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How educational choices respond to large labor market shocks: Evidence from a natural experiment☆

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ABSTRACT

This paper uses the closure of the Israeli labor market as a large labor market shock and examines its impact on Palestinian youths' educational choices. The sudden closure denied thousands of Palestinian workers, mostly low-skilled males, access to the Israeli labor market. We provide evidence that the conflict-induced labor demand shock reduced the opportunity cost of attending school and consequently, male students' high school dropout rates.

1. Introduction

Human capital accumulation is one of the main factors contributing to economic growth (Mankiw et al., 1992). Youths, especially in developing countries, face many challenges that prevent them from obtaining an adequate level of education, and they are particularly vulnerable to changes in the labor markets conditions, both locally and abroad. The extent to which the educational choices of youths in a labor-exporting country respond to large and abrupt shocks in the destination country is an important question, and yet, it is only partially addressed.

Identifying the impact of a large shock on educational choices is challenging for two main reasons. First, large labor market shifts usually happen over a long period and are usually coupled with economy-wide reforms. Second, the conventional econometric techniques of causal inference might suffer from spillover and general equilibrium effects. This study attempts to overcome these two limitations by utilizing a natural experiment that restricted Palestinian workerss' access to the Israeli labor market during the Palestinian Second Intifada (uprising). This allows us to examine schooling choice responses in the origin country, which is characterized by isolated local labor markets, to a sudden, conflict-induced labor market closure in the top migrant-receiving country.

The closure of the Israeli labor market following the Second Intifada is suitable for addressing the question at hand for three main reasons. First, the shock was abrupt, sizable, and to a certain degree, unanticipated. The share of Palestinian workers in Israel (commuters) dropped markedly from comprising 25% of total Palestinian employment before the Second Intifada to comprising below 10% in 2002.[1] Second, immediately after the outbreak of the Second Intifada, Israel started to implement long-term measures and policies, such as building the Separation Wall, accelerating the immigration of foreign (non-Palestinian) workers, and effecting a strict permit policy that denied young Palestinian workers access to the Israeli labor market. Many Palestinians perceived these changes as long-term structural shifts, with protracted implications for

[1] Although the commuters' share increased in the years that followed, it did not revert to its pre-Intifada period value.

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the dynamics between the two labor markets.[2] Finally, Israel severely restricted mobility within the West Bank (WB) during the Second Intifada, effectively turning the WB into a geographically isolated labor market.[3]

The closure might have induced youths (aged 15–19 years old) in the WB to invest more in education by lowering the opportunity cost of attending high school and increasing returns to schooling. Low-skill jobs in Israel were largely outsourced to the Palestinian labor market before the Intifada, generating a huge demand for unskilled workers who were employed and earning relatively higher wages compared to the wages for similar jobs in the WB. Restricted access to the Israeli labor market greatly reduced the demand for unskilled workers, resulting in fewer employment prospects and lower wages for school dropouts. Further, for parents, the shock constituted a sharp negative income shock. Faced with liquidity constraints, parents could not afford to send their children to school, resulting in an increase in the high school dropout rate. We carry out our analysis at the local labor market level to identify the shock's overall effect on educational choices. This provided us with a unique opportunity to identify the impact of a large shock, which simultaneously affected remittances and the opportunity cost of schooling, on human capital formation.

The responses of schooling choices in home to labor market shocks in foreign countries have never been more relevant in the global economy, which is characterized by a growing number of migrants, a great degree of regional market interconnectedness, constant technological change, and policy- or conflict- induced shocks. Migrants with relatively similar skills are usually concentrated in a very limited number of destinations; therefore, it is important to understand how labor market changes affect the top migrant-receiving countries in terms of the human capital consequences in the migrants' origin countries. As the overall impact of migration on human capital will be dominated by one of two opposing mechanisms (i.e., income vs. the opportunity cost of schooling), this study is relevant to designing future education-enhancing policies in developing countries, which are characterized by a large concentration of unskilled migrants who are concentrated in a very small number of destinations.

Measured as the locality share of commuters in total male employment, our empirical strategy relies on the variations in exposure to the Israeli labor market across WB localities before the Second Intifada. We start with the difference-in-difference estimator (DD) to analyze the high school dropout rate among the youths in the treated localities (with a high pre-shock commuting share) against the comparison localities (with a low pre-shock commuting share) both before and after the closure. However, because most commuters were males (more than 98%), the shock was asymmetrical not only across localities, but also across genders. This allows us to extend our DD analysis to the triple difference framework (DDD). The DDD estimator compares the male school dropout rate to the female one within the treated localities, purging out gender difference in schooling in comparison localities both before and after the shock, while controlling for locality time-varying characteristics that could bias the DD estimator. Additionally, we show that the data support the main identifying assumption of equal high school dropout rate trajectories in the treated and comparison localities before the shock. Further, we provide evidence against the compositional effect (changes in the observable variable driving our results) and internal migration.

Using Palestinian Labor Force Survey (PLFS) data from 1999 to 2006, we find that the probability of dropping out of high school declined following the closure. Importantly, the effect is driven by the responses of male, not female, students, leading to a smaller gender gap for high school dropouts. However, we did not find any significant effect of the closure on college attendance. The opportunity cost mechanism seems to dominate the income mechanism in the case of high school enrollment (there is almost no cost for attending high school in the WB), whereas the income mechanism counterbalances the opportunity cost and the returns to education mechanisms for college education, which is relatively expensive.

The closure presumably reduced the opportunity cost of schooling by lowering job employment prospects for school dropouts. We test for the opportunity cost mechanism by demonstrating that school dropouts' unemployment probability increased more in highly-exposed localities compared to low-exposure localities. If the prospect of employment is the true mechanism, one would expect to see closure exert a differential impact on unemployment across gender and skilled vs. unskilled workers within localities, with the differential effect being more salient in the treated as opposed to the control localities. We provide evidence that unemployment probability increased more for male than female dropouts as well as for males with educational degrees in hard-hit localities compared with those in low-hit localities. However, we find little evidence for the skill wage premium. We conduct several sensitivity, placebo, and heterogeneous effect analyses to test the robustness of our results and rule out alternative mechanisms. Taken together, this studys's results suggest that the closure reduced the opportunity cost of attending school for male students, thereby leading to a decline in the high school dropout rate in the WB.

Our paper is related to three strands of literature. The first is broad literature on how human capital responds to economic shocks (Charles et al., 2015; Shah and Steinberg, 2017), skill-biased technological changes, and labor market shocks (Black et al., 2005; Cascio and Narayan, 2015; Emery et al., 2012; Michaels, 2011; Weber, 2012; 2014), as well as to policy reforms and new economic opportunities (Abramitzky and Lavy, 2014; Adukia et al., 2019; Amuedo-Dorantes and Antman, 2017; Atkin, 2016; Kuka et al., 2018; Oster and Steinberg, 2013). Compared with the above-mentioned studies, the magnitude of shock in this study is much larger. The shock is coupled with political violence and a sharp decline in economic activities. We add to the literature by providing further evidence that in an imperfect labor market, human capital responds to a large labor shock in a manner that is consistent with previous studies, thus emphasizing the idea that in an unstable economic and political environment, the opportunity cost of attending school is still an imperative driver of educational choices.

The second strand of literature concerns the multi-channel impact of migration on education in the country of origin through remittances (Cox-Edwards and Ureta, 2013; Yang, 2008), wage premiums (Batista et al., 2012; Shrestha, 2017), or the overall effect (McKenzie and Rapoport, 2011; Theoharides, 2018). It is important to underscore the differences between our setting and typical migration literature. First, Palestinian workers are not allowed to stay overnight in Israel; therefore, they must commute to Israel daily to work and return to the WB at night. Therefore, in our setting, the usual concerns about unobservable individual variables correlated with decisions to migrate and remit are less prevalent. Second, in our setting, we can arguably rule out migration's effect on education through other mechanisms such as parental absence and family disruptions. Third, unlike most studies on migration, instead of targeting the intergenerational transmission of education and brain drain, we focus on how unskilled-biased demand for migrants impacts prospective migrants (commuters) as human capital. Although our setting is specific, and perhaps unique, our results can be generalized to all settings in which migrants from the origin country are concentrated in jobs that require a specific skillset and particular educational qualifications.

In the spirit of Theoharides (2018), this paper identifies the overall effect of a labor market shock in the destination country on educational choices at the local labor market level. She finds that education in the

[2] Evidently, the share of commuters to Israel increased to 15% (10 percentage points less than the pre-shock level) at the end of 2012 (which was almost 7 to 8 years after the end of the Second Intifada). For the impact of foreign workers on labor market outcomes in Palestine refer to (Aranki and Daoud, 2010; Miaari and Sauer, 2011).

[3] World Bank (2007a) assessed the effect of mobility restrictions on the WB's economy.

Philippines responds positively to migration, driven mainly by the income effect. In contrast, our findings suggest that the opportunity cost of attending school dominates the income effect when migrants are relatively unskilled and high school education is relatively cheap. In our setting, the prospect of working abroad in relatively highly-paid low-skill jobs lowers returns to schooling and students' incentives to invest more in education. Our results are, to some extent, in line with McKenzie and Rapoport (2011), who find that migration from Mexico to the United States (US) led to lower rates of school completion among migrants' children in Mexico because their return to education is higher in Mexico compared with Mexican migrants in the US. Our findings are not necessarily trivial because the importance of the counteracting mechanisms, that is, remittances (budget constraint) and incentives, might operate differently for positive and negative shocks. The size of the shock in the current study is comparable to that of Dinkelman and Mariotti (2016), who estimate the impact of large shocks in South Africa as a destination country on human capital in Malawi. They find that in Malawi, in the long run, remittances have a positive effect on children's educational outcomes.

Finally, this paper contributes to three important previous studies that address the impact of the Second Intifada on Palestinians' educational attainment. Di Maio and Nistico (2019) find that among household heads working in Israel, parental job losses that were induced by the intensity of the conflict during the Second Intifada had a negative effect on the school dropout rate. By conducting their analysis at the household level, they capture the effect of income on schooling. Conversely, our study estimates the shock's overall effect at the locality level, isolating the effects of labor market changes from the violence effect. Brück et al. (2019) show that while violence lowers high schoolers' national exam scores, it does not have the same effect on the high school dropout rate at the locality school level. These findings further support our identification strategy for two reasons: (1) they show that within localities, change in violence over time is as good as random, and hence, it might not be correlated with the pre-shock share of commuters; and (2) by finding that violence had no effect on the school dropout rate, we are confident that our results are driven by changes in the labor market.[4]

This paper is closely related to Di Maio and Nandi (2013), who find that child labor increases with conflict intensity as measured by within-year quarterly fluctuations in the number of days Israel closed its border with the WB, while school attendance weakly decreases for children aged between 10 and 14 years old. However, our paper differs from Di Maio and Nandi (2013) in an important way. In Di Maio and Nandi (2013), child labor increases as a short-term insurance strategy for coping with economic hardship caused by the temporary border closure, but children continue to attend school. The decision to work and attend school is not mutually exclusive for 10–14 year old boys because the majority of working children (mainly in unpaid and part-time jobs at family-owned businesses) also attended school. In contrast, our empirical design captures the long-run effect on the high school dropout rate of a perceived long-term decline in the opportunity cost of attending school, following the initial closure and the long-term policies Israel adopted shortly after to restrict Palestinians' access to its labor market. In addition, by comparing schooling outcomes between localities before and after the shock, our empirical strategy is more successful in controlling for unobservable economy-wide variables that might be correlated with the number of closure days and school attendance, such as violence and school closures.[5]

In addition, by providing a cleaner identification strategy, this paper complements the literature on the Second Intifada's impact on the WB's labor market (Cali and Miaari, 2018; Fallah, 2016; Farsakh, 2002; Mansour, 2010; Miaari and Sauer, 2011; Ruppert Blumer, 2003). The DDD technique controls for unobservable locality time-varying characteristics that might bias estimates of the Second Intifada's impact on locality labor market outcomes. In line with the findings of the aforementioned studies, this study confirms the closure's adverse impact on employment and wages in the WB, especially for unskilled workers.

The remainder of the paper is organized as follows. Section 2 provides a brief background of the labor market and education system in Palestine. Section 3 discusses the data and provides descriptive statistics. In Sections 4 and 5, we discuss the empirical design, results, and underlying mechanisms. Section 6 concludes the paper.

2. Institutional background

Immediately after Israel occupied the WB and the Gaza Strip, the Israeli government granted Palestinian workers full access to seek employment in its labor market (Farsakh, 2002). High unemployment in the WB and the Gaza Strip, coupled with the availability of higher wages in the Israeli labor market, induced many Palestinian workers to commute to Israel. By 1970, the share of commuters accounted for one-third of the total Palestinian workforce.

