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Send correspondence to:
Ayhab F. Saad
Doha Institute for Graduate Studies
asaad@dohainstitute.edu.qa

[a] Falah is affiliated with Palestine Polytechnic University, Palestine. Email. Bfallah2000@yahoo.com. Saad is affiliated with the Department of Development Economics, Doha Institute for Graduate Studies, Doha, Qatar. Email. asaad@dohainstitute.edu.qa

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Abstract

This study uses the closure of Israeli labor market to examine the effect of a large labor market shock on educational choices of Palestinian youth. Immediately after the outbreak of the Second Intifada (Palestinian uprising against Israel) in October 2000, Israel severely restricted the entrance of Palestinian workers (commuters) to its labor market, which resulted in a more than 50% reduction in the number of Palestinian workers in Israel, mostly male workers concentrated in low-skill jobs. Our identification strategy relies on the variation in the geographical distribution of commuters within the West Bank prior to the Second Intifada. We implement a difference-in-difference strategy to compare high school dropout rates between localities with different commuting shares before and after the shock. We find that the closure had led to a significant decline in high school dropout rates in the case of male students aged between 15-19 years, but not for female students. The triple difference analysis confirms that the gender gap in high school dropout rates had decreased more in localities with high commuting shares than that in localities with low commuting shares. These effects are driven by the significant decline in employment prospects for school dropouts in localities with high commuting shares before the shock.

JEL Classification: I20, I26, J2, J24, O12

Keywords: High school dropout, labor market shock, the Second Intifada, Palestinian youth

ملخص

تستخدم هذه الدراسة إغلاق سوق العمل الإسرائيلي لدراسة أثر صدمة سوق العمل الكبيرة على الخيارات التعليمية للشباب الفلسطيني. فبعد اندلاع الانتفاضة (الفلسطينية) الثانية (ضد إسرائيل) مباشرة في أكتوبر 2000، فرضت إسرائيل قيودا مشددة على دخول العمال الفلسطينيين (الذين يتنقلون إليها) إلى سوق العمل، مما أدى إلى انخفاض عدد العمال الفلسطينيين في إسرائيل باكثر من 50%. ويتركزالعمال الفلسطينيين في إسرائيل، ومعظمهم من الذكور، في وظائف منخفضة المهارات. و اعتمدت الدراسة على التباين في التوزيع الجغرافي للعمالة الفلسطينية في إسرائيل بين مناطق الضفة الغربية قبل اندلاع الانتفاضة الثانية لتحديد العلاقة السببية بين الإغلاق و الخيارات التعليمية للشباب الفلسطيني.

نحن في دراستنا نطبق إستراتيجية فرق الاختلاف لمقارنة معدلات التسرب من المدارس الثانوية بين محليات ذات نسب تنقل مختلفة قبل وبعد صدمة سوق العمل. ونجد أن الإغلاق أدى إلى انخفاض كبير في معدلات التسرب من المدارس الثانوية في حالة الطلاب الذكور فقط الذين تتراوح أعمارهم بين 15 - 19 سنة. يؤكد تحليل فرق الاختلاف الثلاثي أن الفجوة بين الجنسين في معدلات التسرب من المدارس الثانوية قد انخفضت بدرجة أكبر في المحليات ذات المساهمة العالية في التنقل أكثر منها في تلك الموجودة في المواقع ذات المساهمة المنخفضة في التنقل. ويعزى هذا التأثير إلى انخفاض كبير في فرص عمل المتسربين من المدارس في المحليات ذات المساهمة المرتفعة فى التنقل قبل الصدمة.

1 Introduction

Human capital accumulation is one of the main factors contributing to economic growth (Mankiw, Romer, and Weil, 1992). Youth, 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 market conditions. The extent to which the educational choices of youth respond to large and abrupt shocks in imperfect labor markets is an important question and, yet, is largely unanswered. Sizable shocks, especially in developing economies, can potentially disincentivize youths from investing in human capital and thereby undermine ambitious national development goals in the long-run.

While the link between human capital and labor markets have been extensively studied in the literature, the empirical evidence on the impact of abrupt, large, and long-term perceived shocks in imperfect labor markets on youth's schooling choices is limited. The reason is that large shifts in labor markets happen over a long period and are usually coupled with economy-wide reforms, which pose serious difficulties in finding proper identification strategies.

This study examines the schooling choices of Palestinian youth (aged 15–19 years) in the West Bank in response to a sizable labor market shock. After the outbreak of the Second Intifada, in October 2000, Israel almost entirely closed its labor market, forcing many daily Palestinian commuters to seek jobs in the West Bank. The closure of the Israeli labor market might have induced youth in the West Bank to invest more in education by lowering the opportunity cost of attending high school and thereby increased returns to schooling. Prior to the Second Intifada, low-skill jobs in Israel were largely outsourced to the Palestinian labor market, generating a huge demand for unskilled workers who were employed at relatively high wages. Post the Second Intifada, youths at the verge of entering the labor force were not readily employable in the lucrative Israeli labor market; this scenario led to the disappearance of highly accessible and well-paid jobs for school dropouts (negative external demand shock for unskilled Palestinian workers). Additionally, unskilled workers in the local labor market were faced with increased competition from both skilled and unskilled return commuters, further lowering the prospects of employment and wages of unskilled workers (positive unskilled labor supply shock).

This natural experiment is suitable to address the question at hand for two main reasons. First, the shock is abrupt, sizable, and, to a certain degree, unanticipated. In the West Bank, at the eve of the Second Intifada, the share of commuters dropped from 25% to 5% in the first quarter of 2001, recovering slowly to reach 12% in 2006. The impact of the shock on the Palestinian labor market was enormous. Notably, wages declined, and the unemployment rate soared to an unprecedented level, mainly for the low skilled laborer.[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 and accelerating the import of foreign (non-Palestinian) workers, which were perceived by many Palestinians as long-term structural shifts with protracted implications on the dynamics between the two labor markets.[2]

[1] Refer to, for example, (Mansour, 2010; Miaari and Sauer, 2011; Fallah, 2016).

[2] Evidently, the share of commuters to Israel has risen to 15% (10 percentage points less than the pre-shock level) at the end of 2012 (almost 7–8 years after the end of the Second Intifada). For the impact of foreign workers on labor

Our empirical strategy relies on the variations in the share of commuters out of the total employed across West Bank localities before the Second Intifada. The closure is expected to have a differential impact on labor market outcomes and educational choices across localities with high and low commuting shares before the shock. Our difference-in-difference estimation (DD) compares the school dropout rates in treated localities (high commuting shares before the Second Intifada) with the comparison localities (low commuting shares) before and after the closure. The data supports the main identifying assumption of equal trajectories in high school dropout rates in treated and comparison localities before the shock. We provide evidence that our results are not driven by changes in observable variables (compositional effect), and that internal migration within the West Bank was rather very limited during the investigated period. To further address the concern that commuting share prior to the shock might be correlated with locality unobservable variables, we use the distance between the locality and the nearest crossing point to Israel to instrument for commuting shares. The main results are robust to the instrumental variable approach.

Using Palestinian Labor Force Survey (PLFS) data from 1999 to 2006, we find that the probability of dropping out from high school decreased more for youth in treated localities when compared to the comparison localities; the high school dropout rate declined by 4 percentage points (about 22% relative to the mean dropout rate in the West Bank from 1999–2006) more in localities at the 75th percentile than that in localities at the 25th percentile of the commuting share distribution, prior to the shock. Nonetheless, we do not find a significant effect of the closure on college enrollment.

We develop a simple discrete choice model of educational choices and show that the closure impacted educational choices through the following two mechanisms: (1) the prospect of employment for school dropouts and (2) the forgone lucrative wage in Israel coupled with increasing skilled wage premium in the domestic market. Consistent with our theoretical predictions, we show that the impacts of commuting restrictions on schooling choices are driven by changes in local labor markets and cannot be explained by alternative mechanisms. The results indicate that the prospect of employment for school dropouts and wages for unskilled laborers declined more in the treated than in the comparison localities. To rule out other mechanisms, we provide evidence that violence, household income and parental job loss, and expansion in the public sector cannot be driving our results.

