definition of dropout, i.e. that occurring between two consecutive quarters. In fact, this is a choice imposed by the nature of our main dataset. While the semi-panel structure of our data allows us to precisely identify immediate effects, it limits the possibility to look for - probably larger - retarded and longer-run ones. Second, as discussed in Section 2, education attendance in the OPT is very high. In this sense, the OPT is a tough test for studying the effect of household job loss on child school dropout and our results should thus be interpreted as indicating the potentially important role of the former even when the latter is a rare event and preference for education is very strong.

Israel are relatively privileged in terms of access to education. For instance, children of low-skilled workers employed in Israel have a 1% higher probability of being enrolled in school at time t (i.e. before job loss) than those with the household head employed in the OPT.²⁷ This relates to the fact that - controlling for observable characteristics such as age, education level, sector of activity, and type of employment - Palestinian workers in Israel earn a significantly higher wage than those in the OPT. Our results are thus compatible with a situation in which the job loss-induced contraction in the household budget increases the school dropout probability for those children who were able to enroll in the first place only because the household head was employed in Israel. In Section 5.4, we explore in detail this household income mechanism as a possible explanation of our results.

Robustness checks As a first robustness check, we re-estimate our model using different samples. First, we consider only children in mandatory grades, i.e. those who have not completed grade 10, irrespective of the age. In this case, the sample is reduced to 8,579 observations and the 2SLS estimate becomes 0.082 while remaining significant at 5%. Second, we add to our sample children aged 18 and 19, to be sure to include also students that are in the last year of secondary school but may have repeated one or more grades. With this sample (10,762 observations), the 2SLS estimate is larger in magnitude (0.153) than in our baseline specification and becomes significant at 1%.

Second, we replicate our analysis using an alternative definition of job loss. As in Table 2, we define job loss as the transition from employment in Israel to unemployment in the OPT. Table A.4 indicates that parental unemployment increases the probability of child school dropout by 24 percentage points, with the effect being significant at 10% in the most demanding specification (column 4). The larger effect of job loss on school dropout with respect to our main result (the coefficient magnitude more than doubles) reflects the fact that the sample of compliers now does not include household heads who

²⁷These characteristics of the OPT context make our analysis of the effects of job loss somehow different from others. In our case, the main consequence of losing the job is in fact to lose a situation of relative privilege, namely the wage premium for being employed in Israel. At the same time, the very existence of this initial privileged condition makes it very unlikely that job loss for this sample of workers is voluntary, hence strengthening the confidence in our identification strategy.

are re-employed in the OPT. These are workers likely to suffer a smaller negative income shock with respect to those who become unemployed. It follows that - by excluding from the sample of analysis those who are re-employed in the OPT - the effect of job loss on school dropout increases (even if the precision of the estimation is somehow reduced).

Third, we perform a set of robustness checks to exclude the possibility that our results are driven by some geographical features of our data. These tests are reported in Table A.5. As a first test, we re-run our analysis exploiting the different geographical size as a way of testing for the possibility that households with the head employed in Israel are located in different areas than households with the head employed in the OPT. While we cannot test directly for this - because the LFS does not geo-localize the household and only reports the district of residence and type of residential location (rural, urban, refugee camp) - we run two different tests to explore this possibility. Results are reported in columns 1-2 of Table A.5. Column 1 reports the 2SLS results when we exclude from the analysis the two largest districts in the OPT (i.e. Hebron and Ramallah). By excluding from the analysis the largest districts, we are therefore only considering those districts where the intra-district variation in household spatial localization is more limited, i.e. districts in which it is more likely that households with household head employed in Israel and those with household head employed in the OPT are located in the same area. The results in column 1 indicate that the magnitude of the effect of job loss on school dropout is unchanged with respect to our main specification. Column 2, instead, reports the 2SLS results when we exclude from the sample all the districts that have no borders with Israel, namely Nablus and Jericho. By doing so, we are dropping from the analysis districts which are located further from Israel. It is likely that workers from these districts face relatively larger costs to reach the Israeli border and thus have relatively larger benefits from working in Israel (rather than in the OPT). This is a possible explanation why when excluding them from the sample the estimated effect of job loss becomes slightly smaller in magnitude (though significant at 10%). We also consider the possibility that our results are affected by the special status of Palestinians living in East Jerusalem given that their access to Israel is not restricted but their access

to alternative employment might be. Results reported in A.5 column 3 show that when we exclude these workers the effect of job loss on child dropout slightly increases with respect to our main specification (from 0.092 to 0.110) and remains significant at 10%. Finally, we test for the possibility that our results are driven by the fact that - given the small size of districts in the Gaza Strip - district-level differences in the number of fatalities within that region may not be meaningful to capture differences in conflict exposure. To this end, we re-run our main specification considering all Gaza Strip districts as only one district.²⁸ Table A.5 column 4 indicates that the results are unchanged with respect to our main specification.²⁹

Fourth, we account for other conflict-related events that may represent confounding factors in our analysis and can be used as alternative proxies for conflict intensity. As discussed in Section 2, the IDF has used several different security-motivated military measures during the Second Intifada. One of the most important of such measures is the closure of borders between Israel and the OPT. During closure days, Palestinian workers employed in Israel are not allowed to leave the OPT and thus cannot reach their workplace, potentially increasing the probability of job loss. Since the number of closure days varies only at the country level (IDF's decision to close the borders affects all the OPT districts at the same time) its effect is already controlled for in our main regression by the time fixed effects. Yet, it is possible that the effect of closures depends on how far is the place of residence of the worker from the Israeli border. For this reason, we augment our main regression by including the number of closure days interacted with the distance between the capital of the worker's district of residence and the closer entry point in Israel. The estimated first-stage regression coefficient for this variable is negative - suggesting that the effect of closure days on job loss is smaller for workers living further away from the borders - but it is never significant. As shown in Table 6 column 2 (column 1 reports our baseline result), the magnitude of $Fatalities_{jt}$ is unchanged.

²⁸In practice, we compute for each individual in the Gaza Strip the level of conflict exposure as the per-capita number of fatalities occurred in the whole Gaza Strip (rather than in her district of residence)

²⁹In any case, it should be noted that - as documented by several Reports (World Bank 2007), during the Second Intifada the Gaza Strip suffered the same difficulties in terms of military-imposed mobility restrictions as the West Bank. Thus, it was extremely difficult and dangerous to move within the Gaza Strip, even between very close localities. This made the various zones of the Gaza Strip to be isolated.

Fifth, we check that our results are robust to the inclusion of a number of additional covariates. In particular, we include: 1) the full set of household head occupation dummies;³⁰ 2) the full set of household head industry of employment dummies;³¹ and 3) the number of other children in the household attending school. In Table 6 column 3-5, we add each of these variables to the main specification. Finally, in column 6 we include district-specific time trends. Results show that the coefficient of $Fatalities_{jt}$ is remarkably robust and always significant at 5%.³²

————— [Table 6 here] —————

Next, we check that our results are robust to non-linearities in both the control variables and in the instrument. To this end, we include the quadratic terms of all continuous control variables and all the two-way interactions between the dummy control variables. Results reported in Table A.6 column 1 indicate that the effect of household head job loss slightly increases with respect to the baseline. We also consider the possibility of non-linearities in the instrument used in the first-stage regression. To test for this, we include the quadratic term of $Fatalities_{jt}$ as additional instrument in equation 2. Also in this case, the results are virtually unchanged (see column (2) Table A.6).

Finally, we redo all our analysis using as an alternative instrument the predicted probability of household head job loss obtained from a probit model of $JobLoss_{ihjt}$ on $Fatalities_{jt}$ and all controls.³³ All our results are unchanged when using this alternative instrumental variable (detailed results are reported in Table A.7).

³⁰These are: a) legislators, senior officials, and managers; b) professionals, technical, associate and clerks; c) service, shop and market workers; d) skilled agricultural and fishery workers; e) craft and related trade workers; f) plant and machine operators and assemblers; g) elementary occupations.

³¹These are: a) agriculture; b) manufacturing; c) construction; d) commerce, hotels, and restaurants; e) transport, storage, and communication; f) services.

³²As an additional robustness check to account for the small number of clusters, we also compute the p-values using wild bootstrapping. The effect remains significant at 10% (results available upon request).

³³This procedure is expected to increase the precision of the estimated coefficient of interest given that the variable to be instrumented is a dummy. See Windmeijer and Santos Silva (1997) for a detailed discussion on this procedure. It is worth noting that the robustness of this estimator does not depend on a correct specification of the equation for household head job loss, i.e., estimator is robust to misspecification of such equation as probit (Wooldridge, 2002, p. 623).