After the outbreak of the First Intifada[6], Israel started implementing a closure policy. The Israeli government adopted a permit regime that was responsible for granting access to its labor market based on several criteria, including age, marital status, and security clearance. The extent to which this policy is enforced has depended on the intensity of violence and security needs. However, the Israeli labor market continued to employ a large proportion of male Palestinian workers throughout the 1980s and 1990s, with the share of commuters accounting for more than 40% of the total workforce in some years (Angrist, 1996; Ruppert Blumer, 2003). The share of commuters not only varied significantly across time, it also varied markedly across districts and localities within districts—an aspect that is vital to our empirical methodology (refer to Online Appendix, Table A.2 there will be more on this in the upcoming sections).

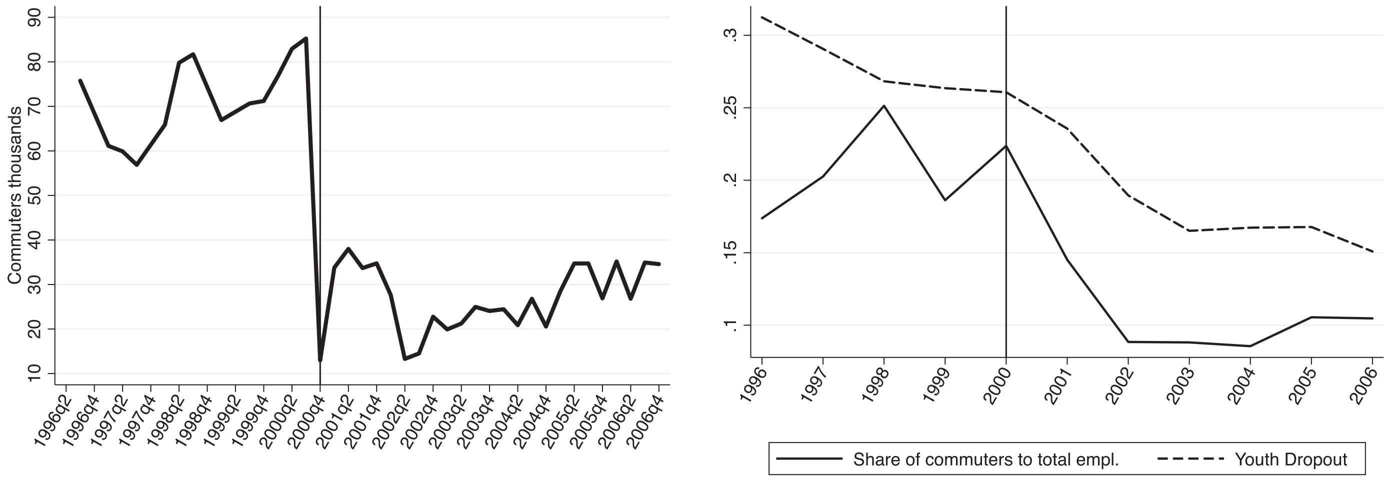
On the eve of the Second Intifada, in October 2000, Israel closed its border with the WB, suspending work permits for most commuters, thus leading to a significant decline in the share of commuters. The number of Palestinian workers in Israel tumbled from around 85,000 to 15,000 in the first few months following the Second Intifada. The number increased slightly in the first two quarters of 2001, only to fall below 20,000 in 2002. The number of workers has not reverted to its pre-Intifada level, staying below 35,000 (see Graph (a) of Fig. 1). Graph (b) of the same figure shows the year-by-year share of commuters from 1996 to 2006. The commuting share dropped from 22.5% in 2000 to less than 10% for the period 2002–2004, recovering marginally to 11% in 2006.

The strength and length of commuting restrictions in the Second Intifada indicated a fundamental shift in the dynamics between the two labor markets. Prior to the Second Intifada, Israel relied on the WB and the Gaza Strip as its main sources of cheap, unskilled workers. The occasional imposition of stringent commuting restrictions prior to 2001 was perceived as a temporary measure taken in response to shifts in the political environment. After 2001, Israel began to adopt long-term policies

[4] An older piece of literature examined returns to schooling in the WB and Gaza during the 1980s and the 1990s (Angrist, 1995; 1996).

[5] To compare our results with Di Maio and Nandi (2013), we estimate the closure's effect on children's (aged 10–14 years old) education, using our empirical framework. As expected, we find that the closure had no long-term effect on school attendance for children aged between 10 and 14 years old. Thus, children's schooling is minimally affected by labor market outcome changes, and employment prospects for dropouts as job opportunities for children are restricted and mostly limited to unpaid jobs at family businesses.

[6] The First Intifada started in 1987 and ended in 1991 (after the Madrid Conference). Eventually, the Palestinian Authority was established in 1993, following the Oslo Accords. They took control of administrative issues, such as taxation, education, health, and legislation, in the WB and the Gaza Strip. The Israeli government maintained its control over borders and major security issues.



(a) Total number of commuters (b) Commuters Share

Fig. 1. The share of commuters and dropout ratios (WB excluding East Jerusalem). **Notes**: Graph (a) shows the quarterly number of Palestinian workers in Israel. In Graph (b), the solid and dashed lines show the annual share of commuters in total employment and the ratio of school dropouts for youths aged between 15 and 19 years old, respectively. The vertical line indicates the beginning of the Second Intifada. Data source: The Palestinian Labor Force Survey (PLFS).

that aimed to structurally alter the relationship between the two markets. Particularly, Israel accelerated its importation of foreign workers as substitutes for Palestinian workers and began building the Separation Wall between itself and the WB. By mid-2001, Palestinians had noted these structural changes and updated their expectations about regaining access to the Israeli labor market in the near future accordingly (Mansour, 2010).

The Second Intifada and subsequent Israeli policies had severe ramifications for the Palestinian economy, especially with regard to the unemployment rate and low-skilled workers' wages, with the overarching effect of transforming the structure of the WB's labor market. Figs. A.1-A.2 in the Online Appendix plot the share of commuters, the unemployment rate, and the skill wage premium from 1996 to 2006 to visualize the correlation between commuting shares and labor market outcomes.[7]

Was there a difference between commuters and non-commuters before the Second Intifada? Were commuters concentrated in low-skill jobs in the Israeli labor market? Our argument is that the closure of the Israeli labor market had a differential impact on workers with different skills, and thus on schooling choices. In other words, the closure constituted a positive unskilled labor supply shock and, importantly, a negative demand shock for existing and prospective unskilled workers.

Data from the 1999 PLFS show that commuters were less educated than non-commuters, with the former mostly being concentrated in low-skill jobs (industries). In 1999, non-commuters' average years of schooling exceeded commuters' years of schooling by one year. The years of schooling distributions reveal stark differences between commuters and non-commuters (refer to Fig. 2 and Table 1). For instance, 18% of commuters had at least a high school (or above) qualification, while only 1% earned a bachelor's degree or above. Conversely, 34% and 12% of non-commuters had a high school and bachelor's degree, respectively.

Most commuters were male (98%) and performed low-skill jobs, with approximately 82% holding craft and elementary occupations. In terms of industry type, in 1999, 68% of commuters were employed in the construction sector, followed by agriculture (10%), and manufacturing (10%).

Commuters were, on average, four years younger than non-commuters (30- vs. 34 years old). Approximately, 29% of male youth

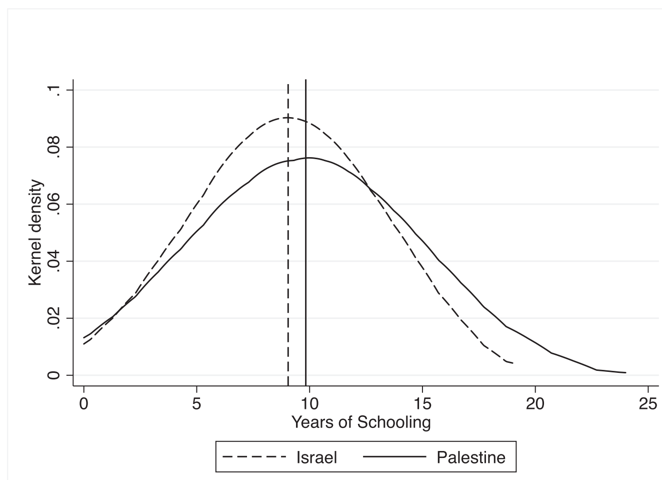


Fig. 2. The distribution of years of schooling for male workers by place of work in 1999. **Note**: The distributions of years of schooling for males working in Israel and the WB in 1999 are shown by the dashed(-) and solid distributions, respectively. The dashed and solid vertical lines denote the mean years of education for workers in Israel and the WB, respectively. Data source: PCBS: The Palestinian Labor Force Survey (PLFS).

workers were employed in Israel (compared to around 24% of adults), and youth comprised 13% of the total commuters. Table A.1 in the Online Appendix shows that the returns to education for commuters were negligible. Nonetheless, on average, commuters' daily wage was almost 40% higher than non-commuters'.

2.1. The palestinian school system

School education in Palestine consists of two stages. The compulsory primary level or the first stage consists of Grades 1 to 10. Students start the primary level at 6 or 7 years old and finish Grade 10 around the age of 16. The second stage consists of Grades 11 and 12 and is non-mandatory. Although both Palestinian society and the government place a high value on education, the parents of primary school dropouts do not face legal consequences (Nicolai, 2007). However, the data show a non-trivial primary school dropout rate, especially after Grade 8. Subse-

[7] The Second Intifada (2000–2006) is considered one of the most violent episodes of the ongoing Israeli–Palestinian conflict. More than 4000 Palestinians and about 1000 Israelis were killed, according to a B'Tselem report in 2007.

Table 1
Characteristics of commuters vs. non-commuters in 1999.

Variables	Place of Work	
	West Bank	Israel
Years of education	9.8	8.9
High school degree or above	0.34	0.18
Bachelor degree of above	0.12	0.01
Higher education	0.02	0.00
Urban	0.50	0.34
Rural	0.43	0.59
Age	34	30
Daily wage	66	102

The table shows the mean values of the number of years of education, age, and daily wages of male workers by place of work. It also shows the proportion of workers who have a high school degree or above, a bachelor's degree or above, and higher education by place of work. The numbers corresponding to Urban and Rural show the proportion of workers who come from urban and rural areas by place of work. Daily wages are measured in Israeli shekel. Data source: The PCBS Labor Force Survey (PLFS).

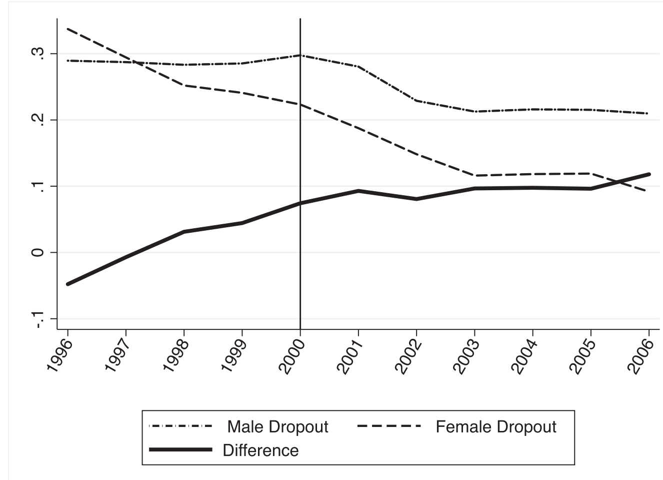


Fig. 3. Youth high school dropouts by gender. **Note**: The dashed lines show the school dropout rates for males and females aged between 15 and 19 years old, respectively. The solid line traces the male female dropout gap. The vertical line indicates the beginning of the Second Intifada. Data source: The Palestinian Labor Force Survey (PLFS).

quently, we focus on the school choices of tenth-, eleventh-, and twelfth-graders. Approximately 95% of students in Grades 10–12 are between 15 and 19 years old (Brück et al., 2019). Palestine's Ministry of Education and Higher Education applies a strict month- year birthdate entry cutoff for Grade 1, resulting in an age gap of up to 11 months between students in the same grade. In addition, class repetition might slightly increase the average age of high school students. Less than 5% of students repeat a class during elementary education. More importantly, about 30% of students in Grade 12 fail the national high school exam (the Tawjihi exam is equivalent to the SAT exam). Students must pass the exam if they wish to continue on to college education.[8] The Tawjihi exam is conducted annually, toward the end of the academic year (May/June), and until recently, students had to wait at least one year to re-take it.[9]

During the Second Intifada, the WB's education system had to endure many challenges. Some schools were partly or totally damaged by Israeli forces; students, teachers, and parents were subjected to extreme physical and psychological violence; and mobility measures, such as checkpoints and curfews, that were imposed by Israel made it very hard for students and teachers to reach their schools. Despite these obstacles, the schooling system continued to function relatively well in the WB (Brück et al., 2019). Interestingly, there was a rapid decline in the youth dropout rate after 2000 (Brück et al., 2019; Nicolai, 2007; World Bank, 2007b).