The closure is expected to have a differential impact not only across localities but also across genders as more than 98% of commuters are males. We provide evidence that the impact of closure on educational attainment is driven by males. The triple difference model (DDD) compares the school dropout rates between genders within treated localities, purging out gender differences in schooling within comparison localities, before and after the shock. Again, we find the differential impact of closure on male-female schooling choices within localities. If the prospect of employment for school dropouts is the true mechanism, then we should find that this mechanism is at play only for males. The empirical results confirm that the differential impact on the probability of unemployment for school dropouts across localities is driven by males. These findings reinforce our identification strategy and provide strong evidence that our results might not be driven by alterna-

market outcomes in the West Bank, refer to (Aranki and Daoud, 2010; Miaari and Sauer, 2011).

tive mechanisms.[3] Taken together, the results of this study suggest that an increase in commuting restrictions reduced the opportunity cost of attending school, thereby leading to a decline in the school dropout rates in the West Bank.

This study contributes to an area that has become an active research topic in the last 20 years. The surge in wage income inequality in the last three decades has coincided with an increase in the number of college graduates. This suggests a sluggish response of educational attainment to skill-biased technology changes that fundamentally transformed the world economy. In the light of these developments, researchers were drawn toward employing the celebrated theory of human capital of Becker (1969) by empirically quantifying the degree to which educational attainment responds to changes in the opportunity cost of attending school and in returns to schooling.

Referring to the labor demand shocks induced by the introduction of fracking technology or booms and bursts in natural resource prices, many studies show that the demand for less-educated male labors in fracking and the high opportunity cost of remaining enrolled increased the school dropout rate (Black, McKinnish, and Sanders, 2005; Emery, Ferrer, and Green, 2012; Cascio and Narayan, 2015; Michaels, 2011; Weber, 2012, 2014). Our study differs from the aforementioned studies along two important directions. First, the magnitude of shock emanating from the closure of Israeli labor market is much larger and more economically significant when compared to shocks discussed in these studies. To put things into perspective, Cascio and Narayan (2015), Maniloff and Mastromonaco (2014), and Feyrer, Mansur, and Sacerdote (2017) find that the introduction of fracking technology increased employment in the U.S. by less than 0.5 percentage points (around 315,000–600,000 jobs). Second, we study the response of educational choices to changes in the opportunity cost of schooling in an imperfect labor market and under deteriorating economic conditions. This feature distinguished this study from the rest in that it sheds light on youth schooling choices in developing countries wherein large shocks induce major economic impacts for both households and individuals.

This study is related to studies that estimate the impact of policy reforms and access to global markets on human capital accumulation. For instance, Kuka, Shenhav, and Shih (2018); Amuedo-Dorantes and Antman (2017) study the impact of the enactment of the Deferred Action for Childhood Arrivals (DACA) in the U.S on educational attainment of immigrants. Abramitzky and Lavy (2014) investigate the changes in the high school- and college attendance in Israeli kibbutzim due to a salient redistributive policy reform. Atkin (2016) and Oster and Steinberg (2013) evaluate the impact of expanding manufacturing and services exports opportunities in Mexico and India, respectively, on school enrollment. This study contributes to these important studies by considering the impact of a general economy-wide shock rather than context-specific policy reforms or a shift in demand for skilled (unskilled) laborers induced by sector-specific exports. Our study also adds to studies examining the impact of economic shocks on investment in human capital. Shah and Steinberg (2017) find that a negative economic shock led to a decline in the school dropout rate in rural India, whereas Charles, Hurst, and Notowidigdo (2015) show that college enrollment declined in response to the housing boom in the U.S. Again, the magnitude of the shock discussed in the current study is larger when compared to shocks discussed in above-mentioned studies and

[3] An additional alternative mechanism that can be ruled out by the DDD difference analysis is the quality of schools.

has a more salient effect on the economy.

In the context of the Palestinian economy, to the best of our knowledge, this study is the first to examine the impact of the closure of Israeli market on the schooling choices of the Palestinian youth. From a social perspective, few studies evaluate the effect of violence on children's elementary and secondary education and on the quality of education for high school students. Maio and Nisticó (2016) find a negative effect of parental job loss, induced by the intensity of conflict during the Second Intifada, on child school dropout.[4] Brück et al. (2014) show that violence lowers the score on the high school national exam, but not high school dropout, at the locality-school level.[5] Conversely, this study aims to isolate the impact of major shifts in labor market outcomes from violence and declining household income on youth school dropout. While these studies find a negative impact of violence on educational outcomes during the Second Intifada, our results show that the effects of shifts in labor markets dominate other shocks related to the Second Intifada and thereby contribute to an improvement in overall school enrollment.

This study complements the literature on the impact of the Second Intifada on the labor market of the West Bank (Mansour, 2010; Fallah, 2016; Miaari and Sauer, 2011; Cali and Miaari, 2018; Farsakh, 2002; Ruppert Blumer, 2003) by providing a different identification strategy, relying on the locality pre-shock share of commuters rather than on the change in commuting share over time within districts. In line with the findings of the aforementioned studies, this study confirms the adverse impact of the closure on employment and wages in the West Bank, especially for unskilled workers. Nonetheless, this study emphasizes the long-run consequences of the Israeli labor market on the Palestinian economy by evaluating the effect of commuting restrictions on investment in human capital.

The rest of the paper is organized as follows. Section 2 provides a brief background of the labor market in Palestine and its connection with the Israeli labor market. Section 3 discusses the data and provides descriptive statistics. In Section 4, we discuss the empirical design and the results. In Section 5, we develop a simple model of educational choices and examine the potential mechanisms. Section 6 concludes the study.

2 Institutional Background

Immediately after Israel occupied the West Bank and Gaza strip (WBGs), the Israeli government granted full access to Palestinian workers to seek employment in its labor market (Farsakh, 2002). High unemployment in the WBGs and higher wages in the Israeli labor market had induced many Palestinian workers to commute to the Israeli labor market. By 1970, the share of commuters accounted for one-third of the total Palestinian workforce. The share of commuters continued rising throughout the 1980s, accounting for more than 40% of the total workforce (Angrist, 1996; Ruppert Blumer, 2003).

[4] In a related vein, Di Maio and Nandi (2013) find that an increase in the number of closure days led to an increase in child labor during the Second Intifada.

[5] An older literature examined returns to schooling in the West Bank and Gaza during in the 1980s and the 1990s (Angrist, 1995, 1996).

After the outbreak of the first Intifada[6], and mainly since the first Gulf war in 1991, Israel started implementing a closure policy. The Israeli government set 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 of enforcement of this policy has depended on violence intensity and security needs. The share of commuters not only varied significantly by districts of the West Bank but also markedly varied across localities within districts (Refer to Online Appendix, Table A.2).

On the eve of the Second Intifada, Israel closed its border with the West Bank (WB), barring work permits for most commuters, and consequently leading to a significant decline in the share of commuters. Fig.(2) depicts the quarterly share of commuters from 1999–2006. The commuting share dropped from 25% in the third quarter of 2000 to around 5% in the first quarter of 2001. It slowly increased in the subsequent quarters but remained far below the initial level (around 10% in 2006). The strength and length of commuting restrictions indicated a fundamental shift in the dynamics between the two labor markets. Prior to the Second Intifada, Israel relied on the WBGs as the main source of cheap and unskilled workers. The occasional imposition of stringent commuting restrictions from before 2001 was perceived as a temporary measure taken in response to shifts in the political environment. After 2001, Israel started to adopt long-term policies that aimed to change the linkage between the two markets structurally. Particularly, Israel accelerated the imports of foreign workers, as a substitute to Palestinian workers, and started building a wall between the WB and Israel—the separation wall. By mid-2001, Palestinians had captured these structural changes and updated their expectations about regaining access to the Israeli labor market in the near future (Mansour, 2010).