5.3 Heterogeneity

5.3.1 Child characteristics

The effect of parental job loss on child school dropout is heterogeneous as for the gender and the academic performance of the child. The reduced form estimates are reported in Table 7. Column 1 and 2 show the results when we look separately at boys and girl: the effect of household head job loss is significant for the former group but not for the latter, and the difference between the two is significantly different from zero at 5%. These results are in line with the fact that - as discussed in Section 2 - households in the OPT value very much girl education (MoEHE, 2006; Nicolai, 2007). These results are also consistent with the fact that child labour - a possible household strategy to cope with a negative economic shock - is an option only for boys in the context of the OPT (Di Maio and Nandi, 2013). We elaborate more on this point in Section 5.4 where we discuss the possible mechanisms behind our main result.

Next, we explore whether the effect of parental job loss on child school dropout depends on the previous academic performance of the child. Results in columns 3 and 4 show that the effect of job loss on school dropout is high and significant for children who repeated at least one grade while it is not significant for those who never repeated a grade, with the difference between the two coefficients being significantly different from zero at 1%. This indicates that household head job loss is more likely to induce the household to withdraw the child from school if his/her academic performance is low, i.e. the expected returns to schooling are lower.

Finally, we do not find evidence of a differential effect for children in compulsory grades versus those who have completed compulsory education (results available upon request).³⁴ This is confirmed by the result that the effect for older students (16-17) is larger in magnitude but the difference is not statistically significant at conventional levels. Interestingly, this indicates that parental job loss does increase school dropout also for younger children, i.e. those for whom dropout is likely to be more harmful because it

³⁴As described in Section 2, education in the OPT is mandatory until grade 10. Grades 11 and 12 are non-mandatory but required to access university.

leads to a early interruption of the process of human capital accumulation.

————— [Table 7 here] —————

5.3.2 Household characteristics

The effect of parental job loss on child school dropout also varies with the level of parental education, and the number of children in the household. Reduced form results are reported in Table 8. Column 1 and 2 show the results when we split the sample according to level of education of the household head. Our results indicate that the effect of parental job loss is significant for children whose household head has at most primary education while there is no effect for children whose household head has secondary or higher education, with the difference between the two coefficients being statistically different from zero at 1%. This suggests that the household’s response to negative economic shocks may depend on how parents value education. At the same time, this result is in line with numerous previous studies showing that parental schooling is positively associated with better education outcomes for the child (Orazem and King, 2008).

Our results also show that the effect of parental job loss also varies depending on the composition of the household. Using as threshold the average household number of children in the OPT (3 children), columns 3 and 4 show that the effect of parental job loss is significant only for large households while it is not significant for smaller ones, with the difference between the two being significantly different from zero at 5%.

————— [Table 8 here] —————

5.4 Mechanisms

Our results document a large and significant effect of household head job loss on child school dropout. Yet, there are different potential explanations for this effect. We focus on four main mechanisms: 1) reduction in household income; 2) household head becoming a fighter; 3) family distress, e.g. parental divorce; and 4) household residential relocation.

5.4.1 Household income

Household income is a key determinant of household's decision concerning investment in education. Previous research has shown that higher household income is associated with better child education outcomes, including enrollment, test score, and attainment (Behrman and Knowles, 1999; Dostie and Jayaraman, 2006). In the case of the Palestinian workers employed in Israel, providing a precise measure of household income is very challenging. The PLFS does not report the household income and thus the latter needs to be constructed from individual wages. Unfortunately, the latter variable has a large number of missing values due to the large number of households with members who are self-employed and do not declare a wage. Moreover, less than one-third of the individuals who are employee report the wage in two consecutive quarters, significantly reducing the sample and making the estimation very imprecise.

Table 9 presents the reduced form results of the effect of conflict intensity - as proxied by $Fatalities_{jt}$ - on household income for our sample of Palestinian workers employed in Israel. To overcome the data limitation, we measure household income using different alternative proxies. In column 1, we minimize the noise in the household income variable by proxying it with a dummy taking value 1 if the household suffers an income loss, i.e. household income declines between quarter t (the time of the first interview) and quarter $t + 1$ (the time of the second interview), and zero otherwise. Results show that conflict intensity increases households' probability of suffering an income loss, though the effect is not significant at conventional levels, likely for the reduced sample size. In column 2, we perform the same analysis using a household income loss variable constructed by imputing the missing values for wages.³⁵ The effect of conflict on the household income loss probability is positive and significant at 5%. The magnitude of the coefficient indicates that one additional fatality in 10,000 inhabitants is associated with a 5.3 percentage point

³⁵The methodology to impute missing wages for Palestinian workers employed in Israel in quarter t (at the time of the first interview) is the following. As for the wage level at quarter t , we assign the worker the average wage level of Palestinians employed in Israel in the same industry, in the same quarter, and with the same level of education. As for the wage level at quarter $t + 1$ (the time of the second interview), the imputation depends on the employment status. If the worker is employed in Israel, we impute the same wage level as in previous quarter. If the worker is re-employed in the OPT, we impute the average wage level of Palestinians employed in the OPT in the same industry, in the same quarter, and with the same level of education. Finally, if the worker is unemployed, we impute a zero wage.

increase in the household’s probability of suffering an income loss between two consecutive quarters (i.e. between the first and the second interview). Finally, in column 3, we proxy household income loss using the (log) value of the reduction in the (actual and imputed) wages of the household members. Results indicate that one additional fatality in 10,000 inhabitants reduces household income by 16%. Interestingly, the effect of fatalities on household income is instead very small and not significant when we consider workers employed in the OPT (results available upon request). The evidence of a conflict-induced drop in household income only for Palestinian workers employed in Israel suggests that - *ceteris paribus* - changes in household income could be a potential mechanism explaining why parental job loss increases child school dropout.

————— [Table 9 here] —————

To provide additional support to this argument, we split the sample according to the number of employed members in the household other than the household head at the time of the first interview. The reduced form estimates reported in Table 10 indicate that household head job loss increases child school dropout only if the number of employed members in the household other than the household head is equal or less than 2 (i.e. the average value for this variable in our sample), with the difference between the coefficients for the two samples being statistically different from zero at 10%. One possible interpretation of this result is that - as long as the number of employed household members is taken as a proxy for household income - the effect of job loss is significant only for households with lower income, i.e. those for whom a negative economic shock is expected to be more binding. While the composition of the two sub-samples might be endogenous, this result contributes to suggest a role of household income drop in explaining the effect of household head job loss on child school dropout. Moreover, we note that the income mechanisms is coherent with the results (discussed in Section 5.3.2) showing that parental job loss has a stronger effect on child school dropout in households with more children and where the education level of the household head is lower.

————— [Table 10 here] —————

Interestingly, the household income drop mechanism would also provide an explanation for why the effect of parental job loss is stronger for boys than for girls (see Section 5.3 and Table 7). One of the possible coping strategies for a household affected by a negative economic shock is to withdraw the child from school and make him/her generating additional income. Yet, in the context of the OPT, child labour is not an option for girls (Di Maio and Nandi, 2013). Our data indicate that more than 40% of boys who drop out of school start working, and the percentage increases to 47% for those aged 15 or above (i.e. after mandatory school is completed). Instead, less than 1% of girls who dropout of school start working. This evidence is in line with the survey results reported in Sharek Youth Foundation (2009). Among the motivations for school dropout for male students, the economic ones are the most important: 38% of males report dropping out of school to support their household and 24% because they could no longer afford school. Instead, only 18% of females mention economic-related reasons (including school cost) for school dropout while the most common motivation is marriage (46%).

Taken together, this evidence indicates that household income loss is a possible mechanism behind the effect of parental job loss on child school dropout and that its impact is heterogeneous as for the gender of the child. In particular, the fact that Palestinian households are unlikely to resort to female work to generate additional income implies that - in line with the results in Table 7 - any given negative economic shock is more likely to increase the probability of school dropout for boys than for girls.

5.4.2 Other possible mechanisms

Household head becoming a fighter Another possible explanation for our main result is that those who lose the job in Israel decide to join in the violence (i.e. become fighters) and for this reason withdraw their children from school. Unfortunately, the available data do not allow to test this mechanism directly. Indeed, BTselem data on fatalities in the OPT do not report the employment status or the place of work of the individual. While we cannot exclude that some of those losing the job in Israel could take part in the uprising, our data do not indicate that job losers in Israel are more

likely to do so. For instance, results in Table A.8 show a lack of correlation between the job loss rate of Palestinians employed in Israel and the number of fighting fatalities at the district level in the OPT during the Second Intifada. In particular, in column 1 we regress the district-level number of Palestinians killed while fighting on the district-level job loss rate of Palestinians employed in Israel using a panel regression with district and quarter fixed effects. Results indicate that the effect is not significantly different from zero.³⁶ This result is confirmed also when - to rule out potential longer run effects - we perform the same regression using the district-level number of Palestinians killed while fighting in the next quarter (column 2) and in two quarters time (column 3). As an additional test, in columns 4-6 we replicate the analysis using as dependent variable the number of Palestinians killed while fighting who are older than 34 years (recall that 35 is the age limit for the work permit in Israel). The effect is again never significant. Because these measures of conflict intensity are the ones that would more accurately reflect if Palestinians who lost the job in Israel tend to join in the violence, we interpret this evidence as suggesting that it is unlikely that this mechanism may explain the effect of parental job loss on child school dropout. At the same time, these results strengthen the confidence in our identification strategy as they provide evidence against the possibility of reverse causality between job loss and fatalities.