Aggregate data from the Educational Statistical Yearbook, which are publicly available on the Palestinian Census Bureau of Statistics (PCBS) website, show that school attendance in the WB for students aged 15–17 increased by 11 percentage points from 2000–2006 (from 72.4% in 2000 to 83.6% in 2006). In our sample, using micro-data on schooling (see the next section for a data description), the youth (aged 15–19 years old) school dropout ratio declined by about 13 percentage points (from 28% in 1999 to 15% in 2006—refer to Fig. 1). It is evident that the decline in dropout rate two to three years after the onset of the Second Intifada, accelerated and followed a relatively flat pattern after 2004. Fig. 3 plots the annual dropout rate for youth by gender. The Figure reveals that females had a lower dropout rate than males for almost the entire period. Importantly, the educational gap widened at an accelerated rate before the Second Intifada, taking on a flat trend afterward. The revelation that is demonstrated in Fig. 3 is central to our argument: the shock stemming from the closure is asymmetrical across genders, and thus, it is expected to have a differential impact on the high school dropout rate for males and females.

3. Data and descriptive statistics

Our primary dataset source is the PLFS collected by PCBS. The PLFS data are collected on a quarterly basis, with a nationally representative sample of approximately 7600 households in the WB and the Gaza Strip. The PLFS contains a rich plethora of information about individual and household characteristics, including employment status, daily wage, sex, age, years of schooling, whether a person is a school dropout, place of work and residence by locality, marital status, and number of household members. The quality of the PLFS dataset is well documented, as it has been used in many papers that were published in high-quality economic journals (Amodio et al., 2019; Amodio and Di Maio, 2018; Cali and Miaari, 2018; Di Maio and Nistico, 2019; Mansour, 2010).

In the main empirical analysis, we use PLFS data for the period 1999–2006. We restrict our analysis to 2006 (the unofficial end year of the Second Intifada) because mobility restrictions within the WB were eased thereafter. For some specifications and robustness analyses, we extend the period of analysis to 2010 to examine the closure's long-term effect on educational choices. The locality-level place of residence data are only available from 1999; this restricts our analysis of the pre-shock trend in the context of school dropouts to only two years. To overcome this limitation, we use a unique household identifier to match households that appeared before and after 1999. This approach allows us to identify household locality in 1997 and 1998. Due to the sampling techniques that PCBS used, only a relatively small number of observations in 1997 and 1998 were matched.[10] Therefore, we only use the extended

[8] Some universities require a score of at least 70%.

[9] Youths who never attended school or who dropped out early (i.e., before Grade 8) were dropped from our sample because those school choices might not have been related to the shock or labor market changes.

[10] Observations in any quarter comprise roughly half of the individuals (households) interviewed in the previous quarter, plus another half consisting of randomly drawn households. By design, each household is interviewed four times: in quarters 1 and 2 of year X and quarters 1 and 2 of year X + 1. To deal with the rotating panel sampling issue, repeated panel observations in a specific year were deleted, except for the first appearance. Eventually, the final sample consisted of randomly repeated cross-sectional observations, as presented in the current study's proposed econometric specifications.

Table 2
Summary statistics of the main variables of localities by share of commuters in 1999.

Variables	Descriptive Statistics Year 1999					
	Bottom 50th		Above 50th		All	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Commuters						
Share of commuters in total employment	0.12	0.07	0.42	0.13	0.25	0.18
Educational Attainment						
Dropout ratio for youth	0.25	0.12	0.30	0.13	0.27	0.13
Dropout gender gap	0.06	0.18	0.07	0.21	0.07	0.19
Years of schooling (all population)	7.92	1.06	7.32	0.77	7.67	0.99
Baseline locality characteristics						
Unemployment rate	0.10	0.07	0.14	0.09	0.11	0.08
Ratio of waged workers to total workers	0.62	0.15	0.69	0.12	0.65	0.14
Share of population younger than 16 years old	0.23	0.05	0.25	0.05	0.24	0.05
Urban areas	0.48	0.50	0.19	0.40	0.36	0.48
Rural areas	0.36	0.48	0.71	0.46	0.51	0.5
Other locality Characteristics						
Share of population out of labor force	0.62	0.07	0.66	0.05	0.64	0.06
Av. Daily local wage	61.21	15.66	58.70	11.70	60.18	14.18
No. of household members	6.11	0.87	6.45	0.92	6.25	0.9

The localities are divided, based on the share of commuters in 1999, into the following two groups: localities in the lower half of the commuting share distribution (bottom 50th) and localities in the upper half of the distribution (top 50th). The average is weighted by locality size.

dataset (1997–2006) to further examine trends across localities that existed before the shock.[11]

We restrict our analysis to localities in the WB, excluding East Jerusalem and the Gaza Strip because localities in East Jerusalem were not subject to mobility restrictions either to Israel or internally. Furthermore, our identification strategy hardly applies to the Gaza Strip because labor market localities in the Gaza Strip contiguously form a single labor market. As a robustness check, we treat the Gaza Strip as one big locality and include it in our analysis.[12] We also use Jerusalem as a counterfactual to the WB, demonstrating that high school dropout rates were similar for the two regions, only starting to diverge after the Second Intifada.

In addition to the PLFS, we use two different data sources. First, data on Palestinian fatalities related to the Second Intifada are obtained from the Israeli NGO (B'TSELEM) and can be downloaded directly from B'TSELEM's website. It is a rich dataset that provides information about the victims' age, sex, gender, place of residence, and other data, along with a detailed description of incident. Again, this dataset has been widely used in the literature (e.g., Mansour and Rees, 2012). Second, data on the geographical coordinates and distances between localities and the nearest crossing point to Israel are provided by the Applied Research Institute.

In the PLFS, a youth dropout is identified if his/her response to the question "Are you attending school" is "I attended and dropped out." We cross-check the answers to the attendance question with the years of schooling variable to construct our binary measure of school dropouts.[13] However, one might worry that the analysis might suffer from measurement error because some respondents who were out of school at the time of the interview might rejoin school later and would be erroneously classified as dropouts. To deal with this measurement error issue, we develop two alternative measures and illustrate that the results are almost identical. In the first alternative, only respondents who are out of school and have a schooling gap that amounts to at least 2 years compared with the average student their age are considered dropouts. In the second alternative, relying on the rotational nature of the quarterly PLFS data, a youth is considered a school dropout only if he/she has been out of school for two consecutive quarters.

The yearly locality-level variables were aggregated using the PLFS probability weights. Our final sample consisted of 162 (out of a total of 284) localities in the base year 1999 across the WB's ten districts. Based on the PCBS Population, Building and Establishment Census of 1997, the 162 localities in our sample account for more than 85% of the WB's total population.[14] Table 2 summarizes the main locality-level indicators by the share of commuters in 1999 (the base year). A quick inspection of Table 2 demonstrates otherwise relatively similar localities in 1999, with few exceptions. A significant similarity between localities lends additional credibility to our identification. Nonetheless, to rule out the possibility that our results might have been driven by the small observed pre-shock differences across localities, we control for many locality-level characteristics in our preferred econometric specifications. These differences are of less concern in the DDD model because it compares schooling choices across genders within a locality over time.

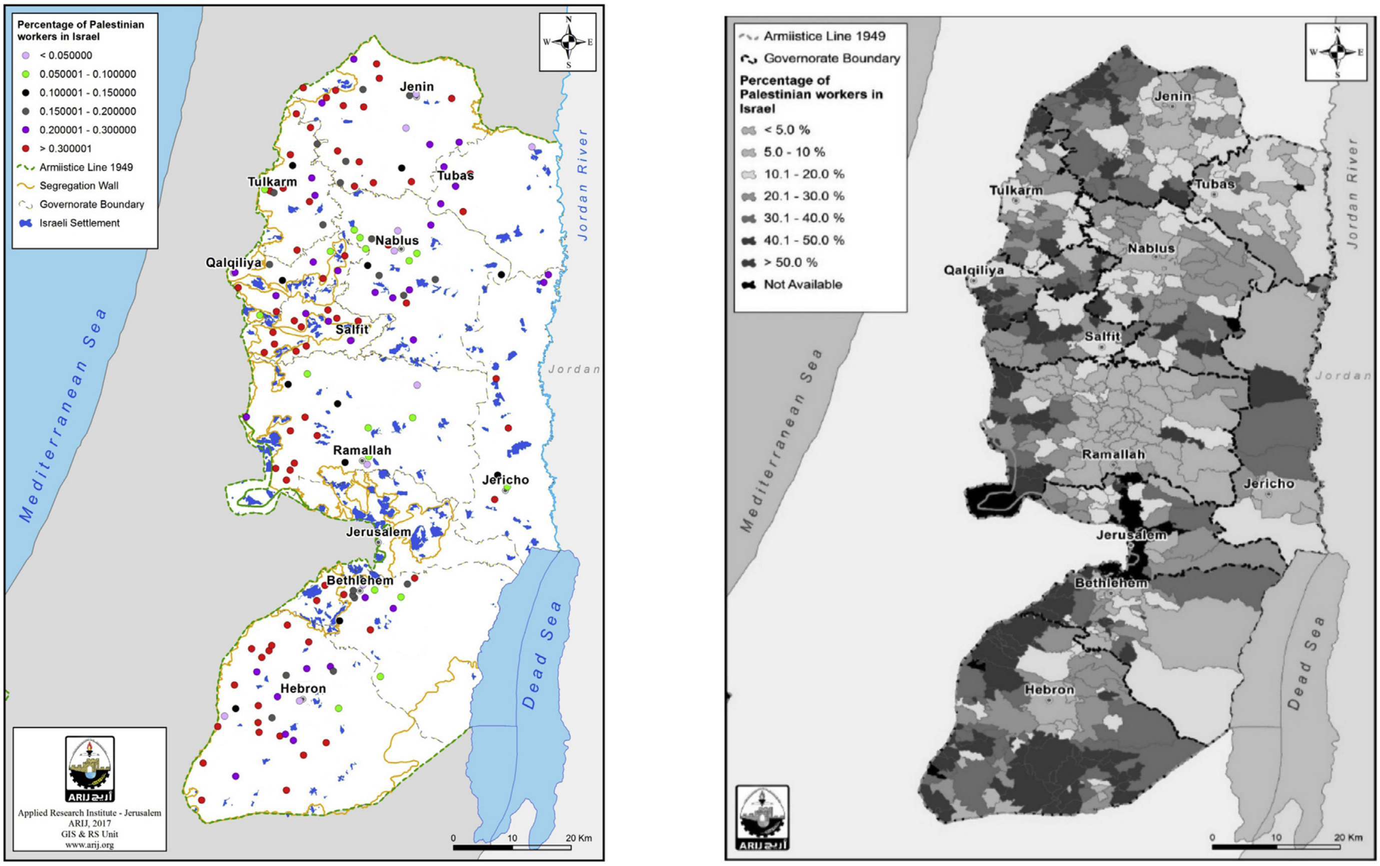
While finding that localities are similar in terms of demographic and labor market characteristics in 1999 is reassuring, it is important to discern factors that might explain the variations in commuting share across localities. A promising approach is to rely on historical locality-level data on the number of workers in Israel before 1999. However, unfortunately, such a dataset does not exist. Miaari et al. (2014) make a serious attempt to explain the spatial variation in commuting share in the WB before the Second Intifada. By relying on Israeli administrative datasets on the number of work permits that were granted to Palestinians, the authors show that the variations within the locality with regard to the number of workers in Israel can be explained by the following two main factors: (1) the locality's distance from Israel, given that localities near the Israeli borders have more commuters, and considering that commuting distance matters a great deal because Palestinian workers, who are

[11] Nonetheless, this paper's main results are robust to using the 1997–2006 dataset.

[12] In addition, the trajectory of the Gaza Strip economy diverged from the WB's due to the blockade and several devastating wars that were initiated by Israel.

[13] Before 2003, only two answers to the question were provided, namely attending or not attending. From 2003 onward, the options changed to attending, attended and dropped out, never attended, or attended and graduated. The latter respondent type, attended and graduated, is considered a dropout if the corresponding years of schooling amount to less than 12.