The Second Intifada and the subsequent Israeli policies had severe ramifications for the Palestinian economy, especially for the unemployment rate and wages of the low-skilled workers, transforming the structure of the labor market in the WB. Figs.(A.1)-(A.2), in the Online Appendix, plot the quarterly share of commuters, unemployment rate, and skill wage premium from 1999–2006 to visualize the correlation between commuting shares and labor market outcomes. Simultaneously, school dropout rates for the youth witnessed a rapid decline in the investigated period for both males and females. The youth school dropout ratio declined by about 13 percentage points (from 28% in 1999 to 15% in 2006—refer to Fig.2). It is evident that the decline in dropout rates, 2–3 years after the commencement of the Second Intifada, had accelerated and followed a relatively flat pattern after 2004, closely following the pattern of the commuting share. Fig.(3) plots the quarterly dropout rate for youth by gender. The Figure reveals that females had lower dropout rates than males for the whole period. Importantly, the educational gap widened at an accelerated rate before the second Intifada, followed by a stable period up to 2003, and subsequently started to increase slowly up to 2006. This Figure presents a very telling argument. It suggests that the closure of the Israeli labor market led to a decline in the school dropout rates in the West Bank, but the effect was mainly evident in the case of males—a central finding in the current study.

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

Was there a difference between the commuters and non-commuters before the second Intifada? Were the 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 their schooling choices. In other words, the closure constituted a positive unskilled labor supply shock and, importantly, a negative demand shock for the existing and prospective unskilled workers.

Data from the PLFS of 1999 shows that commuters were less educated than non-commuters and were mostly concentrated in low-skill jobs (industries). In 1999, the average years of schooling for non-commuters was one year higher than that of the non-commuters. Most of the commuters were male (98%) and performed low-skill jobs; approximately 82% of them worked in craft and elementary occupations. In terms of the type of industry, most of them (68%) were employed in the construction sector, followed by agriculture (11%), and manufacturing (10%) in 1999.

The distributions of the years of schooling reveals stark differences between commuters and non-commuters (refer to, Fig.1 and Table 1). For instance, 18% of commuters had a high school degree or above, and only 1% earned a bachelor's degree or above. Conversely, 32% and 11% of non-commuters had a high school degree and bachelor's degree, respectively. Commuters were, on an average, 4 years younger than non-commuters (30-year-old vs. 34-year-old). Approximately 29% of youth male workers were employed in Israel (compared to around 24% of adults) and 13% of the total commuters were youth. Table (A.1), in the Online Appendix, shows that the returns to education for commuters was negligible. Nonetheless, the daily wage of commuters was almost 40% higher than the daily wages of non-commuters, on average.

3 Data and Descriptive Statistics

Our primary dataset comes from the Palestinian Labor Force Survey (PLFS) collected by the Palestinian Census Bureau of Statistics (PCBS). The PLFS data is collected on a quarterly basis, with a nationally representative sample of approximately 7,600 households in the West Bank and Gaza strip. In this study, we use the PLFS data collected during 1999–2006. We restrict our analysis to 2006 (the official end-year of the Second Intifada) as mobility restrictions to Israeli labor market and within the West Bank had been loosened and the number of Palestinian workers in Israel started to increase in 2007. The earliest readily available data that we could obtain is for 1999. The PLFS contains rich information about the individual and household characteristics including employment status, daily wage, sex, age, years of schooling, whether a school dropout, place of work, district and locality of residence, marital status, and the number of household members.

We restrict our analysis to localities in the West Bank, excluding East Jerusalem and Gaza strip. The reason is that many localities in the East Jerusalem were not subject to the mobility restrictions. We also exclude from our analysis localities from Gaza strip because our identification strategy does not apply to Gaza strip.[7] To deal with the pseudo panel sampling issue, all panel observations are eliminated, except the first appearance.[8] Eventually, the final sample consists of randomly repeated

[7] Labor market localities in Gaza strip contiguously form one labor market.

[8] Observations in any quarter consist of roughly one-half of individuals (households) interviewed in the previous quarter plus another one-half randomly drawn household. By design, each household is interviewed four times—

cross-sectional observations, as presented in the proposed econometric specification in the current study.

Our final sample consists of 165 localities, 37,355 households, 142,835 individuals, of which 22,425 are youth (aged between 15 and 19 years). The yearly locality-level variables were aggregated using the PLFS probability weights. In our analysis below, we focus on the high school dropouts, and thus we exclude all youth who never attended school or who dropped out early. Table (2) summarized the main locality-level indicators. We divide the localities into the following two groups: localities with a low exposure to the Israeli labor market (share of commuters to Israel in the total employment by locality was lesser than the median share) and localities with a high exposure to the Israeli labor market in 1999 (commuters' share was larger than that of the median share). 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 be driven by the small observed differences across localities prior to the shock, we control for many locality-level characteristics in our preferred econometric specifications.

4 Empirical Design

We utilize a difference-in-differences (DD) model to estimate the effect of commuting restrictions that Israel imposed during the Second Intifada on high school enrollment for youth in the occupied West Bank. The model is specified as follows:

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

The binary variable Y_{ilst} is equal to one if the individual i in locality l, district s, and year t is a high school dropout, and zero otherwise. The treatment variable $S1999_l$ is continuous, measuring the locality's share of commuters in 1999. The share is measured relative to total male employment. This is taken as a continuous treatment measure because localities with a higher share of commuters in 1999 were disproportionately affected by the commuting restrictions; that is, these localities witnessed a larger unskilled labor supply and a decline in employment potential in Israel owing to the closure of the Israeli labor market (refer to the next subsection for further discussion). $S1999_l$ is interacted with the treatment period dummy $Post$. The dummy variable $Post$ takes a value of zero for 1999 and 2000 and one for the remaining years until 2006.[9] The main coefficient of interest, β, is the DD estimate, comparing the probability of school dropout after and before the shock for localities with different commuting shares in 1999.

We also include several locality-level and individual-level controls. We interact the $Post$ with several baseline locality control variables, measured in 1999, collected in the vector $X1999_l$. These

quarter 1 and 2 of year X and quarter 1 and 2 of year X+1.

[9] As shown by (Mansour, 2010), at the outset of the second Intifada in October 2000, most commuters perceived the initial closure of the Israeli labor market as a temporary procedure. After the second quarter of 2001, most previous commuters stopped reporting Israel as their place of work. Eventually, the share of commuters (employed and unemployed) in labor force started to decline by mid-2001.

include the unemployment rate, share of individuals below 16 years of age in the total population, share of wage workers in total employment, and urban and rural areas dummies (camps are treated as the reference group). The reason behind including these variables is to control for factors that might affect the future trajectories of the locality's dropout rates apart from the labor market shock. It also ensures that the commuting shock does not confound other factors. This would be the case if, for example, cross-locality differences in the commuting share correlate with poor economic conditions and lead to a possible change in the impact of these variables on schooling choices after the shock. Individual and household level controls, collected in the vector W_{ilst}, include years of education for both parents (or the head of the household in some specifications), age and sex dummies, the number of members in the household below 16 years of age, and the total number of household members. Year dummies, T_t, control common time-varying trends. 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 detrending locality 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. This is possible because the within-district variation of the locality-commuting share is as large as the cross-district variation. The error terms ϵ_{ilst} are clustered at the locality level (refer to Table A.2 in the Online Appendix)

4.1 Identification

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

$$Y_{ilst} = \alpha + \sum_{\tau \neq 1999} (\beta_\tau S1999_l \times T_t^\tau) + \sum_{\tau \neq 1999} (\boldsymbol{\theta_1'} \boldsymbol{X1999}_l \times T_t^\tau) + \boldsymbol{\theta_2'} \boldsymbol{W}_{ilst} + T_t + \lambda_l + \gamma_{st} + \epsilon_{ilst}, \quad (2)$$

T_t^τ denotes the individual year dummy variable that takes 1 if $\tau = t$, for $\tau = 2000, .., 2006$, and zero otherwise. β_τ takes a unique value for each year from 2000–2006. The coefficient for the baseline year 1999, $\beta 1999$, is set to zero. The remaining variables are defined above. This setting informs us whether differences in pre-existing trends will confound our findings. If the coefficient, β_{2000}, is statistically indistingushable from zero, this suggests that the outcomes of interest were not following different trajectories in localities with different commuters' shares before the shock.