Family disruption Household head job loss can affect child school dropout by increasing family distress, possibly leading to family disruption, i.e. parental divorce. Charles and Stephens (2004) find an increase in the probability of divorce following layoffs, and numerous studies document the detrimental effect of divorce on children's academic achievement (Stevens and Shaller, 2011). Reduced form results reported in Table A.9 show that conflict intensity does not increase the probability of divorce for Palestinians em-

³⁶Miaari et al. (2014) show that among the 373 localities in the West Bank those facing a larger reduction in the share of population employed in Israel (between the quarter before and after the start of the Second Intifada) are the ones in which more fatalities took place in the first 18 months of the Second Intifada. There are three aspects of the Miaari et al. (2014)s analysis which may explain why their result do not hold in our setting. First, their analysis only considers the West Bank while our analysis includes the whole OPT, i.e. both the West Bank and the Gaza Strip. Second, the period considered in their analysis is much shorter than ours. They only consider the first 18 months of the Second Intifada while our analysis includes the period 2000-2006. Finally, they restrict the analysis only to 20-49 years old male employees while we consider all working-age individuals (18-64).

ployed in Israel. To corroborate this result, we also look at the data from the Child Labour Force Survey 2004 (PCBS, 2004) which provides very detailed information on household structure. Again, we find that child school dropout is not correlated with the rate of parental divorce (results available upon request). While we cannot control for other possible intra-household effects, such as increases in stress and violence that may be associated with parental job loss, the available evidence induce us to exclude family disruption as a potential mechanism explaining the effect of household head job loss on child school dropout.

Residential relocation As a consequence of household head job loss, the household may decide to relocate. Relocation can be a very exhausting experience and is often associated with increased psychological distress for all household members (McLanahan, 1983; Stevens and Shaller, 2011). In particular, this may create difficulties to the child’s learning process, increasing the probability of grade repetition, and ultimately that the child dropouts from school. However, while residential relocation may be an important mechanism in other contexts, this is not the case in the OPT during the Second Intifada. In fact, the whole period of the Second Intifada has been characterised by extremely low internal and external mobility (see also the evidence discussed in Section 4.1.1). Mobility across cities in the OPT was severely limited through different measures such as checkpoints and internal closures (Mansour, 2010; Cali and Miaari, 2018; Abrahams, 2015). Moreover, the conflict situation induced Israel to severely limit the international mobility of Palestinian households. Taken together, this evidence suggests that - at least in the case of the OPT - residential relocation is not a likely mechanism through which household head job loss affects child school dropout.

6 Conclusions

In this paper, we have studied the effect of a negative household-level economic shock, namely the job loss faced by the household head, on child school dropout. To identify the effect of job loss, we focused on Palestinian household heads employed in Israel during

the Second Intifada (2000-2006) and implemented an instrumental variable strategy using worker's exposure to conflict as a source of exogenous variation in job loss. The size of the effect is large: household head job loss increases child school dropout probability by 9 percentage points. The effect varies with the gender and the academic performance of the student, with the educational level of the household head, and with the number of children in the household. We have also explored different possible mechanisms whereby household head job loss can affect the school dropout decision. Our results indicate that the household income drop associated with the job loss is likely to be the main motivation behind the household decision to withdraw the child from school.

Our paper contributes to a better understanding of the effects of idiosyncratic negative economic shocks on education investment choices at the household level. In particular, we have documented that the household head job loss has an immediate negative impact by increasing a child's school dropout probability. This result has important policy implications because school dropout is a difficult to revert outcome, especially in developing countries. In fact, even if they may be able to go back to school later on, they may still suffer permanently from lower educational attainment. Our results suggest that, where markets are imperfect and risk-mitigation policies are not effective, even possibly temporary and short-term negative economic shocks may be a serious obstacle to the process of human capital accumulation and thus have dramatic long-run effects on economic development.

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Tables and Figures

Table 1: SUMMARY STATISTICS

Variable	Obs	Mean	Std. Dev.	Min	Max
Child school dropout	9539	0.013	0.115	0	1
Household head job loss	9539	0.341	0.474	0	1
Fatalities	9539	0.358	0.579	0	5
Child gender (male)	9539	0.515	0.500	0	1
Child age	9539	12.718	2.231	10	17
Child years of schooling	9539	6.392	2.211	0	12
Household head age	9539	42.029	6.247	23	75
Household head education: primary	9539	0.623	0.485	0	1
Household head education: secondary	9539	0.208	0.406	0	1
Household head education: tertiary	9539	0.071	0.257	0	1
Household head employment status: self-employed	9539	0.117	0.322	0	1
Household head employment status: employee (government)	9539	0.022	0.147	0	1
Household head employment status: regular employee (private sector)	9539	0.753	0.431	0	1
Household head employment status: irregular employee (private sector)	9539	0.108	0.310	0	1
Household size	9539	6.650	2.380	3	20
Number of children in the household	9539	3.310	1.282	1	9
Number of household members employed other than the household head	9539	1.583	0.947	1	8

Note - The sample includes all Palestinian children aged 10-17 who at quarter t (the time of the first interview) are enrolled in school and have the household head employed in Israel. *Child school dropout* is a dummy variable which takes value 1 if a child is attending school in quarter t (the time of the first interview) and is not attending school in quarter $t + 1$ (the time of the second interview). *Household head job loss* is a dummy variable which takes value 1 if the child's household head is employed in Israel in quarter t and is not employed in Israel in quarter $t + 1$. *Fatalities* measures the district-level number of Palestinians killed by the Israeli Defense Forces per 10,000 inhabitants by quarter. (Sources: Labour Force Survey - PCBS; B'TLESEM.)

Table 2: MAIN IDENTIFICATION RESULTS

	Sample	
	Household head employed in Israel (1)	Household head employed in the OPT (2)
<i>PANEL A</i>	Household head job loss	
Fatalities	0.021** (0.009)	-0.001 (0.004)
<i>PANEL B</i>	Child school dropout	
Fatalities	0.005** (0.002)	0.001 (0.001)
All controls	Yes	Yes
Quarter FE	Yes	Yes
District FE	Yes	Yes
Observations	9539	42691

Note - OLS regression results. The sample includes all Palestinian children aged 10-17 who at quarter t (the time of the first interview) are enrolled in school and have the household head employed in Israel (in column 1) versus in the OPT (in column 2). The dependent variable is *Child school dropout* in the upper panel and *Household head job loss* in the lower panel, respectively. *Child school dropout* is a dummy variable which takes value 1 if a child is attending school in quarter t (the time of the first interview) and is not attending school in quarter $t+1$ (the time of the second interview). *Household head job loss* is a dummy variable which takes value 1 if the child's household head is employed in quarter t and is not employed in quarter $t+1$. The main explanatory variable, *Fatalities*, measures the district-level number of Palestinians killed by the Israeli Defence Forces per 10,000 inhabitants by quarter, and serves as instrumental variable. *All controls* include: 1) child-specific controls: gender, age, and years of schooling; 2) household head-specific controls: age, age squared, a set of dummies for the level of education, and a set of dummies for the employment status; 3) household-specific controls: size, number of children, number of members employed other than the household head, and type of residential location (rural, urban, refugee camp). Standard errors in parentheses are clustered at the district and type of residential location level. (Sources: Labour Force Survey - PCBS; B'TSELEM.)

*, **, *** Significant at the 10%, 5%, 1% level, respectively.

Table 3: OLS RESULTS

	Child school dropout			
	(1)	(2)	(3)	(4)
Household head job loss	0.009*** (0.003)	0.008*** (0.003)	0.008** (0.003)	0.007** (0.003)
Child gender (male)		0.003 (0.002)	0.003 (0.002)	0.003 (0.002)
Child age		0.009*** (0.001)	0.009*** (0.002)	0.009*** (0.002)
Child years of schooling		-0.005*** (0.002)	-0.004*** (0.002)	-0.004** (0.002)
Household-specific controls	No	No	No	Yes
Household head-specific controls	No	No	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Observations	9539	9539	9539	9539
Mean of dependent variable		0.013		

Note: OLS regression results. The sample includes all Palestinian children aged 10-17 who at quarter t (the time of the first interview) are enrolled in school and have the household head employed in Israel. *Child school dropout* is a dummy variable which takes value 1 if a child is attending school in quarter t (the time of the first interview) and is not attending school in quarter $t + 1$ (the time of the second interview). *Household head job loss* is a dummy variable which takes value 1 if the child's household head is employed in Israel in quarter t and is not employed in Israel in quarter $t + 1$. *Household head-specific controls* include household head age, age squared, a set of dummies for the level of education, and a set of dummies for the employment status. *Household-specific controls* include household size, number of children in the household, number of members employed in the household other than the household head, and type of residential location dummies (rural, urban, refugee camp). Standard errors in parentheses are clustered at the district and type of residential location level. (Sources: Labour Force Survey - PCBS; B'TSELEM.)