[14] Many localities in the WB are small villages, with populations of less than 200 people. According to PCBS, small localities do not necessarily get surveyed every year. Although we carried out our analysis using the unbalanced dataset of 162 localities, our results are robust to using a balanced dataset comprised of the largest 95 localities.



(a) Spatial commuters Share 1999

(b) Spatial commuters share 2017

Fig. 4. The map of the WB. **Notes**: We would like to thank the Applied Research Institute for providing us with the maps. Data source: The Palestinian Labor Force Survey (PLFS) for Graph (a) and the PCBS Population Census 2017.

not allowed to stay overnight in Israel, must commute on a daily basis; and (2) networks, given that the recruitment of new Palestinian workers in Israel was largely facilitated by previous commuters, especially relatives and friends residing in the same locality.

Using our dataset, we visualize the correlation between the distance to Israel and the commuting share in 1999 in Graph (a) of Fig. 4. The graph shows a map of the WB, the Armistice Line (the border with Israel), the localities, and the share of commuters (indicated using different colors). Evidently, localities with a high commuting share are clustered near the border with Israel. To examine the stability of the spatial distribution of commuters in the WB over time, we use data from the Palestinian census in 2017 (i.e., eleven years after the end of the Intifada).[15] The map in Graph (b) of the same figure shows the geographical distribution of commuters in 2017. The similarity between the two maps is remarkable, indicating a strong, stable association between commuting and geographical location, and the persistent effect of networks. Overall, while we cannot completely rule out that the share of commuters in 1999 might be correlated with time-varying unobservable variables, the above analysis suggests that the locality fixed effect will soak out a lot of the non-random cross-locality variations of the commuting share in the base year. In the upcoming sections, we will address this issue formally using various econometric techniques and a robustness analysis.

The average gender gap among high school dropouts is almost identical for localities belonging to the lower and upper half of the commuting share distribution. Nonetheless, the youth dropout rates seem to be slightly higher for highly-exposed localities.[16] As discussed by (Kahn-Lang and Lang, 2019), this might pose some difficulty in interpreting the common pre-shock trends; it therefore warrants a serious discussion about the DD model's functional form, particularly if the outcome is trending upward or downward before the shock. We observe that the differences in dropout rates across localities (districts) remained constant during the pre-shock periods, indicating an equal decline in dropout rates as represented in percentage points; however, this was not so after the shock (see Fig. 7).

4. Empirical Design

To estimate the closure's effect on youth dropouts, we use a DD model as our starting point:

$$Y_{ilst} = \alpha + \beta S1999_l \times Post + \boldsymbol{\theta_1'} \boldsymbol{X1999}_l \times Post + \boldsymbol{\theta_2'} \boldsymbol{W}_{ilst} + T_t + \lambda_l + \lambda_l.t + \gamma_{st} + \epsilon_{ilst} \quad (1)$$

The binary variable Y_{ilst} equals one if the individual i in a particular locality l, district s, and year t is a high school dropout; otherwise, it is zero. The locality's share of commuters in 1999 in the context of the

[15] PCBS conducts the Population, Housing, and Establishment Census every 10 years, starting from 1997. PCBS began to include a question about place of work only in the 2007 census, and hence, we could not identify commuters in the 1997 census.

[16] The correlation between commuting share and dropout rates in 1999 is about 0.13.

locality's total male employment, $S1999_l$, captures the locality's exposure to the shock. $S1999_l$ is interacted with the treatment period dummy *Post*. The dummy variable *Post* takes a value of zero for 1999 and 2000, and a value of one for the remaining years up until 2006.[17] The main coefficient of interest, β, is the DD estimate, comparing the probability of becoming a high school dropout both before and after the shock in localities with different commuting shares in 1999.

We also include several locality- and individual-level controls. We interact the *Post* with several baseline locality control variables that were measured in 1999 and collected in the vector $X1999_l$. These include the unemployment rate, the share of individuals under the age of 16 in the total population, the share of wage workers as a fraction of total employment, and urban and rural area dummies (with camps treated as the reference group). These variables are included to control for factors other than the labor market shock that might have affected the future trajectories of localities' dropout rates. Their inclusion also ensures that the commuting shock did not confound any other factors. This would be the case if, for example, cross-locality differences in the commuting share correlate with poor economic conditions, leading to a possible change in these variables' post-shock impact on schooling choices. Individual- and household-level controls, collected in the vector W_{ilst}, include years of education for both parents (household heads), age and sex dummies, the number of members in the household under the age of 15, and the total number of household members.

Year dummies, T_t, control for yearly shocks that are common to all localities. The locality fixed effects, λ_l, remove the unobservable time-invariant, locality-specific factors. We include the locality-specific time linear trend, $\lambda_l.t$, to control for the locality-omitted variables that change linearly within localities. Additionally, by de-trending locality high school dropout rates, we rule out the possibility of the results being driven by the locality time trend instead of the treatment. We also control for the district-by-year fixed effect, γ_{st}, to account for unobserved time-varying factors that are common to localities in the same district. The error terms ϵ_{ilst} are clustered at the locality level.

A major identifying assumption in estimating Eq. (1) is that the school dropout rates across localities with different commuter shares before the Second Intifada would not have evolved differently in the absence of the Second Intifada (a parallel trend assumption). While testing this assumption directly is not feasible because we do not observe post-counterfactual trends, we estimate the following generalized DD model to examine the pre-shock trends across localities:

$$\begin{aligned} Y_{ilst} = \alpha + \sum_{\tau\neq 1999} (\beta_\tau S1999_l \times T_t^\tau) + \sum_{\tau\neq 1999} (\boldsymbol{\theta}_1' \boldsymbol{X1999}_l \times T_t^\tau) + \boldsymbol{\theta}_2' \boldsymbol{W}_{ilst} \\ + T_t + \lambda_l + \gamma_{st} + \epsilon_{ilst}, \end{aligned} \tag{2}$$

T_t^τ denotes the individual year dummy that takes a value of one if $\tau = t$, for $\tau \neq 1999$, and zero otherwise. The coefficient for the baseline year 1999, $\beta1999$, is set to zero. The remaining variables are defined above. This setting informs us whether differences in pre-existing trends would confound our findings. If the coefficients, β_τ, for the pre-shock period are statistically indistinguishable from zero, this suggests that the outcomes of interest were not following different trajectories in localities with different pre-shock commuter shares.

We estimate Eq. (2) using the extended dataset (1997–2006) to check whether the paths for the youth dropout rates were evolving differently before the shock for localities with different commuting shares in 1999. Fig. 5 visualizes the annual treatment estimates using Eq. (2) and controlling for the baseline locality and individual covariates. It shows that the treatment coefficients for the years 1997, 1998, and 2000 are small and statistically insignificant, suggesting a common trend among high school dropout rates across localities before the shock. Fig. 6 shows the estimates of Eq. (2) using the main dataset for the period 1999–2006. Again, the coefficient for 2000 is small and insignificant. The results also show that the impact of the Israeli market's closure occurred in 2001,

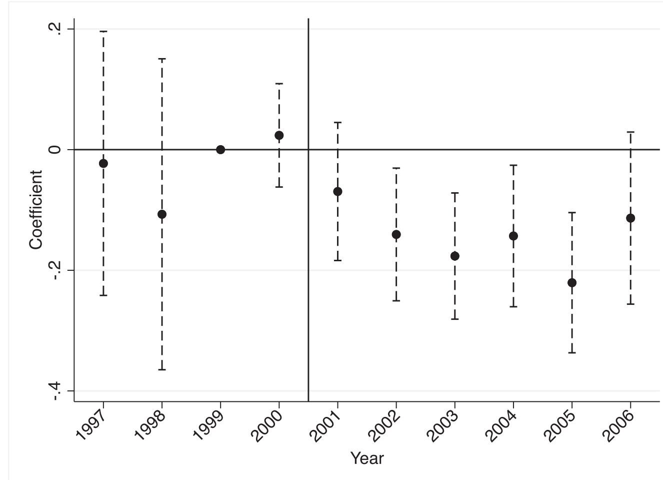


Fig. 5. The closure's impact on the high school dropout among youth: Extended dataset for the period 1997–2006. **Notes**: The graph plots the coefficients from the generalized DD regression of the school dropout dummy on a set of year indicator variables interacted with the share of commuters in 1999. The regression controls for locality and district-year dummies, locality baseline characteristics in 1999 interacted with the corresponding year's dummy, and individual characteristics. The vertical dashed lines represent a 90% confidence interval for each of the estimates. The coefficient of the interaction of $S1999$ and the Year 1999 dummy is normalized to zero to identify the model. The solid vertical line separates the pre- and post-shock coefficients. The probability weights provided by PCBS are used in the regression.

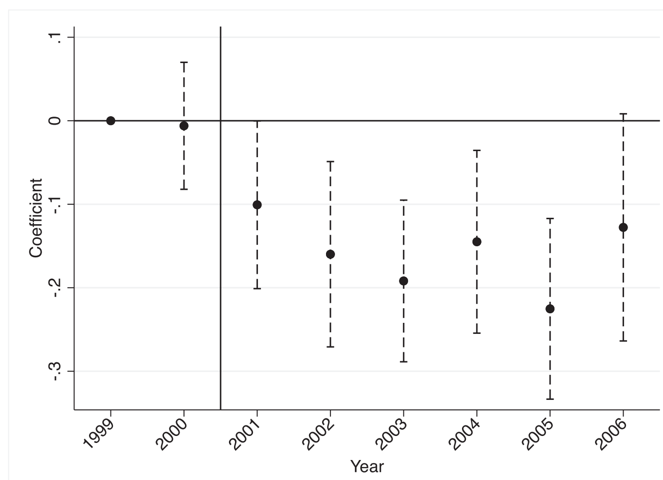


Fig. 6. The closure's impact on the high school dropout rate among youth. **Notes**: The graph plots the coefficients from the generalized DD regression of the school dropout dummy on a set of year indicator variables interacted with the share of commuters in 1999. The regression controls for locality and district-year dummies, locality baseline characteristics in 1999 interacted with the corresponding year's dummy, and individual characteristics. The vertical dashed lines represent a 90% confidence interval for each of the estimates. The coefficient of the interaction of $S1999$ and the Year 1999 dummy is normalized to zero to identify the model. The solid vertical line separates the pre- and post-shock coefficients. The probability weights provided by PCBS are used in the regression.

[17] As (Mansour, 2010) shows, at the outset of the Second Intifada in October 2000, most commuters perceived the Israeli labor market's initial closure as temporary. However, after the second quarter of 2001, most former commuters stopped reporting Israel as their place of work. Eventually, by mid-2001, the share of commuters (employed and unemployed) in the labor force started to decline.

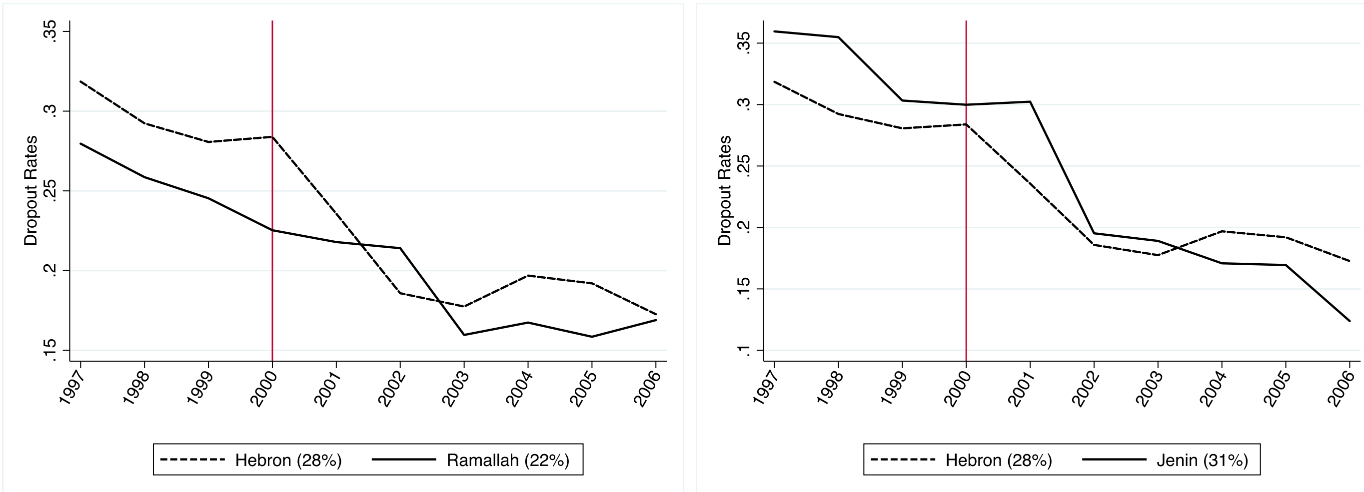


Fig. 7. The evolution of high school dropout rates: Districts of the WB. **Notes**:The percentages represent the share of commuters for the correspondent district.

remaining both negative and statistically significant over the treatment years.