Our empirical design and identification crucially hang on the assumption of limited internal migration and mobility. This concern is not unique to the current study and common to almost all the studies investigating the differential impact of the Second Intifada on a locality (district) post intifada (Miaari et al., 2014; Mansour, 2010; Zimring, 2015; Maio and Nisticó, 2016; Amodio and Di Maio, 2017). Using the migration data, Zimring (2015) shows that more than 95% of the popu-

lation in the West Bank live in the same born-district. Additionally, around 7% of internal migrants in the West Bank had migrated for work-related reasons. Abrahams (2018) reports a low incidence of internal migration in the West Bank, using the 2007 census data. (Mansour, 2010; Miaari et al., 2014) argue and show that commuting across the West Bank cities was limited during the Second Intifada, and hence the effect of returning commuters was observed locally.[10]

To further investigate the identifying assumption of DD, we show that the small differences of baseline individual and household characteristics across localities are stable over time and the divergent path of the outcome after the shock between localities with high and low commuting shares cannot be accounted for by these observable variables. We proceed in the following two steps. First, we fit a logistic regression model for school attendance for youth aged between 15 and 19 years before the shock using individual observable variables including dummies for age and sex, the number of household members aged less than 16 years, the total number of household members, and parents' years of education. Subsequently, we generate the predicted school dropout for the entire period (up to 2006). In the second stage, we estimate Eq.(2) using the predicted outcomes as dependent variable. Fig.(4) shows that the predicted dropout cannot be explained by our treatment variable (commuting share in 1999).[11] Moreover, we use the DD approach, as in Eq.(1), and placebo regressions show that the dropout ratios for children and older generations did not follow differential paths after the shock across different localities, mediating the concern that internal migration might be driving the results. Overall, there is ubiquitous evidence of limited migration and commuting within the West Bank during the investigated period due to severe mobility restrictions 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 and during the investigated period is correlated with the locality pre-Intifada share of commuters.[12] We argue that this is not the case. First, Israel almost entirely closed its labor market indiscriminately for all Palestinian commuters, regardless of their geographical location, personal characteristics, and security clearance status. Second, Israel started to increase the number of permits in the subsequent years, adopting a number of pre-shock existing criteria including age, marital status, and security history (not education level), which are mostly uncorrelated with the pre-shock share of commuters (refer to the discussion below and Fig. 5).

If the pre-shock commuting share is correlated with locality-specific omitted variables and the impact of these variables on educational choices varied before and after the shock, then there might be a possibility of the DD estimate being biased. 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—the proximity to Israel and, to a lesser extent, the network effect (Miaari et al., 2014). We show that

[10] Contrary to internal migration that might bias our results, within the West Bank daily commuting works against our results. It weakens the differential impact of the closure of Israeli labor market on schooling choices across localities, but the impact is still identifiable and unbiased—refer to Section (5).

[11] We experimented with different methods to produce the predicted outcomes. We tested continuous probability on the interval $[0, 1]$ (used in the Fig.4) and binary predicted outcomes using different probability cutoffs. The results are robust to these methods.

[12] The DD estimators will be biased if the selection on treatment is correlated with unobservable time-varying covariates.

our results are robust to using the geographical distance between localities and the nearest crossing point to Israel as instrumental variable to commuting share.

Another concern regarding our identification is that the second Intifada conflated other shocks, such as violence, disruption in international trade, and economic slowdown; this conflation might affect the education of the Palestinian youth in the West Bank. Although these are facts, we argue that these shocks are not locality-specific; additionally, they are uncorrelated with the commuting share in 1999 and do not drive our finding. In Section (5), we address this issue in detail. By presenting arguments, we show that these conflated shocks are either uncorrelated with the commuting share in 1999 or cannot be driving our results. We also control for the locality-specific time-varying variables, such as violence, and show that the results hold.

Before discussing the results, we conduct an additional check on the empirical design to show that the treatment variable—the share of commuters in 1999—captures the proposed treatment (the size of the shock).[13] We estimate that the Eq.(1) for all male workers with Y_{ilst} takes one if individual i is employed in Israel and zero otherwise. The results in Table (3) show that the probability of being employed in Israel for male workers declined by 13 percentage points more (about 90% relative to mean commuting share between 1999–2006) in localities at the 75th percentile of commuting share in 1999, 0.4, than in localities at the 25th percentile, 0.15. Clearly, as shown in Fig.(5), the change in 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.

4.2 Results

The estimates of the effect of the commuting restriction on the probability of school dropout are presented in Columns (1)–(5) of Table (4). Column (1) exhibits a parsimonious model, including the estimate of the treatment variable, year fixed effects, locality fixed effects, and district-by-year fixed effects. The locality-specific variables interacted with $Post$ are added to Column (2). Column (3) further controls for individual and household characteristics. Column (4) restricts the sample to youth living with their parents and adds mother's education to the individual controls. Column (5) controls for the locality time trend. The DD estimates across all specifications are highly significant and fairly stable. In Columns 4 and 5, a one standard deviation increase in commuting share in 1999 lowers the dropout probability by 3.1 (3.5) percentage points (about 15.5% (17.5%) of the mean probability of youth dropout from 1999–2006, 0.20).

Fig.(6) visualizes the year-by-year treatment estimates using the Eq.(2) and controlling for the baseline locality and individual covariates. The dots denote the magnitudes of the impact of the closure for each corresponding year, β_τ, while the vertical red lines reflect the corresponding 95% confidence interval with standard errors clustered at the locality level. The results show that the estimate of $\beta_{\tau=2000}$ is small compared to the post-treatment-years and statistically insignificant, indicating that the outcome variable did not follow differential pre-trends for localities with different com-

[13] The share of commuters in 1999 would trivially as well as fully capture the size of the shock on the locality level in case of a complete closure of the Israeli market (if the commuters' share dropped to zero everywhere, thus making the size of the shock equivalent to the pre shock share of commuters).

muting share prior the shock. The results also show that the impact of the closure of Israeli market occurred in 2001 and remained negative and statistically significant over the treatment years. The magnitudes of the impacts are close to our preferred model estimates with small variation over time.

Table (5) reports the results separately for the male and female samples with and without the locality linear trend as in Columns (4) and (5) of Table (4). The effect of the commuting restriction is only negative, sizable, and highly significant for the 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 5 percentage points (20% relative to the male average dropout, .245). Fig.(7) shows that the dropout rates for males (females) followed a similar trend before the shock for localities with a high- and low-commuting share.

Given that more than 98% of commuters prior the shock are males, the shock is supposed to have a differential impact on gender within localities. To exploit this third dimension (asymmetric shock across localities and gender over time), we estimate a triple difference model, DDD, by comparing the probability of school dropout across time, gender, and locality. Contrary to the DD wherein estimations are conducted for males and females separately, the DDD allows us to compare male-female dropout within localities.[14] The coefficient of the triple interaction term (gender-Share1999-Post) is negative and statistically significant, which lends more credibility to our identification and the heterogeneous effect analysis. Compared to localities at the 25th percentile of the commuting share, the gap in the high school dropout rates between males and females narrowed by 5.6 percentage points more in localities at the 75th percentile of the commuting share (around 56% relative to the mean dropout gender gap from 1999–2006, 0.1). The DDD rules out the concern that the unobservable locality time-varying variables might confound the negative impact of the closure on schooling dropout, as shown in the above estimates. Overall, the heterogeneous effect analysis is an important evidence that the estimated response of educational choices to the closure is mainly driven by shifts in labor market outcomes and it might not be driven by shocks that are conflated by the Second Intifada, which have common effects, such as violence, on both the genders (for further discussion on this point, refer to Section 5).