*, **, *** Significant at the 10%, 5%, 1% level, respectively.

Table 4: FIRST STAGE: Timing of the effect of Fatalities

	Household head job loss		
	(1)	(2)	(3)
Fatalities: current quarter	0.053*** (0.015)	0.049** (0.018)	0.047** (0.019)
Fatalities: next quarter		0.010 (0.018)	
Fatalities: previous quarter			0.019 (0.021)
All controls	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes
Observations	9539	9539	9539
Mean of dependent variable		0.341	

Note - OLS regression results. The sample includes all Palestinian children aged 10-17 who at quarter t (the time of the first interview) are enrolled in school and have the household head employed in Israel. The dependent variable, *Household head job loss*, is a dummy variable which takes value 1 if the child's household head is employed in Israel in quarter t and is not employed in Israel in quarter $t + 1$. *Fatalities: current quarter* is the district-level number of Palestinians killed by the Israeli Defence Forces per 10,000 inhabitants in the quarter of the first interview. *Fatalities: next quarter* is the district-level number of Palestinians killed by the Israeli Defence Forces per 10,000 inhabitants in the quarter after that of the first interview. *Fatalities: previous quarter* is the district-level number of Palestinians killed by the Israeli Defence Forces per 10,000 inhabitants in the quarter before that of the first interview. *All controls* include: 1) child-specific controls: gender, age, and years of schooling; 2) household head-specific controls: age, age squared, a set of dummies for the level of education, and a set of dummies for the employment status; 3) household-specific controls: size, number of children, number of members employed other than the household head, and type of residential location (rural, urban, refugee camp). Standard errors in parentheses are clustered at the district and type of residential location level. (Sources: Labour Force Survey - PCBS; B'TSELEM.)

*, **, *** Significant at the 10%, 5%, 1% level, respectively.

Table 5: 2SLS RESULTS

	Child school dropout			
	(1)	(2)	(3)	(4)
Household head job loss	0.103** (0.052)	0.094** (0.048)	0.093* (0.048)	0.092** (0.046)
Child gender (male)		0.003 (0.002)	0.003 (0.002)	0.003 (0.002)
Child age		0.008*** (0.001)	0.009*** (0.002)	0.008*** (0.002)
Child years of schooling		-0.004** (0.001)	-0.004** (0.001)	-0.004** (0.002)
Household-specific controls	No	No	No	Yes
Household head-specific controls	No	No	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Observations	9539	9539	9539	9539
Cragg-Donald Wald F statistic	25.79	25.12	24.92	24.39
Kleibergen-Paap Wald rk F statistic	13.09	13.17	12.10	12.45
Anderson-Rubin Wald test p-val	0.018	0.032	0.026	0.027
Mean of dependent variable		0.013		

Note - 2SLS regression results. The sample includes all Palestinian children aged 10-17 who at quarter t (the time of the first interview) are enrolled in school and have the household head employed in Israel. The dependent variable is *Child school dropout*, a dummy variable which takes value 1 if a child is attending school in quarter t (the time of the first interview) and is not attending school in quarter $t + 1$ (the time of the second interview). The main explanatory variable, *Household head job loss*, is a dummy variable which takes value 1 if the child's household head is employed in Israel in quarter t and is not employed in Israel in quarter $t + 1$. The instrumental variable used in first-stage regression, *Fatalities*, measures the district-level number of Palestinians killed by the Israeli Defence Forces per 10,000 inhabitants by quarter. *Household head-specific controls* include: age, age squared, a set of dummies for the level of education, and a set of dummies for the employment status. *Household-specific controls* include: size, number of children, number of members employed other than the household head, and type of residential location (rural, urban, refugee camp). Standard errors in parentheses are clustered at the district and type of residential location level. (Sources: Labour Force Survey - PCBS; B'TSELEM.)

*, **, *** Significant at the 10%, 5%, 1% level, respectively.

Table 6: ROBUSTNESS 2SLS RESULTS: Including additional controls

	Child school dropout					
	(1)	(2)	(3)	(4)	(5)	(6)
Household head job loss	0.092** (0.046)	0.094** (0.045)	0.094** (0.044)	0.096** (0.046)	0.101** (0.050)	0.104** (0.051)
District-specific time trends	No	No	No	No	No	Yes
Household head occupation dummies	No	No	No	No	Yes	Yes
Household head job industry dummies	No	No	No	Yes	Yes	Yes
No. siblings in school other than i	No	No	Yes	Yes	Yes	Yes
No. closure days*district distance from Israel	No	Yes	Yes	Yes	Yes	Yes
All controls	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	No
Observations	9539	9539	9539	9539	9539	9539
Cragg-Donald Wald F statistic	24.92	24.97	24.64	23.54	21.71	20.98
Kleibergen-Paap Wald rk F statistic	12.45	11.77	11.77	10.46	9.96	10.37
Anderson-Rubin Wald test p-val	0.026	0.023	0.021	0.022	0.025	0.025
Mean of dependent variable	0.013					

Note - 2SLS regression results. The sample includes all Palestinian children aged 10-17 who at quarter t (the time of the first interview) are enrolled in school and have the household head employed in Israel. The dependent variable is *Child school dropout*, a dummy variable which takes value 1 if a child is attending school in quarter t (the time of the first interview) and is not attending school in quarter $t + 1$ (the time of the second interview). The main explanatory variable, *Household head job loss*, is a dummy variable which takes value 1 if the child's household head is employed in Israel in quarter t and is not employed in Israel in quarter $t + 1$. The instrumental variable used in first-stage regression, *Fatalities*, measures the district-level number of Palestinians killed by the Israeli Defence Forces per 10,000 inhabitants by quarter. *Household head occupation dummies* include: a) legislators, senior officials, and managers; b) professionals, technical, associate and clerks; c) service, shop and market workers; d) skilled agricultural and fishery workers; e) craft and related trade workers; f) plant and machine operators and assemblers; g) elementary occupations. *Household head job industry dummies* include: a) agriculture; b) manufacturing; c) construction; d) commerce, hotels, and restaurants; e) transport, storage, and communication; f) services. *All controls* include: 1) child-specific controls: gender, age, and years of schooling; 2) household head-specific controls: age, age squared, a set of dummies for the level of education, and a set of dummies for the employment status; 3) household-specific controls: size, number of children, number of members employed other than the household head, and type of residential location (rural, urban, refugee camp). Standard errors in parentheses are clustered at the district and type of residential location level. (Sources: Labour Force Survey - PCBS; B'TSELEM.)

*, **, *** Significant at the 10%, 5%, 1% level, respectively.

Table 7: HETEROGENEITY RESULTS: By child's characteristics

	Child school dropout			
	Gender		Grade repeated	
	Male (1)	Female (2)	Yes (3)	No (4)
Fatalities	0.010*** (0.003)	-0.001 (0.002)	0.016*** (0.006)	0.001 (0.002)
All controls	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Observations	4909	4630	2751	6788

Note - OLS regression (reduced-form) results. The sample includes all Palestinian children aged 10-17 who at quarter t (the time of the first interview) are enrolled in school and have the household head employed in Israel. The dependent variable is *Child school dropout*, a dummy variable which takes value 1 if a child is attending school in quarter t (the time of the first interview) and not attending school in quarter $t + 1$ (the time of the second interview). The main explanatory variable, *Fatalities*, measures the district-level number of Palestinians killed by the Israeli Defence Forces per 10,000 inhabitants by quarter. *All controls* include: 1) child-specific controls: gender, age, and years of schooling; 2) household head-specific controls: age, age squared, a set of dummies for the level of education, and a set of dummies for the employment status; 3) household-specific controls: size, number of children, number of members employed other than the household head, and type of residential location (rural, urban, refugee camp). Standard errors in parentheses are clustered at the district and type of residential location level. (Sources: Labour Force Survey - PCBS; B'TSELEM.)

*, **, *** Significant at the 10%, 5%, 1% level, respectively.