To further support the analysis of the pre-existing trends (see Fig. 7), we trace the dropout rates for different districts in the WB for the period 1997–2006 (district level data are available for before 1999). It is evident that the dropout rates were trending down before the shock for all districts; however, the evolution of trends started to diverge after the shock. For example, the dropout rates for Hebron and Ramallah were almost parallel prior to the shock, but after the shock, the district with the higher commuting share, Hebron, experienced a steeper decline in high school dropouts. Fig. A.3 in the Online Appendix uses Jerusalem, a district that has an identical educational system but was not subject to the closure, as a counterfactual to the WB. The trajectories of high school dropouts in the WB and Jerusalem between 1997 and 2000 were relatively similar, but they began to diverge after 2001, with the WB experiencing a higher decline in the school dropout rate.

4.1. Triple difference

Prior to the shock, the vast majority of commuters were male (more than 98%). Therefore, the shock is supposed to have a differential impact on gender within localities. To exploit this third dimension (asymmetrical shock across localities and gender over time), we estimate the DDD by comparing the probability of becoming a high school dropout across time, gender, and locality.

The DDD rules out the concern that the unobservable locality time-varying variables might confound the differential impact of the shock across localities in the DD model. In addition, by comparing the gender gap in education within localities over time, we further restrict the way differences in pre-shock locality characteristics might bias our estimates, thus strengthening our identification strategy. Overall, the heterogeneous effect analysis is important evidence that the estimated response of educational choices to the closure is mainly driven by shifts in labor market outcomes and not by Second-Intifada-induced shocks, which exert common effects on both genders (For further discussion on this point, refer to Section 5). The DDD specification is given as follows:

$$\begin{aligned} Y_{iglt} &= \alpha + \beta(S1999_l \times Post \times gender_{iglt}) + \boldsymbol{\theta_2'} \boldsymbol{W}_{iglt} + gender_{iglt} \times T_t \\ &+ gender_{iglt} \times \lambda_l + \lambda_{lt} + \epsilon_{ilt}, \end{aligned} \tag{3}$$

where $g \in \{male, female\}$ denotes a gender dummy and λ_{lt} denotes the locality-by-year fixed effect; all remaining variables are as defined in Eq. (1). Our estimation presents a full DDD specification, in the sense that we include all possible interaction terms with respect to gender, locality, and year dummies. The coefficient of the triple interaction term (gender-Share1999-Post) compares the gap in the male vs. female high school dropout rates within the treated localities both before and after the shock, netting out changes in the high school dropout gap in the control localities.

4.2. Threats to identification

Our empirical design and identification rely heavily on the assumption of limited internal migration. This concern is not unique to the current study; in fact, it is common to almost all the studies that investigate the differential impact of the Second Intifada on a locality (district) post intifada (Amodio and Di Maio, 2018; Di Maio and Nistico, 2019; Mansour, 2010; Miaari et al., 2014; Zimring, 2019). Using the migration data, Zimring (2019) show that more than 95% of the WB's population live in the same district in which they were born. Additionally, around 7% of internal migrants in the WB migrated for work-related reasons. Abrahams (2018) reports a low incidence of internal migration in the WB, using the 2007 census data. (Mansour, 2010; Miaari et al., 2014) demonstrate that commuting across the WB's cities was limited during the Second Intifada, and hence the authors argue that the effect of returning commuters was observed locally.

To further investigate limited migration in our sample, we follow the approach in Kuka et al. (2018) to show that the small differences of baseline individual and household characteristics across localities are stable over time, and that these observable variables cannot account for the post-shock outcome's divergent path between localities with high and low commuting shares. Fig. 8 shows that our treatment variable (the commuting share in 1999) cannot explain the predicted dropout rate, which is based on observable variables.[18] Overall, there is ubiquitous evidence (supported by the above-mentioned papers and the lack of a compositional effect in our sample) of limited migration and commuting within the WB during the investigated period due to severe mobility restrictions that were imposed by Israel during the Second Intifada.

A concern regarding our identification is whether the strength of commuting restrictions to Israel at the outset of the Intifada is correlated with localities' pre-Intifada share of commuters. We argue that this is not the case. Weeks after the outbreak of the Second Intifada, Israel indiscriminately and almost entirely closed its labor market to all Pales-

[18] First, we fit a logistic regression model for youth school dropouts using individual observable variables in the years before the shock. We then use the estimated coefficients to generate predicted school dropouts for the entire period (up to 2006). Finally, we estimate Eq. (2) using the predicted outcomes as the dependent variable.

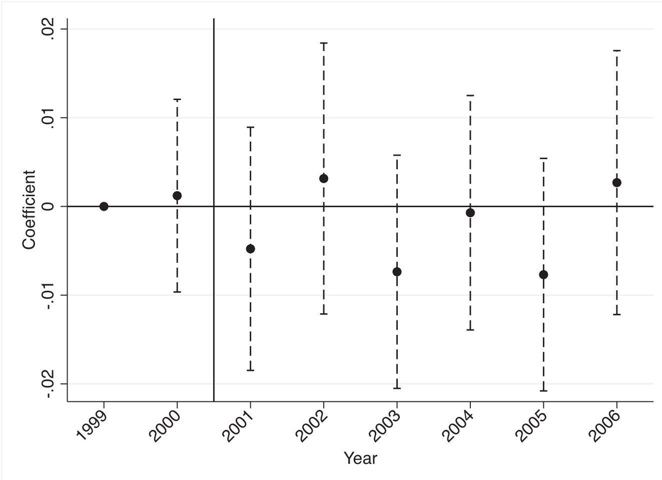


Fig. 8. The closure's impact on the predicted high school dropout rate. **Notes**: The graph plots the coefficients from the generalized DD regression of the predicted school dropout dummy (calculated from a first-stage logistic regression, with the dummy equaling one if the predicted probability is higher than 0.14 (mean dropout ratio for the entire period) on a set of year indicator variables interacted with the share of commuters in 1999. The regression controls for locality and district-year dummies, locality baseline characteristics in 1999 interacted with the corresponding year's dummy, and individual characteristics. The vertical dashed lines represent a 90% confidence interval for each of the estimates. The coefficient of the interaction of $S1999$ and the Year 1999 dummy is normalized to zero to identify the model. The solid vertical line separates the pre- and post-shock coefficients. The probability weights provided by PCBS are used in the regression.

tinian commuters, regardless of their geographical location, personal characteristics, and security clearance status. A few months later, Israel began to implement long-term universal measures, such as work permit suspensions and the construction of the Separation Wall, to further limit Palestinian workers' access; in addition, Israel gradually substituted foreign workers for Palestinian workers.

In relation to the previous point, one might be concerned that schooling decisions could be affected not only by the initial shock in 2001 but also by variation in the closure policy (i.e., closure days, work permits, and enforcement) during the Intifada years. First, changes in the closure policy during the conflict period are not locality-specific, and they are highly unlikely to be correlated with the pre-Intifada commuting share. Second, we believe that the long-term policies that Israel adopted at the beginning of the Second Intifada have fundamentally changed Palestinians' expectations with regard to regaining access to the Israeli market (at least in the medium-term). Third, in the DDD model, the locality-by-year fixed effect will effectively control for the evolution of commuting at the locality level. In the DD model, variations in closure policy will be partially controlled for by the locality linear trend and the district-by-year fixed effect. Finally, we show that the DD model is robust to directly controlling for changes in locality commuting share over time and many other locality time-varying variables, including unemployment and a measure of violence.

The DDD model provides an important piece of evidence that conflict intensity, and hence local demand for labor, might not confound our estimate because locality-by-year fixed effects are controlled for, and there is no reason to believe that violence has differential effects on schooling across gender. For example, Brück et al. (2019) find that conflict intensity had no differential effect on male compared with female students in terms of the likelihood of passing the high school exam. To further rule out the concern of a conflict effect, we re-estimate the DD model using only low conflict intensity localities. The results are strongly stable (almost identical). Finally, we show that our results are robust to directly controlling for changes in conflict intensity within localities over time.

An important time-varying omitted variable at the locality level that we do not control for is the quality of education (e.g., the number of schools and teachers, etc.). We argue that the allocation of education-related resources is very centralized and stable across the WB's localities. However, we acknowledge that schooling quality might have varied with conflict intensity at the locality level. This will weaken our identification in the DDD model only if schooling quality has a differential effect on gender, which is highly unlikely. In the DD model, if the quality of education only varied because of conflict intensity, controlling for violence or limiting the sample to low conflict intensity localities helps alleviate the concern about schooling quality. As an additional check, we show that the results are robust to using the geographical distance between localities and the nearest crossing point to Israel as an instrumental variable (IV) to commuting share in the IV- DD model. We acknowledge that distance might not be a perfect instrument; nonetheless, it lends more credibility to our model.[19]

4.3. Results

Estimates on the commuting restriction's effect on the probability of becoming a school dropout are presented in Columns (1)–(5) of Table 3. The DD estimates across all specifications are highly significant and fairly stable. Column 4 (our preferred model, as in Eq. (1)) shows that a one standard deviation increase in commuting share in 1999 lowers the dropout probability by 3.2 percentage points (p.p.; about 16% of the mean probability of becoming a youth dropout for the period 1999–2006, 0.20). In Column (5), we control for the time-varying locality variables, namely commuting share, unemployment, total population, and the number of Palestinian fatalities related to the Second Intifada (as a measure of conflict intensity). The effect remains large and highly significant.

Table 4 separately reports the results for male and female samples both with and without the locality linear trend, as in Columns (3) and (4) of Table 3. The commuting restriction's effect is only negative, sizable, and highly significant for males. The estimate suggests that our results for the whole sample are mainly driven by males. An increase in commuting shares by one standard deviation in 1999 lowers the dropout probability for males by 4.5 p.p. (19% relative to the average male dropout rate, 0.24).

The coefficient of the triple interaction term (gender-Share1999-Post) is negative and highly statistically significant (p-value= 0.028), lending more credibility to our identification and the heterogeneous effect analysis (in Column 5 of Table 4). Compared to the localities in the 25th percentile of the commuting share distribution, the gap in the male vs. female high school dropout rates is narrowed by 4.5 p.p. more in localities in the 75th percentile of the distribution (around 50% relative to the mean dropout gender gap for the period 1999–2006, 0.09).

Discussion of the results: At the mean level of commuting shock at locality level (0.25), the overall high school dropout rate declined by 4.5 p.p. (22.5%), and the dropout rate decreased by 4.5 p.p. more for males than for females (with the education gap narrowing by around 50%). To compare our estimates with the literature, we calculate the percentage change in schooling enrollment for the mean locality for males, using the DD coefficient in Column (2) of Table 4, 0.25. Here, to facilitate the comparison, we consider a one p.p. decline in the school dropout rate as a one p.p. increase in school enrollment. At the mean commuting share

[19] It must be noted that the variation in commuting share before 2001 is mostly explained by locality-specific variables that do not change over time, namely the proximity to Israel and, to a lesser extent, the network effect (Miaari et al., 2014). Distance might be picking up the effect of urbanization, and therefore, the quality of education. However, adding dummies for urban and rural areas in the IV- DD model could minimize this concern.

Table 3
The effect of commuting restrictions on the school dropout rate among youth aged 15–19 years.

Dep. Variable: School dropout dummy for youth aged 15–19	(1)	(2)	(3)	(4)	(5)
Share of commuters 1999*Post	-0.24***	-0.21***	-0.16***	-0.18***	-0.15***
	(0.045)	(0.052)	(0.053)	(0.068)	(0.056)
Baseline Var.Post	NO	YES	YES	YES	YES
Ind.controls	NO	NO	YES	YES	YES
Locality Time varying Var	NO	NO	NO	NO	YES
Linear trend	NO	NO	NO	YES	NO
No. obs	34,177	34,176	31,401	31,401	31,401

The table reports DD estimates of the closure's effect, measured by Share of commuters 1999*Post, on high school dropouts among Palestinian youth. All specifications have district-year fixed effects and locality fixed effects. Baseline locality-specific variables include the unemployment rate, the share of the population under the age of 16, the share of waged workers in total employment, and rural and urban dummies (the reference group is camps). Locality time-varying variables include the number of Palestinian fatalities, the commuting share, unemployment, and the total population over 9 years old. *Post* is a dummy that equals one for observations in the 2001–2006 year range and zero for 1999 and 2000. Individual controls are both parents' years of education (household heads), the age dummy, the number of household members, and the number of household members under 15 years of age. The probability weights provided by PCBS are used in all the study's regressions. *p-value < 0.1, **p-value < 0.05, and ***p-value < 0.01. Standard errors in parentheses are clustered at the locality level.