In this part, we address the question whether the higher high school enrollment had translated to higher college enrollment. A necessary requirement for college admission is to pass the general high school exam called *Tawjihi*. The most selective universities require at least a score of 80%, while other colleges may accept students with marginal pass scores as low as 60%. Since the marginal students are likely to deliver poor performance in *Tawjihi*, an increase in enrollment of marginal students to high school might slightly affect college enrollment. We estimate the Eq.(1) to investigate the response of college enrollment for 19–22-year-olds, who finished high school, to

[14] The DDD specification is given by:

$$Y_{iglt} = \alpha + \beta S1999_l \times Post \times gender_{iglt} + \boldsymbol{\theta_2'} \boldsymbol{W}_{iglt} + gender_{iglt} \times T_t + gender_{iglt} \times \lambda_l + \lambda_{lt} + \epsilon_{ilt},$$

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 between gender, locality, and year dummies.

the closure of Israeli labor market. We construct a new dependent variable that equals to one if the years of education is greater than 12, and zero otherwise, for students aged 19–22 years who have finished high school. The results are reported in Table (6). Apparently, the closure does not have any effect on the college enrollment of students who finished high school.

4.3 Robustness Checks

In this subsection, we check the robustness of our findings on several issues. Instead of the continuous treatment adopted in the main specifications, we re-estimate the Eq.(1) using a binary treatment dummy that equals one if treated and zero otherwise. The treatment dummy takes one for localities above the median share of commuters in 1999, and zero otherwise. The results are reported in columns (1)–(3) in Table (7). The results are qualitatively similar to the continuous case. The probability of school dropout is 5.5 percentage points lower in treated than in the non-treated localities, following the closure of the Israeli labor market. The heterogeneous effect of the shock on genders is clearly presented in columns (2) and (3). The results are also robust to different treatment assignment cutoffs. We consider localities at the 30th percentile of the commuting distribution in 1999 as controls. Localities at the 70th percentile of the distribution are assigned to the treated group, while all remaining localities are excluded from the sample. The results are shown in columns (3)–(6) in Table (7). The results show that the impact of the closure on educational choices is larger than that of the median treatment cutoff discussed above.[15] The event analysis confirms that the pre-trend in the high school dropout rates exhibited statistically similar paths for the treated and control localities (see Online Appendix Fig.A.4).

We undertake a placebo test that targets educational choices of cohorts that are presumably not affected by the shock—those in an older age category (25–30 years) or younger cohort aged 10–14 years old.[16] The results are reported in the Table (8). Results for all the specifications—younger and older cohorts and males and females—are statistically insignificant. These findings partially ensure that the internal migration and/or the quality of available data might not be driving the estimated negative impact of the closure on youth educational choices. The event analysis confirms that the pre-trend in the school dropout rates exhibited statistically similar paths for the treated and control localities (see Online Appendix Fig.A.5).

In addition, we implement a falsification test to check whether the point estimates of the placebo commuting shares are close to our estimation of the true commuting shares. We reshuffle the commuting shares in 1999 randomly across localities while re-estimating Eq.(1). We repeat this process 2,000 times and plot the probability density function of the coefficients of the interaction term ($Share1999^{placebo} \times Post$) in Fig.(8). The distribution of the coefficients is normally distributed around zero, and the true estimate is located far to the left of this distribution; this indicates that we can strongly reject the hypothesis that the coefficient, β, in Eq.(1) is zero by using the normal distribution created through the placebo falsification exercise.

We check whether our findings are driven by small localities or localities with extremely low (high) commuting shares. To address the former concern, we re-estimate the Eq.(1), dropping all localities

[15] The results in Table (7) are robust to the inclusion of the locality linear trend (not shown).

[16] Elementary schooling is mandatory and relatively free in the West Bank.

with less than 100 observations. The results change slightly (refer to Online Appendix, Tables A.4). For the latter concern, we exclude localities on the 1st and 99th percentiles of the commuting share in 1999 and re-estimate Eq.(1). The results are very close to the main findings (see Online Appendix Table A.3). The event analysis confirms the assumption of statistically similar dropout trajectories across localities before the shock in all the aforementioned specifications (see Online Appendix, Figures A.6 and A.7).

One concern with the event analysis is the short pre-treatment period of 2 years—1999 and 2000—in our sample. The PLFS data exists for the years 1997 and 1998, but the locality identifier is missing for those years. We use the unique household identification to match the 1997 and 1998 surveys with that of the main data at hand. Under the assumption of low internal migration, this approach allows us to identify the household locality in 1997 and 1998. Indeed, there are many issues with this approach, such as the following: (1) wrong locality identification due to migration, which cannot be dealt with in our dataset, and (2) a limited number of observations in 1997 and 1998 are matched due to the employed sampling techniques discussed above. With these caveats in mind, we repeat the event analysis, re-estimating Eq.(2) by using the extended dataset, to check whether the paths for the youth dropout rates were evolving differently for localities with different commuting shares in 1997 and 1998 relative to the base year 1999. Again, Fig.(A.8), in the Online Appendix, shows that the treatment coefficients for the years 1997, 1998, and 2000 are small and statistically insignificant, further confirming our identification assumption.

4.3.1 Results from Instrumental variables

The DD estimator might become biased due to omitted variables if the impact of those unobservable variables changes before and after the Second Intifada. In other words, if the share of commuters before the shock is correlated with locality-specific demographic/socioeconomics conditions, which we have not included in the vector $X1999_l$, then our results will be unbiased only if the impact of those omitted variables on school dropout rates remains constant over time. Admittedly, it is difficult to consider a concrete example of an omitted variable that is correlated with the pre-shock commuting share and has a differential impact on schooling choices before and after the shock. The closest example that we can consider is the locality's exposure to the international trade (exports). It is possible that exporting unskilled labor-intensive commodities will have an effect on schooling choices that is similar to that of exporting unskilled workers to Israel. Exporting unskilled labor-intensive goods might also be negatively correlated with commuting shares. The post-shock effect of exports on schooling choices might be more important when compared to the pre-shock effect after the alternative mechanism of absorbing unskilled workers (exporting workers to Israel) is muted. Under this scenario, the DD estimate is likely to biased downwards. To deal with the potential omitted variables (OV) issue, we implement an instrumental variable approach (IV).

The most obvious IV to pick 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 distance between localities and the closest entry point to Israel as IVs for the commuting share. The IV approach is an important step in ruling out the omitted variables problem in our context. We estimate the model by using a two-stage least squares (2SLS) technique. The

first- and the second stages are as follows:

$$S1999_l \times Post = \alpha + \eta_1 \left(\frac{1}{lndist_l}\right) \times Post + \boldsymbol{\theta_1'} \boldsymbol{X1999}_l \times Post + T_t + \lambda_l + \gamma_{st} + \epsilon_{lst} \quad (3)$$

$$Y_{ilst} = \alpha + \beta \widehat{S1999_l \times Post} + \boldsymbol{\theta_1'} \boldsymbol{X1999}_l \times Post + \boldsymbol{\theta_2'} \boldsymbol{W}_{ilst} + T_t + \lambda_l + \gamma_{st} + \epsilon_{ilst} \quad (4)$$

The natural logarithm of distance is denoted by $lndist_l$, and η_1 is expected to be positive and highly significant. $\widehat{S1999_l \times Post}$ is the predicted value of the commuting share interacted with $Post$ that is obtained in the first stage regression. All remaining variable are defined as in Eq.(1).

The results are reported in Table (9). Columns (1), (4), and (5) show the results from IV estimator for all youth, males, and females, respectively. Consistent with our findings, the impact of closure on high school dropout rates is positive and significant only for males. Column (2) reports the ordinary least squares (OLS) estimates of 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 impacts the school dropout rates indirectly via the commuting shares. The first-stage statistics are shown at the bottom of the table. The Kleibergen-Paap (KP) Wald F statistics for the weak instrument is 5.92; it is within the range of acceptable critical values reported by Stock and Yogo (2005), but below the highest values (16.38). It is worth mentioning that these critical values are generated under the assumption of the independent and identically distributed (i.i.d) stochastic errors and they are not necessarily very informative under clustered standard errors.[17]

5 Potential Mechanisms

Until now, we have shown that the probability of school dropout decreases more in localities with a large share of commuters than those with a low share of commuters in 1999. In this section, we explore potential underlying mechanisms. To motivate our analysis, we develop a simple discrete choice model of educational enrollment. Our model captures the essence of human capital theory that links educational choices to the opportunity cost of attending school and returns to school (Becker 1964). High school students choose between attending school or dropping out and seeking employment depending on the associated payoff, with all else remaining constant. If a given labor market shock decreases the opportunity cost of education, then an individual would more likely choose to stay at school and vice versa. In the following analysis, we explore two potential mechanisms that might change the opportunity cost of education and returns to schooling— employment prospects and skill-wage premium.