Table 8: HETEROGENEITY RESULTS: By household's characteristics

	Child school dropout			
	Household head education		No. children in the household	
	Primary	Secondary or higher	≤ 3	> 3
	(1)	(2)	(3)	(4)
Fatalities	0.008** (0.003)	-0.001 (0.002)	0.000 (0.002)	0.010** (0.003)
All controls	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Observations	6874	2665	5350	4189

Note - OLS regression (reduced-form) results. The sample includes all Palestinian children aged 10-17 who at quarter t (the time of the first interview) are enrolled in school and have the household head employed in Israel. The dependent variable is *Child school dropout*, a dummy variable which takes value 1 if a child is attending school in quarter t (the time of the first interview) and is not attending school in quarter $t + 1$ (the time of the second interview). The main explanatory variable, *Fatalities*, measures the district-level number of Palestinians killed by the Israeli Defence Forces per 10,000 inhabitants by quarter. *All controls* include: 1) child-specific controls: gender, age, and years of schooling; 2) household head-specific controls: age, age squared, a set of dummies for the level of education, and a set of dummies for the employment status; 3) household-specific controls: size, number of children, number of members employed other than the household head, and type of residential location (rural, urban, refugee camp). Standard errors in parentheses are clustered at the district and type of residential location level. (Sources: Labour Force Survey - PCBS; B'TSELEM.)

*, **, *** Significant at the 10%, 5%, 1% level, respectively.

Table 9: MECHANISMS RESULTS: Household income

	Household income loss indicator (1)	Household income loss indicator (imputed wages) (2)	Household (log) income loss (imputed wages) (3)
Fatalities	0.035 (0.041)	0.053*** (0.019)	0.158** (0.067)
All controls	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes
Observations	3198	8353	8353
Mean of dependent variable	0.578	0.520	1.793

Note - OLS regression (reduced-form) results. The sample includes all Palestinian children aged 10-17 who at quarter t (the time of the first interview) are enrolled in school and have the household head employed in Israel. *Household income loss indicator* is a dummy variable which takes value 1 if a child's household income declined from quarter t (time of the first interview) to quarter $t + 1$ (time of the second interview). *Household income loss indicator (imputed wages)* is defined in the same way but income is computed considering imputed wages for missing values. Wages are imputed in the following way. As for the wage level at the time of the first interview, we assign the worker the average wage level of Palestinian workers employed in Israel in the same industry, in the same quarter, and with the same level of education. As for the wage level at the time of the second interview, the imputation depends on the employment status. If he/she is employed in Israel, we impute the same wage level as in previous quarter. If he/she is re-employed in the OPT, we impute the average wage level of Palestinian workers employed in the OPT in the same industry, in the same quarter, and with the same level of education. Finally, if he/she is unemployed, we impute a zero wage. *Household income (log) loss (imputed wages)* measures the difference in the household income (computed considering imputed wages) between quarter t and quarter $t + 1$: it is then multiplied by -1 so as higher values capture higher income loss, and expressed in log terms. The main explanatory variable, *Fatalities*, measures the district-level number of Palestinians killed by the Israeli Defence Forces per 10,000 inhabitants by quarter, and serves as instrumental variable. *All controls* include: 1) child-specific controls: gender, age, and years of schooling; 2) household head-specific controls: age, age squared, a set of dummies for the level of education, and a set of dummies for the employment status; 3) household-specific controls: size, number of children, number of members employed other than the household head, and type of residential location (rural, urban, refugee camp). Standard errors in parentheses are clustered at the district and type of residential location level. (Sources: Labour Force Survey - PCBS; B'TSELEM.)

*, **, *** Significant at the 10%, 5%, 1% level, respectively.

Table 10: MECHANISMS RESULTS: Additional evidence

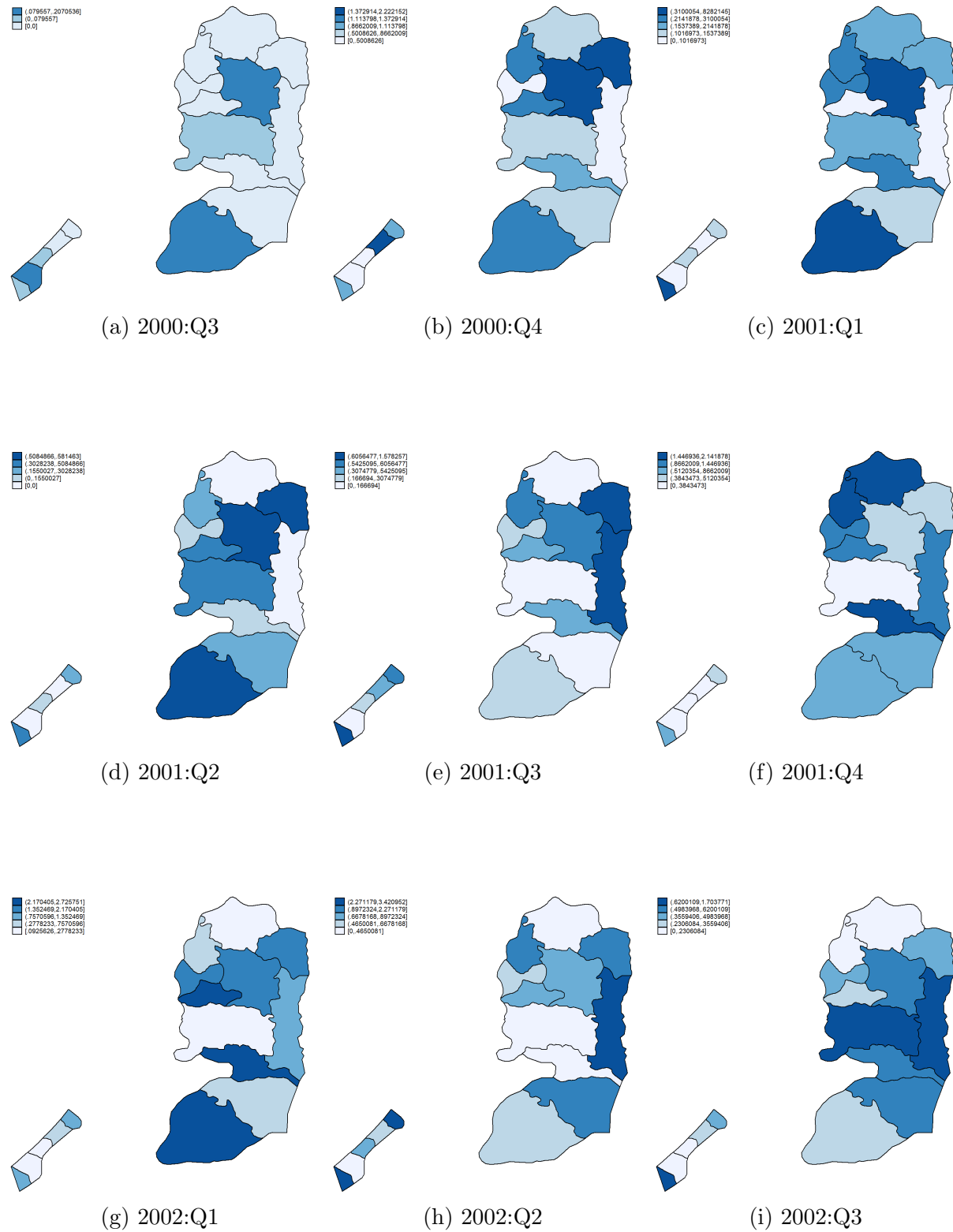
	Child school dropout	
	No. employed members other than household head ≤ 2 (1)	No. employed members other than household head > 2 (2)
Fatalities	0.006** (0.002)	-0.009 (0.009)
All controls	Yes	Yes
Quarter FE	Yes	Yes
District FE	Yes	Yes
Observations	8195	1344

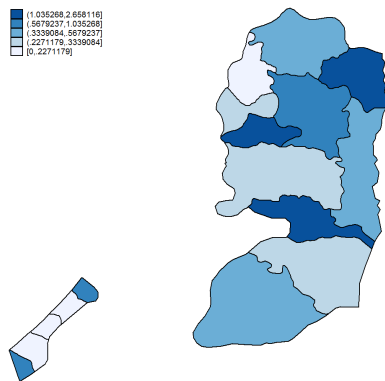
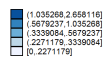
Note - OLS regression (reduced-form) results. The sample includes all Palestinian children aged 10-17 who at quarter t (the time of the first interview) are enrolled in school and have the household head employed in Israel. The dependent variable is *Child school dropout*, a dummy variable which takes value 1 if a child is attending school in quarter t (the time of the first interview) and is not attending school in quarter $t + 1$ (the time of the second interview). The main explanatory variable, *Fatalities*, measures the district-level number of Palestinians killed by the Israeli Defence Forces per 10,000 inhabitants by quarter, and serves as instrumental variable. *All controls* include: 1) child-specific controls: gender, age, and years of schooling; 2) household head-specific controls: age, age squared, a set of dummies for the level of education, and a set of dummies for the employment status; 3) household-specific controls: size, number of children, number of members employed other than the household head, and type of residential location (rural, urban, refugee camp). Standard errors in parentheses are clustered at the district and type of residential location level. (Sources: Labour Force Survey - PCBS; B'TSELEM.)