Table 4
The effect of commuting restrictions on the school dropout rate among youth aged 15–19 years by gender.

Dep. Variable: School dropout dummy for youth aged 15–19	Males (1)	Males (2)	Females (3)	Females (4)	Triple diff (5)
Share of commuters 1999*Post	-0.22***	-0.25***	-0.10	-0.10	
	(0.070)	(0.091)	(0.060)	(0.078)	
Gender*Sh1999*Post					-0.18**
					(0.082)
Baseline Var.Post	YES	YES	YES	YES	
Ind.controls	YES	YES	YES	YES	YES
Linear trend	NO	YES	NO	YES	
Locality-year F.E.					YES
Gender-year F.E.					YES
Gender-locality F.E.					YES
No. obs	16,549	16,549	14,852	14,852	31,402

The table reports DD and DDD estimates of the closure's effect on the school dropout rate by gender. All specifications have district-year fixed effects and locality fixed effects. The baseline localities-specific variables, individual controls, and *Post* are defined in Table 3. *p-value < 0.1, **p-value < 0.05, and ***p-value < 0.01. Standard errors in parentheses are clustered at the locality level.

level in a locality, the high school enrollment is 6.2 p.p. higher, representing an 8.2% gain in average high school enrollment about 0.75. Our estimate is in the range of the estimates of a size-comparable Malawi migration shock that was reported by Dinkelman and Mariotti (2016) (who show a 7–18% increase in schooling at the mean level of migration shock at the district level). However, given Malawi's low average schooling enrollment in 1998 (the share of the adult population with any primary schooling is 0.41), one would expect a smaller percentage gain in education in our setting. To give more accurate estimates, we scale the mean shock at the locality level according to the decline in the probability of commuting to Israel before and after the shock (given that the closure was not complete and the numbers of commuters did not fall to zero). In Section 5, we calculate that the probability of commuting falls by 13.75 p.p. for localities with the mean commuting share. The rescaled estimate of 4.6% is slightly below the lower bound of the estimates in Dinkelman and Mariotti (2016). In the Philippines, Theoharides (2018) finds a 3.5% increase in secondary school enrollment in response to an average increase in migration shock, placing our estimate within the range of the estimates that are provided in the literature.

Since the magnitude of the average shock differs across studies, we use the elasticity of education with respect to migration shock, measured as the percentage change in enrollment divided by the percentage change in migration in the mean locality. The estimates of the elasticity of education with respect to average migration in Dinkelman and Mariotti (2016) and Theoharides (2018) range from 0.07 to 0.17. In our setting, the elasticity of school enrollment to mean commuting shock lies within the range of their estimates at 0.085.[20]

In addition to the related migration literature, we are interested in comparing our results with those that are emerging from papers investi-

[20] The elasticity of education to mean commuting came from the point estimate (0.25), the mean high school enrollment (0.75), the decline in the probability of commuting at the locality level (13.75 p.p.), and the commuting share in the mean locality: $(0.25 \times 00.1375/0.75)/(13.75/25)$. The estimate range of 0.07 0.17 is reported in Dinkelman and Mariotti (2016).

Table 5
The effect of commuting restrictions on the school dropout rate among youth aged 15–19 years: Conflict & the early vs. the late effect.

Dep. Variable: School dropout dummy for youth aged 15–19	Low Conflict Intensity			Early vs. Late Effect	
	(1)	(2)	(3)	(4)	(5)
Share of commuters 1999*Post	-0.16*** (0.053)	-0.18*** (0.067)		-0.15*** (0.051)	-0.16*** (0.051)
Gender*Sh1999*Post			-0.19** (0.093)		
Share of commuters1999*Post2				-0.02 (0.029)	0.00 (0.032)
Baseline Var.Post	YES	YES	–	YES	YES
Ind.controls	YES	YES	YES	YES	YES
Linear trend	NO	YES	–	NO	NO
Locality-year F.E.	–	–	YES	–	–
Gender-year F.E.	–	–	YES	–	–
Gender-locality F.E.	–	–	YES	–	–
No. obs	28,247	28,247	28,248	31,401	28,247

The table reports the DD and DDD estimates of the closures effects on the incidence of school dropouts. All specifications have district-year fixed effects and locality fixed effects. Baseline localities-specific variables, individual controls, and *Post* are defined in Table 3. *Post*2 equals one if year > 2003 and is zero otherwise. In Columns (1)-(3) and Column (5), we drop high conflict intensity localities (the top 10 percent). *p-value < 0.1, **p-value < 0.05, and ***p-value < 0.01. Standard errors in parentheses are clustered at the locality level.

gating the impact of (un)skilled-biased shock on educational attainment. The estimates of the elasticity of schooling with respect to the skill wage premium in the US range from 0.47 to 0.72 (Black et al., 2005; Cascio and Narayan, 2015). In Section 5, we show that the skill wage premium, conditional on being employed in the WB, increases by 3.8% for a 10 p.p. pre-shock increase in the commuting share at the locality level. The elasticity of education with respect to the skill wage premium is then 0.43.[21] It is worth mentioning that our estimates of education's wage premium elasticity are only suggestive because the closure affects education mainly through dropouts' employment prospects (see the next section). To put our estimates in perspective, we calculate the elasticity of high school-level education with respect to employment opportunity. In the next section, we show that a 10 p.p. increase in commuting share leads to a 6.2 p.p. increase in the probability of high school dropouts being unemployed. This puts the elasticity estimate at 0.53 (0.25/0.62 × 1/0.75). Our analysis captures the net effect of the shock at the locality level, including the counteracting mechanism of declining household income; therefore, the estimate of elasticity represents the lower bound of the effect of job opportunities for dropouts on high school enrollment.

To test whether our results were affected by violence, in Table 5 Columns (1)-(3), we drop localities with a high conflict intensity (i.e., the top 10 percent), measured by the total number of Intifada-related fatalities for the period 2000–2006 per 100 individuals and re-estimate Columns (3) & and (4) in Table 3 as well as the triple coefficient in Eq. (3). Again, the results are almost unchanged.

Columns (4) and (5) in Table 5 disentangle the Intifada's early effects from its late effects. As argued above, by mid-2001, Palestinians perceived the closure as a long-lived (permanent) shock; therefore, we expect the closure's effect on school dropouts to be mainly present in the first two to three years following the Intifada. To test for the closure's early vs. late effect, we multiply the commuting share $S1999$ by the indicator variable *Post*2, which equals one if the year > 2003 and zero otherwise. We re-estimate Eq. (1) with the inclusion of this new interaction term, which captures the closure's late effect on schooling choices. The coefficient of the new interaction term is near zero with and without the high conflict intensity localities.

In addition to gender, we investigate whether the closure's effect is heterogenous across two more dimensions, namely parents' education and age. We re-estimate Eq. (1) in Columns (1) and (2) of Table 6 separately for males who belong to an educated household (in which at least one of the parents completed high school) and for those who belong to an uneducated household. The closure has a high, precise effect on school dropouts among students from uneducated households; however, this is not so for those from educated households. Column (3) in the same table presents a modified DDD model that compares the school dropout gap across youth who come from educated and uneducated households within the localities over time. The coefficient of the triple interaction term is negative and significant, confirming the results shown in Columns (1) and (2). Columns (4)-(6) in Table 6 estimate the closure's effects on younger students (aged 15–16) and older students (aged 17–19). The effects are large and highly significant for older students, but not for younger ones. The DDD analysis confirms that the gap in dropouts between old and young students declined more in the treated localities. The heterogeneous analysis is an important piece of evidence that shows that the closure's effect on schooling works through labor market changes, with older students (aged 17–19) being more susceptible to labor market changes than young students (aged 15–16). Students who come from educated households will most likely complete high school, regardless of employment prospects in Israel; hence, they are less affected by labor market conditions.

4.4. Robustness checks

In this subsection, we report on the robustness checks of our findings with regard to several issues. We re-estimate Eqs. (1) and (3) using two alternative measures of dropouts (described in the Data Section). The estimates are almost unchanged, but they are more precise, despite having a smaller sample size (see the Online Appendix, Table A.3). Additionally, as shown in Table A.4 in the Online Appendix, for all measures of high school dropouts, the results are robust to using a balanced dataset, dropping localities that do not show up in all years for the period 1999–2006.

[21] The elasticity was obtained using the point estimate of $\beta = 0.25$, the shock's effect on the wage premium (.38), average high school enrollment (0.75), and the average probability of being wage-employed in the WB (about 0.5): 0.25/.38 × 0.5/0.75.

Table 6
The effect of commuting restrictions on the school dropout rate among males: Heterogeneous effects.

	By Parents education			By Age group		
	No highschool (1)	Yes Highschool (2)	Triple (3)	15–16 (4)	17–19 (5)	Triple (6)
Share of commuters 1999*Post	-0.33*** (0.106)	0.07 (0.192)		-0.08 (0.080)	-0.37** (0.152)	
Parents Edu.*Sh1999*Post			-0.30** (0.138)			
Age group*Sh1999*Post						-0.18* (0.094)
Baseline Var.Post	YES	YES	–	YES	YES	–
Ind.controls	YES	YES	YES	YES	YES	YES
Linear trend	YES	YES	–	YES	YES	–
Locality F.E.	–	–	YES	–	–	YES
Interaction terms	–	–	YES	–	–	YES
No. obs	11,192	5355	16,547	7167	9381	16,548

All specifications have district-year fixed effects and locality fixed effects. Columns (1) and (2) report the DD estimator of the closure's effect on the school dropout rate among students who come from uneducated and educated households, respectively. Column (3) reports the DDD estimator of the closure's effect by parents' education (Parents' Edu. equals one for an uneducated household). Columns (4) and (5) split the sample by age and show the DD estimator. Column (6) reports the DDD estimator of the closure effect by age group (age group equals 1 for 17 19-year-olds). Baseline localities-specific variables, individual controls, and *Post* are defined in Table 3. *p-value < 0.1, **p-value < 0.05, and ***p-value < 0.01. Standard errors in parentheses are clustered at the locality level.

To deal with the potential omitted variables issue in the DD model, we implement an instrumental variable DD model (IV DD). The most obvious IV to select is the locality's geographical proximity from Israel. As discussed above, the variation in commuting shares seems to be strongly associated with the distance from Israel. We use the inverse of log distance between localities and the closest entry point to Israel as IVs for the commuting share. We estimate the model using a two-stage least squares (2SLS) technique.

The IV–DD estimates are reported in Table A.5, Column (1). The closure's impact on the high school dropout rate is negative and significant. Column (2) reports the reduced form regression. The impact of the inverse of the distance is negative and significant. Once the commuting share is controlled for, in Column (3), the coefficient of distance loses its significance, suggesting that distance indirectly impacts the school dropout rate via the commuting shares.

We test whether our results are robust to including the Gaza Strip. Gaza-based workers' access to Israel was almost completely eliminated after the Second Intifada, with the commuting share nearing zero in 2005 (see Fig. A.4 in the Online Appendix). We treat the Gaza Strip as a unique labor market (locality) and re-estimate Eq. (1). The results are strongly robust and precise (see Columns 1 and 2 in Table A.6 in the Online Appendix).

We undertake a placebo test that targets the educational choices of cohorts that are presumably unaffected by the shock—that is, those in an older age category (22–29 years old)[22] or a younger cohort (10–14 years old). In the old cohorts, schooling decisions were undertaken before the shock, and thus we do not expect to see a divergent path for dropout rates across localities, if the shock is what really caused the divergent path. While the shock might have incentivized parents to send their children to school, we expect the effect to be minimal, if any, especially given the low cost of attending school at a younger age; most parents send their young children to school, regardless of the prospect of working in Israel. The results for younger and older cohorts are statistically insignificant and very close to zero (see Table A.7 in the Online Appendix). These findings partially ensure that the internal migration and/or the quality of available data might not be driving the closure's estimated negative impact on educational choices among youth.

[22] A person in this category is considered a school dropout if he/she has not completed 12 years of schooling.