Youth dropouts experience the loss of an important potential employer, which provided relatively higher wages than the domestic market, after the shock. This change in the expected probability of being employed in Israel lowers the expected payoffs for school dropouts, thereby lowering the cost of attending school and acquiring a higher level of education. Youths coming from localities with a high commuting share before the Second Intifada had a high expected probability of commuting,

[17] Indeed, KP F-statistics would be much higher than 16.38 if we do not cluster the standard errors.

and thus the expected payoffs of leaving school declined much more than that of youths residing in localities with a smaller share of commuters. We refer to this channel as *expected commuting*.

As commuting shares drop as a result of the imposition of the mobility restriction, previous commuters are expected to seek employment in their local labor markets, which would lead to an increase in competition over jobs in localities that experienced greater share of return commuting. The increased competition over jobs is expected to disproportionately affect unskilled workers (current and prospect) as a result of an increase in unskilled-biased labor supply (return commuters), further lowering employment prospects for high school dropouts. We call this *increased competition* (Mansour, 2010).

The two channels together (*expected commuting* and *increased competition*) contribute toward a decline in the employment prospects for youth dropouts, thereby lowering the opportunity cost of attending school as well as school dropout rates. Additionally, the forgone wage in Israel for youth and an *increase in competition* might lead to a further decline in wages for unskilled workers, thereby increasing returns to schooling.

To formally analyze these two channels, we consider a very simple discrete choice model in which a high-schooler, i, in locality l is on the verge of choosing educational attainment, $edu \in \{(d)ropout, (c)ontinue\}$ to maximize own expected utility, U_{il}.

$$\max_{edu=d,c} U_{il} = \mathbb{1}_{edu} V_l^d + [1 - \mathbb{1}_{edu}] V_l^c + \zeta_i(edu) \tag{5}$$

The indicator function $\mathbb{1}_{edu}$ equals to one in case of a school dropout, and zero otherwise. The expected payoff of being a school dropout V_l^d is given by the expected wage income earned from working in Israel or the local market—$V_l^d = P_{ls}W_s + P_l W_l + u_l B_u$—where P_{ls}, P_l denote the probability of being employed in Israel and the local market l, respectively. Wages in Israel and the local market are represented by W_s, W_l, and the youth unemployment rate in the locality l is denoted by u_l. The unemployment benefit B_u is set to zero. The expected payoff of having a degree, V_l^c, is assumed to be constant before and after the shock and across localities, $V_l^c = V^c$. Moreover, we assume that skilled workers are substitutes for unskilled workers but not vice versa.[18] $\zeta_i(edu)$ is a random variable drawn from the type 1 extreme distribution function.

We adopt a few simplifying assumptions in the model that are as follows. First, we do not model the choice to work in Israel or local markets—youths calculate the expected payoff from dropping out of school by considering P_{ls} and P_l as given. Second, we assume that the wages in Israel, W_s, are exogenous to labor market condition in the West Bank (small economy). Third, the labor mobility within the West Bank before the Second Intifada was relatively unrestricted, and thus we assume a common wage in the West bank, $W_l = W_{wb} \, \forall \, l$ (domestic wage in 1999 was fairly identical across localities, around 50 Israeli Shekel). Additionally, there were small variations in unemployment rates across localities before the shock (much lower than the commuting share), and therefore we normalize the unemployment rate to zero in all localities before the Second Intifada, $u_l = 0$.[19]

[18] These assumptions are supported by the empirical findings in the literature (refer to Mansour (2010)).

[19] As a direct result of normalizing the unemployment rate to zero in all localities, $P_{ls} + P_l = 1$.

Finally, we assume that only unskilled workers commute to the Israeli labor market (commuters are less educated than non-commuters).[20]

The expected probability of being a dropout in locality l, π_l, which solves the maximization problem in Eq.(5), is given by:

$$\pi_l = \frac{e^{V_l^d}}{e^{V_l^d + V^c}}. \tag{6}$$

In this simple discrete choice model, and given that the wage in Israel is higher than the wage in the West Bank, $W_s > W_{wb}$, it can be clearly shown that the fraction of school dropouts in the locality l increases in P_{ls} (i.e., commuting share) before the Intifada, *ceteris paribus*.[21]

After the Second Intifada in 2001, Israel prohibited Palestinians from working in Israel. To simplify things, let the probability of being employed in Israel after the Second Intifada $P_{ls}^{closure} = 0$ for all l.[22] We analyze the effect of this shock on youth educational decisions under the following two scenarios: (1) free labor mobility between localities after the shock and (2) segmented local labor markets after the shock.

Case 1: free labor mobility. If workers can move freely across local labor markets, then the positive labor supply shock (returning commuters) will be diffused across localities and be absorbed by the entire economy—not locally. Consequently, the unemployment rate, probability of being employed in the domestic market, and wages in the West Bank will be the same for all localities—$u_l.post = u$, $P_{l.post} = P_{post}$, and $W_{l.Post} = W_{wb.post} \leq W_{wb}$. Additionally, as a result, the post payoff will be identical across all localities, $V_{l.post}^d = V_{post}^d$, which is indeed less than $V_l^d, \forall l$. In other words, the expected probability of being a school dropout declines after the shock for the whole economy.

Result 1 *Let $\Delta\pi_l = \pi_{l.post} - \pi_l$ be the change in the probability of a school dropout in the locality l after the shock. Subsequently, (a) $\Delta\pi_l < 0$ and (b) the absolute value of $|\Delta\pi_l|$ will be larger in localities with a high commuting share than that in localities with low commuting shares, before the shock.*

Suppose that there are two localities—one with high commuting shares H and the other with low commuting shares L before the shock. Result (1) can be directly obtained by noticing that $V_H^d > V_L^d$ and $V_{H.post}^d = V_{L.post}^d = V_{post}^d = P_{post} W_{wb.post}$.

Case 2: segmented local labor markets. Immediately after the outbreak of the Second Intifada, Israel not only closed its labor market for Palestinian workers, but also severely restricted mobility within the West Bank by closing and destroying roads, placing numerous checkpoints (permanent

[20] This assumption greatly simplifies our analysis by treating labor markets for skilled and unskilled separately, but it will not change the qualitative predictions of our model.

[21] By invoking the law of large numbers, we use the probability of school dropout and the rate of dropout interchangeably.

[22] As shown above, the probability of being employed in Israel decreased more in the locality with a high commuting share when compared to localities with a low commuting share, prior to the Second Intifada. Hence, our assumption of complete closure effectively captures this fact and simplifies the analysis without driving or altering it.

and arbitrary), and imposing regular curfews. Commuting between localities became very costly—workers moving from one locality to another had to endure hours of delays on checkpoints and used lengthy and very risky alternative routes. In effect, the asymmetric unskilled labor supply shock was largely absorbed by local markets, as shown by Mansour (2010). Suppose there is zero internal mobility, then $P_{l.post} = P_l$ and $u_{l.post} = P_{ls}$. In this scenario, returning commuters are fully absorbed by their locality; this leads to an exact rise in the unemployment rate in that locality by the share of commuters before the shock.[23] The post-shock wages in the locality l, $W_{l.post} \leq W_{wb}$, are expected to decrease more in localities with high commuting shares than in localities with low commuting shares.