*, **, *** Significant at the 10%, 5%, 1% level, respectively.

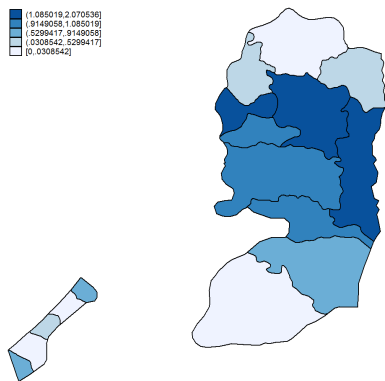
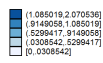
Appendix A: Figures and tables not shown in the text

Figure A.1: Conflict intensity in the Occupied Palestinian Territories (OPT) by district and quarter, 2000:Q3-2006:Q4

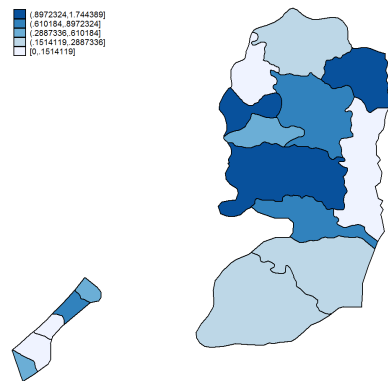
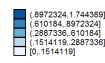




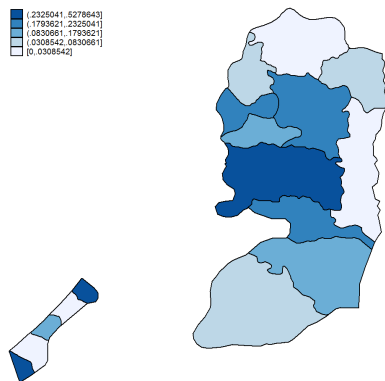
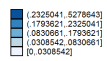
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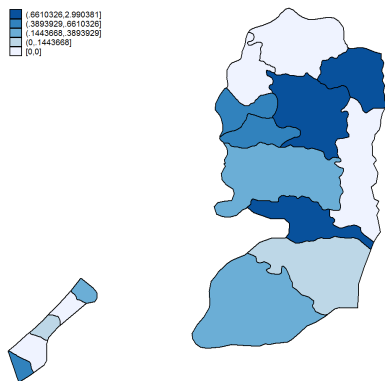
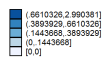
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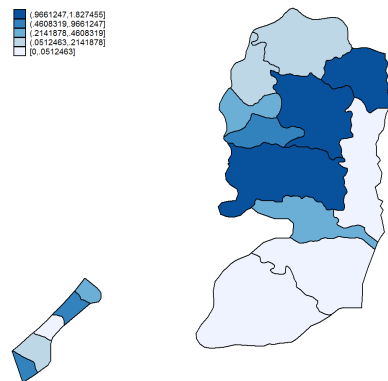
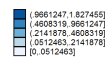
(l) 2003:Q2



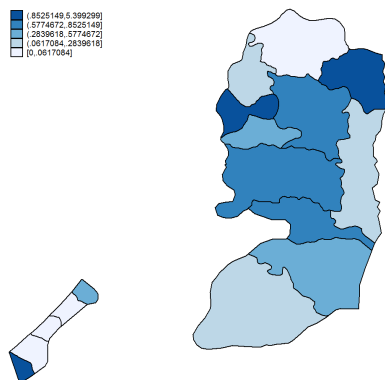
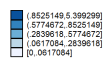
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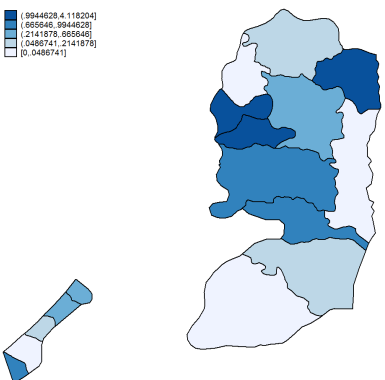
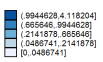
(n) 2003:Q4



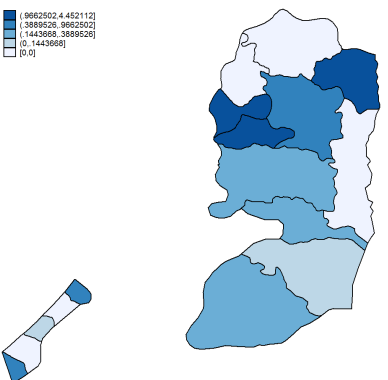
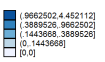
(o) 2004:Q1



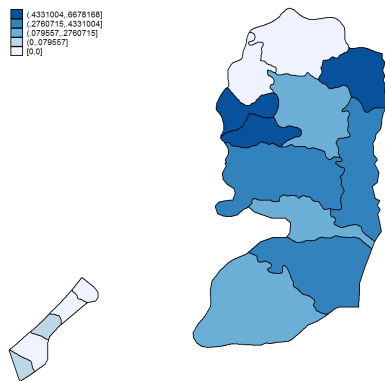
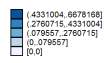
(p) 2004:Q2



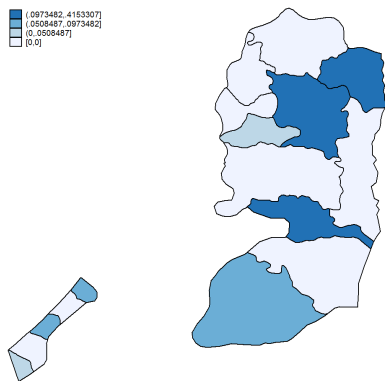
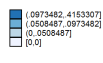
(q) 2004:Q3



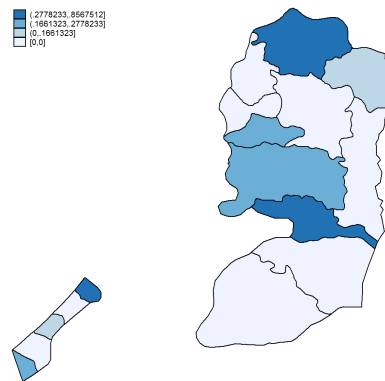
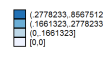
(r) 2004:Q4



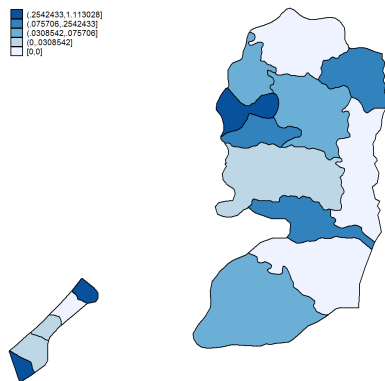
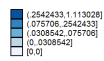
(s) 2005:Q1



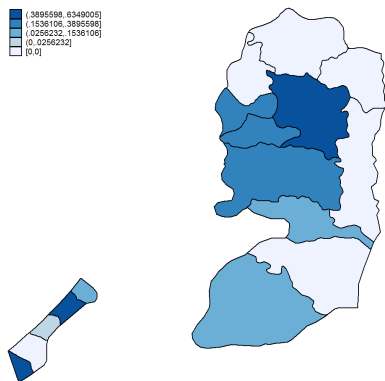
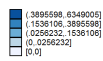
(t) 2005:Q2



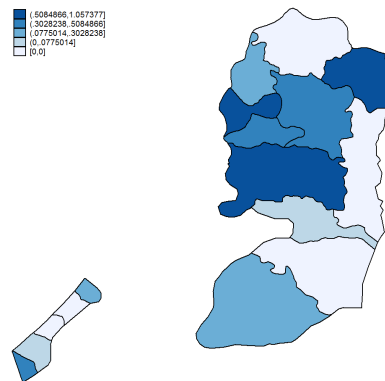
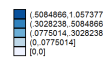
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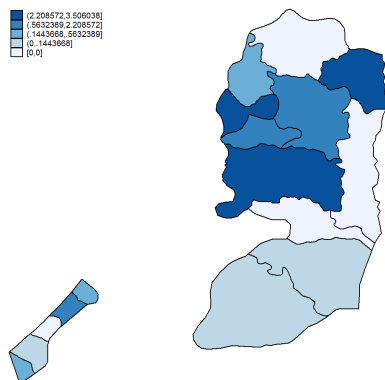
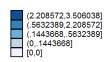
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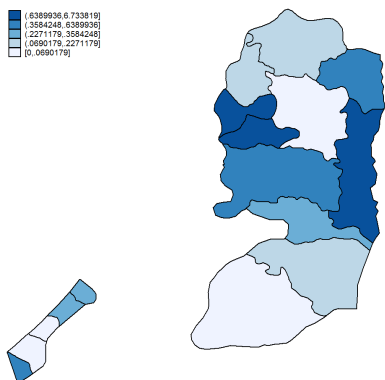
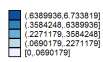
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(x) 2006:Q2