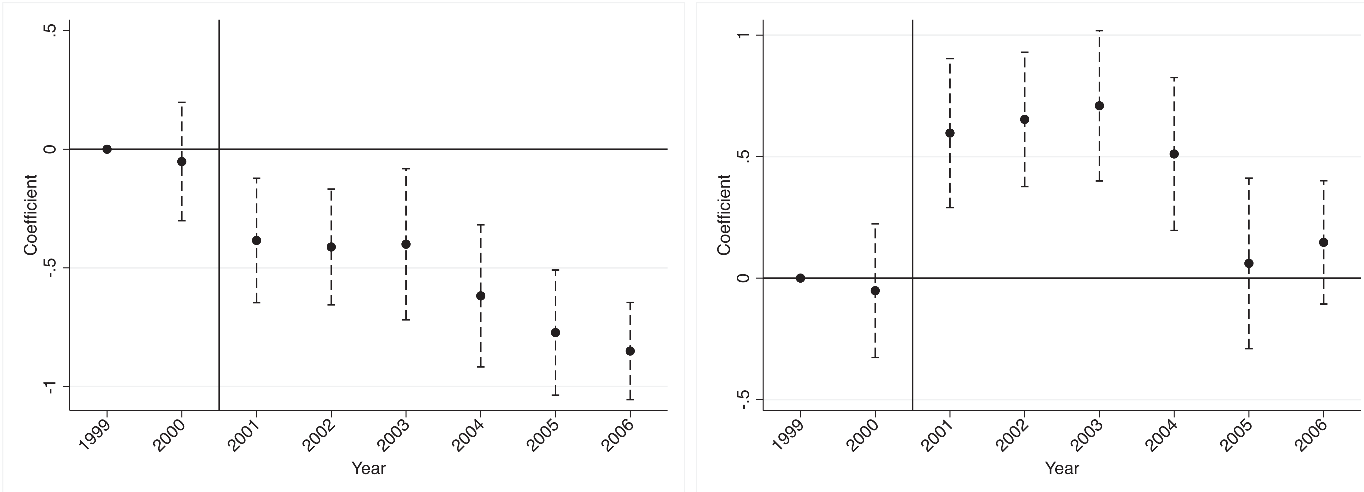
In addition, we implement the Fisher Randomization Test to verify the p-values estimates. We randomly reshuffle the commuting shares in 1999 across localities and re-estimate the DD model corresponding to Column 1 in Table 3. We repeat this process 2000 times and plot the probability density function of the coefficients of the placebo β in Fig. A.5 in the Online Appendix. The distribution of the placebo coefficients is centered around zero, and the true estimate is located far to the left of this distribution (none of the 2000 placebo β reaches -0.24); this indicates that, by using the non-parametric distribution of placebo β, we can strongly reject the hypothesis that the coefficient, β, in Eq. (1) is zero.

5. Potential mechanisms

We argue that the closure has impacted labor market outcomes in the WB and hence youth incentives for schooling. There have been two important shifts in the labor market that might change the opportunity cost of attending school. First, for youth who are about to enter the labor market, the probability of being employed in Israel has declined substantially in the wake of the shock (we call this mechanism *expected commuting*). As a result, the payoff of leaving high school and entering the labor market declined after the shock due to the lower likelihood of acquiring a well-paid job with relatively low skill requirements. Second, potential dropouts are expected to face tougher labor market competitions as a result of the positive unskilled labor supply shock (return commuters), thus leading to lower post-closure employment prospects and wages (*employment prospect*).

For *expected commuting* and *employment prospect* to be confirmed as the true underlying mechanisms linking the closure to schooling choices at the locality level, we have to observe the differential impact of the closure across localities; that is, the decline in the probability of commuting and finding a job for school dropouts will be higher in hard-hit localities than in less-exposed localities. Whether both mechanisms are at work depends on the degree of integration between local labor markets in the WB. To illustrate this point and the validity of our empirical design, we discuss the plausibility of the underlying mechanisms under two extreme assumptions about the WB's local labor markets.

Case (1): Isolated local labor markets. In this scenario, there is no spillover effect across localities, and the shock will be fully absorbed at the local level. In other words, return commuters will reside and seek jobs in their localities of origin. To simplify the discussion, suppose that



(a) Employed in Israel

(b) Unemployment

Fig. 9. Mechanisms. **Notes**: Graph (a) reports the coefficients from the generalized DD regression of the employment in Israel dummy on a set of year indicator variables interacted with the share of commuters in 1999 for youth males. In Graph (b), the outcome variable is the unemployment dummy for male youth dropout. The regressions control for locality and district-year dummies, locality baseline characteristics in 1999 interacted with the corresponding year's dummy, and individual characteristics. The vertical dashed lines represent a 90% confidence interval for each of the estimates. The coefficient of the interaction of $S1999$ and the Year 1999 dummy is normalized to zero to identify the model. The solid vertical line separates the pre- and post-shock coefficients. The probability weights provided by PCBS are used in the regression.

(1) the number of workers in Israel dropped to zero after the Second Intifada, and (2) there are two localities in the WB: the treated locality, H (with a high pre-shock commuting share) and the control locality, L (with a low pre-shock commuting share). It is evident that after the closure, the probabilities of commuting and job prospects decline more in H than in L.[23]

Case (2): Free labor mobility. In this scenario, the shock will be absorbed at the WB level. Specifically, free labor mobility across localities will equalize wages and unemployment rates across the WB. Assuming similar pre-shock local labor markets,[24] the closure's impact on wages and unemployment will be symmetrical for localities H and L, i.e., the *employment prospect* mechanism is not operating. Nonetheless, the *expected commuting* mechanism still effective as the drop in the probability of commuting is higher in H than it is in L, leading to differential educational outcomes across localities.

This simple example illustrates an interesting and important result, as follows: the closure of the Israeli labor market is expected to have a differential impact on the opportunity cost of schooling across localities with different previous commuting shares, regardless of the post-closure assumption about labor mobility within the WB (albeit with different mechanisms and magnitudes). In reality, internal mobility was severely restricted after the Second Intifada, and yet, we suspect some spillover resulting from trade and limited labor mobility.

We test the validity of our empirical design and *expected commuting* by showing that the initial pre-shock share of commuters is driving the post-shock decline in the probability of being employed in Israel. We re-estimate Eq. (1) with Y_{ilst} taking on the value of one if individual i is employed in Israel and zero otherwise.

[23] The pre-shock probability of commuting is well approximated by the share of commuters in the locality $i = H, L$ with $H > L$. Given that there is zero probability of post-shock commuting, it is obvious that $H - 0 > L - 0$. Furthermore, given that more unskilled workers are competing for a fixed number of jobs in H compared with in L, the unemployment rate in H will be higher, and wages will be lower for high school dropouts.

[24] Localities were relatively similar before the shock in terms of labor market outcomes, and mobility within the WB was not restricted.

Table 7
The effect of commuting restrictions on the probability of being employed in Israel.

Dep.Variable Commuting to Israel dummy				
	(1)	(2)	(3)	(4)
Share of commuters 1999*Post	-0.57***	-0.55***	-0.29**	-0.36***
	(0.064)	(0.083)	(0.137)	(0.076)
Baseline Var.Post	YES	YES	YES	YES
Ind.controls	NO	YES	YES	YES
Linear trend	NO	NO	YES	YES
No. obs	4569	4569	4569	49,963

The first three columns report the DD estimates of the closure's effect on the probability of being employed in Israel for employed males between 15 and 19 years old, and Column 4 reports the DD estimate for all employed males. All specifications have district-year fixed effects and locality fixed effects. Baseline localities-specific variables and *Post* are defined in Table 3. We control for individuals' ages and years of education. The probability weights provided by the PCBS are used in all regressions. *p-value < 0.1, **p-value < 0.05, and ***p-value < 0.01. Standard errors in parentheses are clustered at the locality level.

The results in Table 7, Column (2), show that the estimated effect is large and very precise. The probability of being employed in Israel declined for male youth workers by 14 p.p. more (about 90% relative to the mean commuting share between 1999 and 2006) in localities in the 75th percentile of commuting share in 1999 (0.4) compared with localities in the 25th percentile (0.15). This is an important piece of evidence that demonstrates that the strength of the commuting restriction is orthogonal to the pre-shock share of commuters (common mobility measures for all localities), and exposure to the shock is strongly determined by the share of commuters before the Second Intifada. Clearly, as shown in Graph (a) of Fig. 9, the change in the probability of being employed in Israel before the Second Intifada was not significantly different across localities that had different shares of commuters, and the effect of the commuting restriction is persistent in the years following the Second Intifada.

Table 8
Causal mechanisms: Employment prospects and the skill wage premium.

Dep.Var.	Youth Dropout		Males Age 22–30		
	Unemployed Dummy			Log Wage	
				employed	employed locally
	(1)	(2)	(3)	(4)	(5)
Share of commuters 1999*Post	0.62*** (0.133)				
Gender*Sh1999*Post		0.32*** (0.117)			
Dropout*Sh1999*Post			0.21** (0.101)	-0.11 (0.159)	-0.36** (0.146)
Ind.controls	YES	YES	YES	YES	YES
Baseline Var.Post	YES	–	–	–	–
Linear trend	YES	–	–	–	–
Locality-year F.E.	–	YES	YES	YES	YES
Gender-year F.E.	–	YES	–	–	–
Gender-locality F.E.	–	YES	–	–	–
Dropout-year F.E.	–	–	YES	YES	YES
Dropout-locality F.E.	–	–	YES	YES	YES
No. obs	6123	5990	22,557	7185	6396

Column (1) reports the DD estimate of the closure's effect on the probability of being unemployed among youth dropout males. Column (2) reports the DDD estimate of the closure's differential impact on the probability of being unemployed across genders for youth dropouts within the localities. Column (3) reports the DDD estimate of the closure's differential impact on the probability of being unemployed for unskilled and skilled male workers aged 22- 30. Columns (4) and (5) are similar to Column (3), with the outcome variable being log daily wages. Baseline localities-specific variables include the unemployment rate, the share of the population under 16 years of age, the share of waged workers in total employment, and rural and urban dummies (the reference group is camps). *Post* is a dummy that equals one for observations that fall within the 2001–2006 year range and zero for 1999 and 2000. Individual controls are the household heads' years of education, years of education (degree dummies in Columns 3–5), age dummies, the total number of households, and the number of household members that are under the age of 15. The probability weights provided by PCBS are used in all regressions. *p-value < 0.1, **p-value < 0.05, and ***p-value < 0.01. Standard errors in parentheses are clustered at the locality level.

5.1. *Employment prospects for youth dropouts*

The closure reduced the opportunity cost of attending school by decreasing the value of being in the labor market; that is, for high school dropouts, the probability of finding a job declined post-shock more so for highly-exposed localities. To test this, we estimate the DD model, as in Eq. (1), modifying the outcome, Y_{ilst}, to take a value of one if a high school dropout is unemployed or out of the labor force, and zero otherwise. The DD coefficient captures the difference in the change of the probability of being unemployed before and after the shock between localities with high and low commuting shares. Column (1) of Table 8 confirms our theoretical prediction, with the closure's impact on youth unemployment being positive, large, and very precise. The unemployment probability for youth dropouts had increased by almost 15.5 p.p. more in localities in the 75th percentile of the commuting share distribution when compared to localities in the 25th percentile of the distribution. Graph (b) of Fig. 9 shows that, prior to the shock, the unemployment rates followed a statistically similar path for localities with high and low commuting shares.

If the prospect of employment is the true mechanism linking the closure of the Israeli labor market to youth schooling choices in the WB, then it should mainly affect the male cohort. Column (2) of Table 8 reports the DDD coefficient, comparing the change in the unemployment gap between males and females within localities. The closure's differential impact on unemployment by gender is much larger in treated as opposed to control localities. In the same vein, one would expect a sharper increase in the probability of being unemployed for dropouts compared to skilled males. We estimate a DDD model for males aged 22–30, with the coefficient of the triple interaction term comparing the gap in unemployment between unskilled and skilled male workers within and across localities over time. Again, dropouts in treated localities are faced with a higher post-shock probability of unemployment compared to skilled workers.