Result 2 *Under segmented labor market, localities with high commuting shares, before the Second Intifada, experienced a higher decline in the probability of school dropout than the dropout in localities with low commuting shares.*

Again, let us proceed with the example of the two localities to establish this result. First, it must be noted that $V_H^d > V_L^d$, as in the free mobility case. However, the post-shock payoffs of leaving school are not identical across localities in the case of restricted internal mobility; specifically, $V_{H.post}^d \leq V_{L.post}^d$. A school dropout in locality H has a lower probability of being employed than a school dropout from locality L—$P_{H.post} < P_{L.post}$ or equivalently $u_{H.post} > u_{L.post}$. Until $W_{H.post} \leq W_{L.post}$, which must be true given that locality H received a larger labor supply shock than that of locality L (equality in case of rigid wages in short-medium run), the post shock payoffs from being out of school in H is lower than that of L—$V_{H.post}^d \leq V_{L.post}$. Additionally, given that $\pi_H > \pi_L$, it can be clearly shown that the probability of being a school dropout declines more in locality H when compared to that of locality L—$|\Delta\pi_H| \geq |\Delta\pi_L|$. This result is the theoretical counterpart of the empirical findings achieved using DD estimators.

This simple model illustrates an interesting and important result that is as follows: the closure of Israeli labor market is expected to have a differential impact on schooling choices across localities with different prior commuting shares, regardless of the assumption about the internal labor mobility within the West Bank. However, the response of schooling choices to the shock is large in the case of segmented markets. The *expected commuting* channel operates in the both cases, whereas *increased competition* contributes to the divergent path of educational choices between localities only under restricted mobility case. As discussed above, internal mobility was severely restricted after the Second Intifada; however, we suspect that a small fraction of male workers commuted to other localities during the investigated period. Therefore, the DD estimates obtained in the empirical section, $DD \equiv \Delta\pi_H - \Delta\pi_L$, is larger than that obtained for free mobility but smaller than that for segmented markets.

[23] It must be noted that we assume zero growth rates in response to the shock. In reality, however, economic activities and domestic employment had severely declined during the initial years of the Intifada. Apart from the size of the labor shock, the devastating effects of the Second Intifada is not locality-specific or uncorrelated with the share of commuters before the shock; thus, our analysis is robust to this assumption.

5.1 Employment Prospects for Youth Dropouts

An important insight from this simple model is that the size of the change in π_l is linked (proportional) to a change in the probability of being employed for school dropouts, ΔE_l,[24] or, equivalently, the change in probability of being a school dropout in locality l is proportional to the change in the probability of being unemployed—$\Delta u_l = u_{l.post} - u_l$.

$$\Delta\pi_l \propto \Delta u_l = \begin{cases} P_{ls} & \text{under zero mobility} \\ \frac{L_s}{L_{wb}} & \text{under free mobility} \end{cases}$$

where L_s and L_{wb} denote the total number of commuters and total employed unskilled workers in the West Bank. The unemployment rate increased more in locality H than in L, but only under limited mobility within the West Bank. Given the separation between the skilled and unskilled labor markets, this simple framework suggests that high school dropouts (unskilled workers) face a higher probability of being unemployed in localities that used to export a large fraction of its unskilled workers to Israel. Empirically, if the employment potential is a true potential mechanism, then the unemployment rate (probability) for youth dropouts after the Second Intifada would have increased more in localities with high commuting shares. Additionally, finding differential unemployment rate paths across localities after (but note before) the Second Intifada is an important piece of evidence supporting limited migration and mobility within the West Bank.

To empirically test for the employment potential mechanism, we use a modified version of our preferred DD model, as in Eq.(1). The dependent variable is a discrete choice outcome that takes a value of one in the case of an unemployed high school dropout, and zero in the case of an employed high school dropout. 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 (i.e., $\Delta u_H - \Delta u_L$). The results are reported in Column (1) of Table (10). Consistent with our prediction, the impact of the closure on youth unemployment is positive, large, and very precise. The unemployment probability for youth dropouts had increased by almost 13 percentage points more in localities at the 75th percentile of the commuting share distribution when compared to the localities at the 25th percentile of the distribution. Column (6) of the same table confirms that the closure did not have a statistically differentiated effect on the probability of being employed for youths with some educational degree (skilled youth) across localities.[25] The empirical results in this section provide strong evidence that restrictions on commuting to Israel had a differential impact on the employment prospects of school dropouts, and thus on the opportunity cost of attending school.

[24] To derive this, it must be noted that

$$\Delta\pi_l = e^{V^c} \frac{e^{V^d_{l.post}} - V^d_l}{(e^{V^c} + e^{V^d_l})(e^{V^c} + e^{V^d_{l.post}})}$$

Substitute for $V^d_l = P_{ls}W_s + P_l W_{wb}$, $V^d_{l.post} = P_{l.post}W_{wb}$, assuming no change in domestic wages, the result follows.

[25] This result further supports our modeling choices of keeping the payoffs for educated, V^c, constant before and after the shock. Certainly, the change in, V^c, over time will not invalidate our model as long as the change is uncorrelated with the share of commuters in 1999, as shown in our empirical results.

Figure (9) shows that the unemployment rates followed a statistically similar path for localities with high and low commuting shares, prior to the shock.[26]

If the prospect of employment is the true mechanism linking the closure of Israeli labor market to the youth schooling choices in the West Bank, then it should only affect the male cohort. Both channels outlined in the above-theoretical model, *increased competition* and *expected commuting*, are expected to mostly affect males, not females (98% commuters are males and were mostly employed in low skill jobs—construction and manufacturing). We separately estimate the DD model by using the unemployment dummy as a dependent variable for the male and female samples. The results show that the impact of the closure on the probability of being unemployed for male youth dropouts is positive, large, and very precise, as reported in Column (2) of Table (10). The number of female youth school dropouts participating in the labor force is very small in our sample, making it difficult to estimate the DD model for females only. To overcome this issue, we extend the definition of unemployment to those out of the labor force and re-estimate the model for the entire youth sample, males, and females, separately. The results are reported in Columns (3)–(5) of Table (10). The results clearly show that the closure had a differential impact on the prospect of non-employment across localities for male dropouts only. As column (5) demonstrates, the change in the probability of being employed for female dropouts before and after the shock is almost identical in localities with high and low commuting shares in 1999.

This finding provides 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. However, this finding is restricted to the limitation of the sample size and the extended definition of unemployment. Fig.(10) confirms the non-existence of a differential pre-trend of discouraged unemployment trajectories.[27]

5.2 Other Potential Mechanisms

Skill premium: Conditional on being employed, restrictions on commuting to Israel could potentially lead to a differential impact on returns to education (skill premium) by having a differential impact on wages for unskilled workers across localities. The wages for unskilled workers decline through the following two channels: (1) forgone lucrative wages in Israel (expected commuting effect) and (2) a lower locality-specific wage (increased competition effect). Importantly, the degree to which these channels impact the locality-expected wage for unskilled workers depends on the locality's commuting share in 1999. The first channel is clearly captured by the theoretical model under the free mobility restriction. While the change in employment prospects (or probability of being unemployed) before and after the shock is identical for youth living in localities with different commuting shares, the payoffs (expected wage) of being a dropout, V_l^d, declines more for localities with high commuting shares; this is because the forgone expected wage in Israel $P_{ls}W_s$ is larger when compared to localities with low commuting shares. In case of the segmented local market, in addition to the forgone wage channel, the locality-specific wage is expected to decline

[26] The results are robust to the inclusion of the locality linear trend.

[27] Again, the results are robust to the inclusion of the locality linear trend.

more in localities that received a higher unskilled labor supply shock.[28]

We estimate a DD wage model, as in Eq.(1), wherein the dependent variable is the logarithmic daily wage. Unfortunately, the sample size of skilled youth is too small to fit into our estimation strategy. Alternatively, we expand the sample to include all workers aged between 15 and 64 years. We estimate the model for unskilled- and skilled workers and by gender separately. The results are presented in Table (11). Commuting restrictions had a differential negative impact on unskilled workers across localities, as expected by the model (Column 1), and, importantly, this differential effect is only statistically significant for unskilled males (Columns 3 and 5). Columns (2), (4), and (5) reveal that the closure of Israeli labor market did not have a differential impact on skilled workers in different localities, regardless of the gender and commuting share. For comprehensiveness, we estimate the DD wage model of the youth school dropout rates in Column (7). The results show no differential impact of commuting restrictions on wages for youth dropouts in different localities, but this could be due to small sample size. To test for the *increased competition* effect alone, we estimate the DD wage model of non-commuters only—the results are qualitatively identical to the whole sample analysis (results not shown). An important caveat of the current DD wage analysis is that our sample weakly supports the non-existence of the differential pre-trend in log wages. As a result, we tend to frame our results as suggestive evidence of the effect of the skill-wage premium, without emphasizing it as a potential causal mechanism.