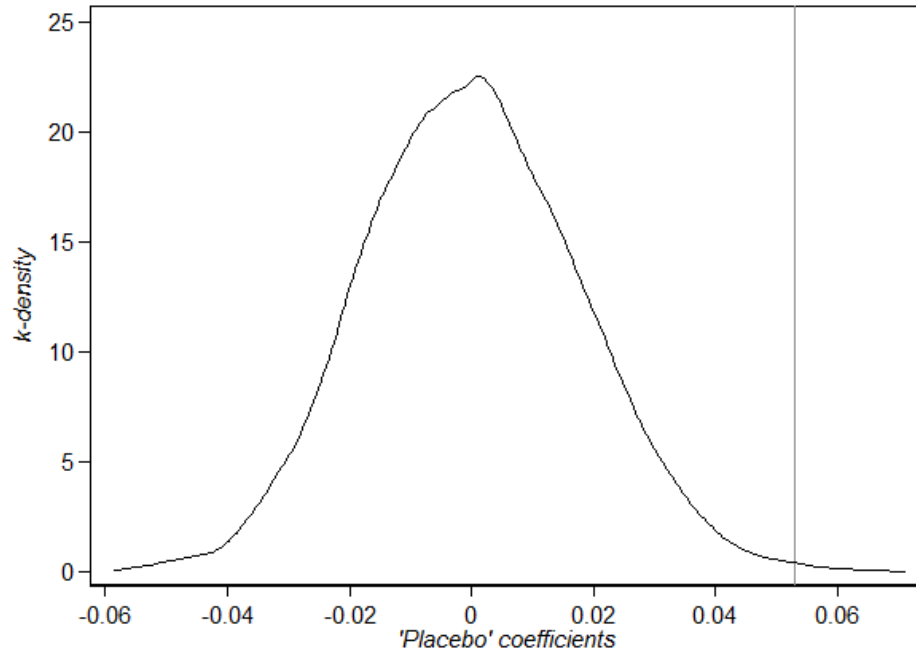


(y) 2006:Q3



(z) 2006:Q4

Figure A.2: FALSIFICATION TEST



Note - The figure depicts the probability density function of the coefficients of $Fatalities_{jt}$ obtained by estimating the first-stage regression with the placebo fatalities as independent variable, and iterating 10,000 times. The vertical line indicates our true point-estimate (0.053), which is reported in column (1) of Table 4.

Table A.1: FURTHER IDENTIFICATION RESULTS: Effects of fatalities on job loss by skill, occupation and type of employment

Household head job loss											
	by skill			by occupation					by type of employment		
	Low-skill (1)	High-skill (2)	Occ 1 (3)	Occ 2 (4)	Occ 3 (5)	Occ 4 (6)	Occ 5 (7)	Occ 6 (8)	Occ 7 (9)	Self-emp (10)	Public (11) Private (12)
Fatalities	0.008 (0.008)	-0.002 (0.004)	0.022 (0.018)	0.003 (0.005)	0.000 (0.009)	-0.018 (0.013)	0.004 (0.013)	-0.003 (0.011)	0.007 (0.012)	-0.006 (0.006)	0.001 (0.005) 0.010 (0.013)
All controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20888	32574	2568	9569	7501	6166	11786	5704	10168	22628	12605 17751

Note - OLS regression results. The sample includes all Palestinian children aged 10-17 who at quarter t (the time of the first interview) are enrolled in school and have the household head employed either in Israel or in the OPT. The dependent variable, *Household head job loss*, is a dummy variable which takes value 1 if the child's household head is employed (either in Israel or in the OPT) in quarter t and is not employed in quarter $t + 1$. The main explanatory variable, *Fatalities*, measures the district-level number of Palestinians killed by the Israeli Defence Forces per 10,000 inhabitants by quarter. Low-skilled is a dummy variable which takes value 1 if the child's household head has a number of years of schooling below 9 (i.e. the median number of years of schooling for Palestinian household heads). Occupation 1-7 is a set of dummy variables which take value 1 if the occupation of child's household head is: 1) Legislators, Senior Officials and Managers; 2) Professionals, Technical, Associate and Clerks; 3) Service, Shop and Market Workers; 4) Skilled Agricultural & Fishery Workers; 5) Craft and Related Trade Workers; 6) Plant and Machine Operators and Assemblers; 7) Elementary Occupations (reference category), respectively. Self-employed is a dummy variable which takes value 1 if the child's household head works as self-employed in quarter t . Public (Private) is a dummy variable which takes value 1 if the child's household head works as employee in the public (private) sector. All controls include: 1) child-specific controls: gender, age, and years of schooling; 2) household head-specific controls: age, age squared, a set of dummies for the level of education, and a set of dummies for the employment status; 3) household-specific controls: size, number of children, number of members employed other than the household head, and type of residential location (rural, urban, refugee camp). Standard errors in parentheses are clustered at the district and type of residential location level. (Sources: Labour Force Survey - PCBS; B'TSELEM.)

*, **, *** Significant at the 10%, 5%, 1% level, respectively.

Table A.2: FURTHER IDENTIFICATION RESULTS: Feedback mechanisms

	District-level number of fatalities	
	(1)	(2)
Dropout rate (students with HH head employed in Israel)	2.754 (1.696)	
Dropout rate (students with HH head employed in OPT)		1.123 (1.317)
Quarter FE	Yes	Yes
District FE	Yes	Yes
Observations	358	400

Note - Panel fixed-effects regression results. The dependent variable, *District-level number of fatalities*, is the district-level number of Palestinians killed by the Israeli Defence Forces per 10,000 inhabitants by quarter in the district of residence of the student. *Dropout rate (students with HH head employed in Israel /OPT)* is the district-level dropout rate computed among students with household head employed in Israel and the OPT, respectively. Standard errors are clustered at the district level. (Sources: Labour Force Survey - PCBS; B'TSELEM.)

*, **, *** Significant at the 10%, 5%, 1% level, respectively.

Table A.3: FURTHER IDENTIFICATION RESULTS: Compositional effects of Fatalities

	District-level average household head education	
	(1)	(2)
Fatalities	-0.034 (0.053)	-0.032 (0.053)
District-level unemployment rate		-0.002 (0.013)
Quarter FE	Yes	Yes
District FE	Yes	Yes
Observations	400	400

Note - Panel fixed-effects regression results. The dependent variable, *District-level average household head education*, is the district-level average household head's education level as measured by the years of schooling. *Fatalities* measures the district-level number of Palestinians killed by the Israeli Defence Forces per 10,000 inhabitants by quarter. *District-level unemployment rate* is the district-level unemployment rate for workers 15-65 years old. Standard errors are clustered at the district level. (Sources: Labour Force Survey - PCBS; B'TSELEM.)

*, **, *** Significant at the 10%, 5%, 1% level, respectively.

Table A.4: ROBUSTNESS 2SLS RESULTS: Using an alternative definition of job loss

	Child school dropout			
	(1)	(2)	(3)	(4)
Household head job loss	0.243** (0.124)	0.223* (0.118)	0.236* (0.130)	0.235* (0.124)
Male		0.007** (0.003)	0.007** (0.003)	0.007** (0.003)
Age		0.006** (0.002)	0.006*** (0.002)	0.006*** (0.002)
Years of schooling		-0.001 (0.002)	-0.001 (0.002)	-0.001 (0.002)
Household-specific controls	No	No	No	Yes
Household head-specific controls	No	No	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Observations	9539	9539	9539	9539
Cragg-Donald Wald F statistic	9.8	9.4	8.2	8.1
Kleibergen-Paap Wald rk F statistic	5.2	5.0	4.2	4.4
Anderson-Rubin Wald test p-val	0.014	0.022	0.027	0.021
Mean of dependent variable		0.013		

Note - 2SLS regression results. The sample includes all Palestinian children aged 10-17 who at quarter t (the time of the first interview) are enrolled in school and have the household head employed in Israel. The dependent variable is *Child school dropout*, a dummy variable which takes value 1 if a child is attending school in quarter t (the time of the first interview) and is not attending school in quarter $t + 1$ (the time of the second interview). The main explanatory variable, *Household head job loss*, is a dummy variable which takes value 1 if the child's household head is employed in Israel in quarter t and is not employed (neither in Israel nor in the OPT) in quarter $t + 1$. The instrumental variable used in first-stage regression, *Fatalities*, measures the district-level number of Palestinians killed by the Israeli Defence Forces per 10,000 inhabitants by quarter. *Household head-specific controls* include: age, age squared, a set of dummies for the level of education, and a set of dummies for the employment status. *Household-specific controls* include: size, number of children, number of members employed other than the household head; and type of residential location (rural, urban, refugee camp). Standard errors in parentheses are clustered at the district and type of residential location level. (Sources: Labour Force Survey - PCBS; BTSELEM.)