Given the positive shock with respect to the unskilled labor supply, returns to schooling, measured as the skill wage premium, might have changed differently across localities in response to the closure, reinforcing the employment prospect mechanism. To test for the skill wage premium, we replace the unemployment dummy with the log daily wage as an outcome and re-estimate the DDD model as in Column (3) of Table 8. Columns (4) and (5) in Table 8 show that the skill wage premium has increased more in treated localities following the shock, but the effect is statistically significant only as conditional on being employed in the WB (Column 5). Our conclusion is that the evidence for the skill wage premium as a driving mechanism is inconclusive.[25]

Along with the heterogeneous effects analysis of the closure in the previous section, the above findings provide strong evidence that our results are driven by changes in labor market conditions, particularly changes in the employment prospects of school dropouts, and might not be explained by other conflated shocks, such as violence, which is expected to have a similar effect on males and females.[26] In addition, the

[25] It is worth mentioning that our analysis in this section is silent on the closure's effect on skilled workers in absolute terms. Mansour (2010) shows that skilled workers were minimally affected by the closure. There are several reasons that this is the case: (1) as we argue in this paper, the shock is asymmetrical across workers with different skills, mainly negatively affecting unskilled workers; (2) the majority of skilled non-commuters work in the public sector, international organizations, non-governmental organizations, and large firms, all of which continued to operate relatively smoothly after the closure; and (3) skilled return commuters might have a better opportunity to find a job in the expanding public sector after the shock.

[26] The unemployment analysis is restricted according to the sample size's limitation and the measurement error, as few youth dropouts are employed, which

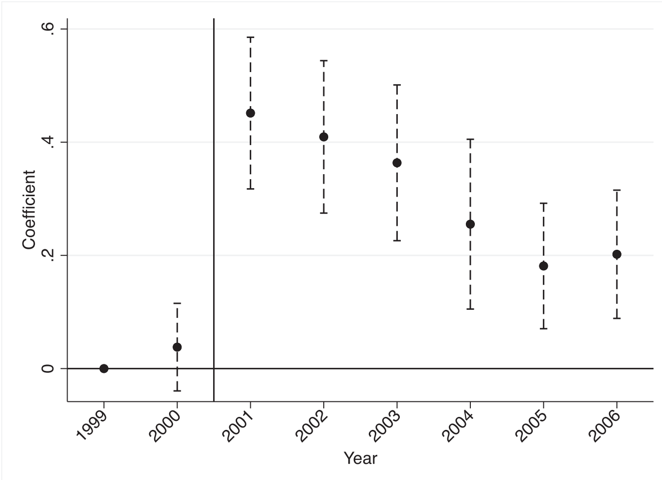


Fig. 10. The closure's impact on unemployment among household heads. **Notes**: The graph plots the coefficients from the generalized DD regression of the unemployment dummy for male household heads on a set of year indicator variables interacted with the share of commuters in 1999. The regression controls for locality and district-year dummies, locality baseline characteristics in 1999 interacted with the corresponding year's dummy, and individual characteristics. The vertical dashed lines represent a 90% confidence interval for each of the estimates. The coefficient of the interaction of $S1999$ and the Year 1999 dummy is normalized to zero to identify the model. The solid vertical line separates the pre- and post-shock coefficients. The probability weights provided by PCBS are used in the regression.

divergent path of unemployment rates and, to a limited degree, wages, support the findings that were reported in the previous literature about the closure's local effect and limited spillover within the WB localities during the Second Intifada.

5.2. Household income and parental job loss

The Second Intifada constituted a severe negative economic shock, including negative economic growth, disrupted trade, violence, and deteriorated labor market conditions. While these conflated shocks are not necessarily correlated with the pre-Intifada locality share of commuters and are considered to be economy-wide shocks, the loss of locality household income might be directly linked to the share of commuters in 1999. A household's average income is expected to decline disproportionately more in hard-hit localities due to a larger loss in wage income for return commuters, a lower locality wage, and a smaller probability of employment. Given the data limitation on household income, we test for this mechanism by estimating the DD for parental job loss. In other words, our dependent variable takes on the value of one if the household head is unemployed and zero otherwise. The event analysis is shown in Fig. 10. The evidence is very clear and precise. After the Second Intifada, the probability of a household head being unemployed had increased more in localities with high commuting shares than in localities with low commuting shares.

Nonetheless, this mechanism works against our findings as parental job loss is expected to increase the dropout probability (Di Maio and Nistico, 2019). While the income effect implies a higher number of school dropouts in response to the shock, the decline in the opportunity cost of attending school caused the dropout rates to fall after the shock. As a result, our estimates present the closure's overall (net) effect on locality-level schooling choices.

might have upwardly biased the results. Nonetheless, there is no reason to believe that the measurement error is correlated with the pre-shock commuting share.

Table 9
The effect of commuting restrictions on college enrollment among students aged 20–23 years.

Dep. Variable: College Enrollment age 20–23	All (1)	Males (2)	Females (3)	Triple diff (4)
Share of commuters 1999*Post	-0.07	-0.16	-0.14	
	(0.117)	(0.150)	(0.245)	
Gender*Sh1999*Post				-0.06
				(0.180)
Baseline Var.Post	YES	YES	YES	
Ind.controls	YES	YES	YES	YES
Linear trend	YES	YES	YES	
Locality-year F.E.				YES
Gender-year F.E.				YES
Gender-locality F.E.				YES
No. obs	8565	4968	3593	8494

The table reports the DD and DDD estimates of the closure's effect on college enrollment for students aged 20–23. Baseline localities-specific variables include the unemployment rate, the share of the population under 16 years of age, the share of waged workers in total employment, and rural and urban dummies (the reference group is camps). *Post* is a dummy that equals one for observations in the year range 2001–2006 and zero for 1999 and 2000. Individual controls are both parents' (household heads) years of education, the age dummy, the number of household members, and the number of household members under 15 years of age. The probability weights provided by PCBS are used in all regressions. *p-value < 0.1, **p-value < 0.05, and ***p-value < 0.01. Standard errors in parentheses are clustered at the locality level.

We argue that the opportunity cost effect dominates the income effect because high school education is relatively cheap (i.e., essentially free) in the WB, and most localities have their own high schools, meaning that not much commuting is required to and from school (Brück et al., 2019; World Bank, 2007b).[27] On the other hand, college education is relatively expensive and requires significant commuting because universities are concentrated in a few large cities. Consequently, for college education, the income effect might dominate the opportunity cost effect. Furthermore, we test whether the closure has an impact on college attendance. For students aged 20–23 years old who have finished high school, the dependent variable in Eq. (1) equals one if the years of education exceed 12; otherwise, the value is zero. The results are reported in Table 9. Apparently, the closure's impact on college attendance is small and insignificant across all DD and DDD specifications.

5.3. The closure's short- vs. long-term effect

Palestinian youth did not gain access to the Israeli labor market after 2006. While Israel slowly started to open its market, unmarried, under-30 Palestinians were denied access. Using the dataset for the period 1999- 2010, we show that the effect of the Second Intifada on the probability of being employed in Israel has not worn off after 2006, and hence the closure continued to affect schooling choices for protracted periods (Online Appendix Fig. A.6).

Nevertheless, local labor markets started to converge slowly into one labor market as mobility restrictions within the WB weakened. The closure's long-term effect is better captured under the assumption of free labor mobility: Case (2). One would expect to see a smaller impact on schooling after 2006 as the employment prospects mechanism becomes weak. While we see a small decline in the closure's impact after 2006, it is still considerably large because the expected probability of youth being employed in Israel declined more after 2006 due to the strict permit policy that totally blocked Palestinian youth from working in Israel (see Fig. A.6). The household income mechanism is another possible explana-

[27] Brück et al. (2019) reported that more than 86% of the WB's population has access to at least one high school in their locality of residence.

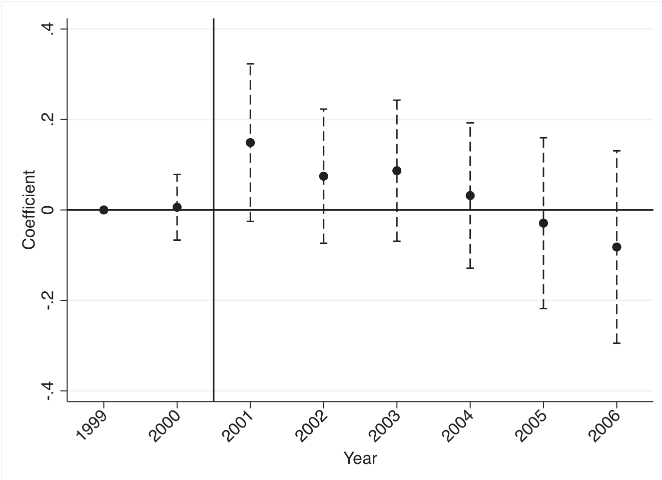


Fig. 11. The closure's impact on public sector employment **Notes**: The graph plots the coefficients from the generalized DD regression of the public employment dummy for employed men aged 14- 24 years old on a set of year indicator variables interacted with the share of commuters in 1999. The regression controls for locality and district-year dummies, locality baseline characteristics in 1999 interacted with the corresponding year's dummy, and individual characteristics. The vertical dashed lines represent a 90% confidence interval for each of the estimates. The coefficient of the interaction of $S1999$ and the Year 1999 dummy is normalized to zero to identify the model. The solid vertical line separates the pre- and post-shock coefficients. The probability weights provided by PCBS are used in the regression.

tion for the closure's large, persistent differential effect across localities despite a convergence in local labor markets for youth. As shown in Fig. A.7, as local labor markets started to converge after 2006, the closure's differential impact on parental job loss has diminished. This reinforces the closure's differential effect as stemming from a declining expectation with regard to the probability of commuting and the forgone wage effect.

5.4. Other potential mechanisms

Public sector employment: Employment in the public sector has increased substantially in the years after the Second Intifada. It is possible that the Palestinian Authority expanded public employment more in localities with high commuting shares to mitigate deteriorated labor market conditions. The increased demand for public sector jobs, which in many instances demand a minimum level of education, might serve as a potential driving mechanism for our results by disproportionately raising returns to education in hard-hit localities. Fig. 11 plots the coefficient of the interaction terms of the event analysis, as in Eq. (2). The dependent variable takes on the value of one if employed men aged 18-24 are employed in the public sector and zero otherwise. The closure had a statistically similar impact on change in the probability of being employed in the public sector in different localities; hence, this mechanism is unlikely to be driving our results.[28]

Violence: One concern regarding our estimation and identification is that commuting restrictions may conflate the effect of violence. While violence might affect academic performance for different reasons (Brück et al., 2019), there is negligible evidence that the intensity of violence is correlated with the share of commuters before the Second Intifada for protracted periods.[29] Additionally, an important mechanism under which violence might impact educational attainment is by disrupting economic activities and labor markets. Cali and Miaari (2018) show that violence, measured by the number of fatalities, had a negligible impact on locality labor markets. This can be attributed to the sporadic nature of violence, especially after 2002.

Brück et al. (2019) show that violence decreases academic performance but not school dropout rates at the school- and locality levels in the WB. In our sample, violence might not be driving our results for the following reasons. First, we control for the district-by-year fixed effect and the locality linear trend, which can sweep out the effect of variation in violence on labor market conditions. Second, the differential impact of commuting restrictions on genders within localities, protracted analysis, heterogeneous analysis, the dropping of localities with high-conflict intensity, and relatively non-violent periods, all of which received consideration in our sample, lead to uncertainty regarding the possibility that violence would play an important role in our estimates. Third, the results are robust to controlling for locality-specific time-varying observables, including the number of Second Intifada-related Palestinian fatalities.

6. Conclusion

This study finds that youth high school dropout rates in the WB declined during the period 1999–2006 in the aftermath of the closure of the Israeli labor market at the outset of the Second Intifada. The Israeli labor market's closure constituted a major negative demand shock and a positive supply shock) for unskilled workers, which reduced the opportunity cost of attending school. The closure's impact is mainly driven by males, owing to the specific nature of commuters (who are mostly male). The chief potential causal mechanism is the decline in employment opportunities for school dropouts.

These findings are striking, and they provide strong support for the human capital theory. To the best of our knowledge, ours is the first study to examine the impact of a labor market shock of this magnitude, which was also coupled with many adverse economic outcomes, on educational outcomes in an underdeveloped labor market.

Moreover, to the best of our knowledge, this study is also the first to shed light on the long-term consequences of the Israeli Palestinian labor market dynamics by evaluating the overall response of educational attainment to disruptive conflict-induced shocks.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.labeco.2020.101901.

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[28] We found that the closure had a differential, albeit small, impact on the probability of being employed in the public sector for the entire employed sample across localities in only two years: 2001 and 2002. Thus, public sector employment is unlikely to be a driving mechanism in our sample.

[29] Miaari et al. (2014) show that variations in the drastic initial decline in employment opportunities during the first 2 months of the Intifada explain one-fifth of the number of fatalities at the locality level up to March 2002. However, there is no evidence on the long-run effect of the pre-Intifada commuting share on violence up to 2006. Brück et al. (2019) show that the level of violence is not correlated with economic and labor market conditions in the WB, and its time-space variation is as good as random.

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