*, **, *** Significant at the 10%, 5%, 1% level, respectively.

Table A.5: ROBUSTNESS 2SLS RESULTS: Addressing the concern about spatial variation

	Child school dropout			
	Excluding the two largest districts in the OPT (1)	Excluding the districts with no border with Israel (2)	Excluding the East Jerusalem district (3)	Considering the whole Gaza Strip as one district (4)
Household head job loss	0.094** (0.046)	0.078* (0.042)	0.110* (0.062)	0.092** (0.038)
All controls	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Observations	7149	9089	8441	9539

Note - 2SLS regression results. The sample includes all Palestinian children aged 10-17 who at quarter t (the time of the first interview) are enrolled in school and have the household head employed in Israel. The dependent variable is *Child school dropout*, a dummy variable which takes value 1 if a child is attending school in quarter t (the time of the first interview) and is not attending school in quarter $t + 1$ (the time of the second interview). The main explanatory variable, *Household head job loss*, is a dummy variable which takes value 1 if the child's household head is employed in Israel in quarter t and is not employed in Israel in quarter $t + 1$. The instrumental variable used in first-stage regression, *Fatalities*, measures the district-level number of Palestinians killed by the Israeli Defence Forces per 10,000 inhabitants by quarter. *All controls* include: 1) child-specific controls: gender, age, and years of schooling; 2) household head-specific controls: age, age squared, a set of dummies for the level of education, and a set of dummies for the employment status; 3) household-specific controls: size, number of children, number of members employed other than the household head, and type of residential location (rural, urban, refugee camp). The excluded districts are Hebron and Ramallah in column 1, and Jerico and Tubas in column 2, respectively. Standard errors in parentheses are clustered at the district and type of residential location level. (Sources: Labour Force Survey - PCBS; BTSELEM.)

*, **, *** Significant at the 10%, 5%, 1% level, respectively.

Table A.6: ROBUSTNESS 2SLS RESULTS: Exploring non linearities

	Child school dropout	
	(1)	(2)
Household head job loss	0.099* (0.051)	0.081* (0.043)
Nonlinearities in IV used in first-stage regression	No	Yes
Nonlinearities in all controls	Yes	No
All controls	Yes	Yes
Quarter FE	Yes	Yes
District FE	Yes	Yes
Observations	9539	9539
Cragg-Donald Wald F statistic	23.27	13.96
Kleibergen-Paap Wald rk F statistic	12.70	6.09
Anderson-Rubin Wald test p-val	0.032	0.072
Mean of dependent variable	0.013	

Note - 2SLS regression results. The sample includes all Palestinian children aged 10-17 who at quarter t (the time of the first interview) are enrolled in school and have the household head employed in Israel. The dependent variable is *Child school dropout*, a dummy variable which takes value 1 if a child is attending school in quarter t (the time of the first interview) and is not attending school in quarter $t + 1$ (the time of the second interview). The main explanatory variable, *Household head job loss*, is a dummy variable which takes value 1 if the child's household head is employed in Israel in quarter t and is not employed in Israel in quarter $t + 1$. The instrumental variable used in first-stage regression, *Fatalities*, measures the district-level number of Palestinians killed by the Israeli Defence Forces per 10,000 inhabitants by quarter. *All controls* include: 1) child-specific controls: gender, age, and years of schooling; 2) household head-specific controls: age, age squared, a set of dummies for the level of education, and a set of dummies for the employment status; 3) household-specific controls: size, number of children, number of members employed other than the household head, and type of residential location (rural, urban, refugee camp). *Nonlinearities in all controls* are addressed by including the quadratic terms of all continuous control variables and all the two-way interactions between the dummy control variables. *Nonlinearities in IV used in first-stage regression* are addressed by including the quadratic term of *Fatalities* as additional instrument in first-stage regression. Standard errors in parentheses are clustered at the district and type of residential location level. (Sources: Labour Force Survey - PCBS; BTSELEM.)

*, **, *** Significant at the 10%, 5%, 1% level, respectively.

Table A.7: ROBUSTNESS 2SLS RESULTS: Using alternative IV

	Child school dropout			
	(1)	(2)	(3)	(4)
Household head job loss	0.064** (0.030)	0.054* (0.031)	0.053** (0.025)	0.051** (0.024)
Child gender (male)		0.003* (0.002)	0.003* (0.002)	0.003 (0.002)
Child age		0.009*** (0.001)	0.009*** (0.001)	0.009*** (0.001)
Child years of schooling		-0.004*** (0.001)	-0.004*** (0.001)	-0.004*** (0.002)
Household-specific controls	No	No	No	Yes
Household head-specific controls	No	No	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Observations	9538	9538	9538	9538
Cragg-Donald Wald F statistic	41.97	40.3	53.2	49.44
Kleibergen-Paap Wald rk F statistic	13.56	14.13	19.31	17.77
Anderson-Rubin Wald test p-val	0.023	0.06	0.043	0.031
Mean of dependent variable			0.013	

Note - 2SLS regression results. The sample includes all Palestinian children aged 10-17 who at quarter t (the time of the first interview) are enrolled in school and have the household head employed in Israel. The dependent variable is *Child school dropout*, a dummy variable which takes value 1 if a child is attending school in quarter t (the time of the first interview) and is not attending school in quarter $t + 1$ (the time of the second interview). The main explanatory variable, *Household head job loss*, is a dummy variable which takes value 1 if the child's household head is employed in Israel in quarter t and is not employed in Israel in quarter $t + 1$. The instrumental variable used in first-stage regression is the predicted household head job loss obtained from a probit model of *Household head job loss* on *Fatalities* and all controls, where *Fatalities* measures the district-level number of Palestinians killed by the Israeli Defence Forces per 10,000 inhabitants by quarter. *Household head-specific controls* include: age, age squared, a set of dummies for the level of education, and a set of dummies for the employment status. *Household-specific controls* include: size, number of children, number of members employed other than the household head; and type of residential location (rural, urban, refugee camp). Standard errors in parentheses are clustered at the district and type of residential location level. (Sources: Labour Force Survey - PCBS; BTSELEM.)

*, **, *** Significant at the 10%, 5%, 1% level, respectively.

Table A.8: MECHANISM RESULTS: Household head becoming a fighter

	District-level number of Palestinians killed while fighting			District-level number of Palestinians older than 34 killed while fighting		
	at time t	at time $t + 1$	at time $t + 2$	at time t	at time $t + 1$	at time $t + 2$
	(1)	(2)	(3)	(4)	(5)	(6)
Job loss rate (HH heads employed in Israel)	0.065 (0.049)	-0.024 (0.060)	0.111 (0.115)	-0.012 (0.012)	0.022 (0.016)	0.019 (0.021)
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	358	358	358	358	358	358

Note - Panel fixed-effects regression results. The dependent variable is the district-level number of Palestinians killed while fighting (all in columns 1-2 and only those older than 34 in columns 3-4, respectively) by the Israeli Defence Forces per 10,000 inhabitants by quarter in the district of residence of the household. *Job loss rate (HH heads employed in Israel)* is the district-level job loss rate computed among household heads employed in Israel. Note that 35 is the age limit for work permit in Israel. Standard errors are clustered at the district level. (Sources: Labour Force Survey - PCBS; B'TSELEM.)

*, **, *** Significant at the 10%, 5%, 1% level, respectively.

Table A.9: MECHANISMS RESULTS: Parental divorce

	Parental divorce (1)
Fatalities	-0.001 (0.002)
All controls	Yes
Quarter FE	Yes
District FE	Yes
Observations	9502
Mean of dependent variable	0.003

Note - OLS regression (reduced-form) results. The dependent variable, *Parental divorce*, is a dummy variable which takes value 1 if the household head face a divorce from quarter t (the time of the first interview) to quarter $t+1$ (the time of the second interview). The main explanatory variable, *Fatalities*, measures the district-level number of Palestinians killed by the Israeli Defence Forces per 10,000 inhabitants by quarter, and serves as instrumental variable. *All controls* include: 1) child-specific controls: gender, age, and years of schooling; 2) household head-specific controls: age, age squared, a set of dummies for the level of education, and a set of dummies for the employment status; 3) household-specific controls: size, number of children, number of members employed other than the household head, and type of residential location (rural, urban, refugee camp). Standard errors in parentheses are clustered at the district and type of residential location level. (Sources: Labour Force Survey - PCBS; BTSELEM.)

*, **, *** Significant at the 10%, 5%, 1% level, respectively.