Public sector employment: Employment in the public sector has increased substantially in the years after the Second Intifada. It is possible that the PA expanded public employment more in localities with high commuting shares before the Second Intifada to mitigate the 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. The Graph (b) of Fig.(11) plots the coefficient of the interaction terms of the event analysis, as in Eq.(2). The dependent variable takes one if the individual is employed in the public sector and zero otherwise. The closure had a statistically similar impact on the change of the probability of being employed in the public sector for youths in different localities; hence, this mechanism is unlikely to be driving our results.[29]

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. The average in-

[28] It is worth mentioning that our model does not explicitly model the response of local wages to the commuting restriction. The only restriction that our model imposes is that the local wages after the shock must be less than or equal to the pre-shock wage. For the increased competition to have its effect through wages, the decline in wage should be increasing in the size of unskilled labor supply shock, which is a plausible theory-supported assumption.

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

come of a household is expected to decline disproportionately more in hard-hit localities due to a larger loss in wage income for return commuters, lower locality wage, and a smaller probability of employment—*increased competition* and *expected commuting* effects. 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 one if the household head is unemployed and zero otherwise. The event analysis is shown in Graph (a) of Fig.(11). 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 (Maio and Nisticó, 2016).

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, Maio, and Miaari, 2014), there is negligible evidence that the intensity of violence is correlated with the share of commuters before the Second Intifada for protracted periods.[30] 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 there is a negligible impact of violence, measured by the number of fatalities, on locality labor markets. This can be attributed to the sporadic nature of violence, especially after 2002.

Brück et al. (2014) show that violence decreases academic performance but not school dropout rates at the school- and locality level in the West Bank. 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, and relatively non-violent periods considered 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 fatalities in Eq.(1), as shown in Table (A.5) in the Online Appendix.

6 Conclusions

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

[30] Miaari et al. (2014) show that the 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. (2014) show that the level of violence is not correlated with economic and labor market conditions in the West Bank, and its time-space variation is as good as random.

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

To the best of our knowledge, this study is the first to shed light on the long-run consequences of the dynamics between the Israeli-Palestinian labor markets by evaluating the response of educational attainment to conflict-induced disruptive shocks. This study is relevant for designing future policies that aim to increase human capital in developing countries. In designing education-enhancing policies, policymakers are advised to consider changes in neighboring labor markets. This is especially important in the case of countries that predominantly export unskilled workers.

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Table 1: **Characteristics of Commuters vs. Non-commuters in 1999**

Variables	Place of Work	
	West Bank	Israel
Years of education	9.7	8.9
High school degree or above	0.32	0.18
Bachelor degree of above	0.11	0.01
Higher education	0.01	0.00
Urban	0.38	0.26
Rural	0.48	0.63
Age	34	30
Daily wage	65	100

The table shows the mean values of the number of years of education, age, and daily wages of males workers by the place of work. It also shows the proportion of workers with high school degree or above, bachelor degree or above, and higher education by the place of work. The numbers corresponding to Urban and Rural show the proportion of workers coming from urban or rural areas by the place of work. The daily wages are measured by the Israeli Shekel. Data source: PCBS Labor Force Survey (PLFS).

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 empl.	0.12	0.07	0.42	0.13	0.25	0.18
Educational Attainment						
Dropout ratio for youth (male)	0.31	0.15	0.37	0.16	0.34	0.16
Dropout ratio for youth (female)	0.28	0.15	0.35	0.17	0.30	0.17
Years of schooling (all population)	7.9	1.07	7.35	0.76	7.68	0.99
Baseline locality characteristics						
Unemployment rate	0.10	0.07	0.13	0.09	0.11	0.08
Ratio of waged workers to total workers	0.62	0.15	0.68	0.12	0.65	0.14
Share of population younger than 16 years old	0.23	0.05	0.24	0.05	0.24	0.05
Urban areas	0.46	0.49	0.23	0.42	0.36	0.48
Rural areas	0.38	0.48	0.68	0.47	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 wage	70	13.8	83	15.5	75.6	15.5
No. of household members	7.5	1.3	7.8	1.2	7.6	1.3

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

Table 3: **The Effect of Commuting Restrictions on the Probability of Being Employed in Israel**

Dep.Variable Commuting to Israel dummy			
	(1)	(2)	(3)
Share of commuters 1999*Post	-0.545***	-0.513***	-0.280***
	(0.028)	(0.034)	(0.072)
Year FE	YES	YES	YES
Locality FE	YES	YES	YES
Dist-by-Year FE	YES	YES	YES
X1999*Post	NO	YES	YES
Ind. controls	NO	YES	YES
Locality trend	NO	NO	YES
N	29653	29653	29653

The dependent variable takes one if an employed male commutes to Israel and zero otherwise. Baseline localities-specific variables in 1999, $X1999$, include the unemployment rate, share of population below 16 years of age, share of wage workers in total employment, and rural and urban dummies (the reference group is camps). $Post$ is a dummy that equals one for observations in years 2001–2006 and zero for 1999 and 2000. 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.

Table 4: **The Effect of Commuting Restrictions on the School Dropout Rates of 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	-0.249***	-0.220***	-0.203***	-0.175***	-0.197**
	(0.044)	(0.054)	(0.050)	(0.052)	(0.079)
Unemployment 1999*Post		-0.105	-0.125	-0.0679	-0.172
		(0.098)	(0.092)	(0.096)	(0.164)
Urban dummy*Post		0.0629*	0.0482	0.0154	0.0172
		(0.033)	(0.031)	(0.026)	(0.054)
Rural dummy *Post		0.0423	0.0267	-0.00405	-0.0141
		(0.034)	(0.032)	(0.028)	(0.057)
Share population below 16(1999)*Post		-0.143	-0.175	-0.186	-0.327
		(0.201)	(0.197)	(0.206)	(0.311)
Share of waged empl. 1999*Post		0.0563	0.0587	0.0262	0.198
		(0.089)	(0.074)	(0.078)	(0.131)
Year FE	YES	YES	YES	YES	YES
Locality FE	YES	YES	YES	YES	YES
Dist-by-Year FE	YES	YES	YES	YES	YES
Ind. controls	NO	NO	YES	YES	YES
Locality trend	NO	NO	NO	NO	YES
N	19715	19715	19296	17470	17470

The dependent variable equals one if a youth is a school dropout and zero otherwise. The Share of commuters 1999 denotes the share of male workers in Israel to the total male employment by localities. Baseline localities-specific variables include the unemployment rate (Unemployment1999), share of population below 16 years of age(Share population below 16 (1999)), share of wage workers in total employment (Share of waged empl. 1999), and Rural and Urban dummies (the reference group is camps). $Post$ is a dummy that equals one for observations in years 2001–2006 and zero for 1999 and 2000. Individual controls are the years of education of the household's head, dummies for gender and age, number of household members under 16 years of age, number of household members, and education of the household head's spouse (only in the last two columns). We exclude all observations with less than 8 years of education or/and those who never attended a school. 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.

Table 5: **The Effect of Commuting Restrictions on the School Dropout Rates of Youth aged 15–19 Years By Gender**

Dep. Variable: School dropout dummy for youth aged 15-19					
	Males	**Males**	**Females**	**Females**	**Triple diff**
	(1)	(2)	(3)	(4)	(5)
Share of commuters 1999*Post	-0.276***	-0.285**	-0.0695	-0.0741	
	(0.076)	(0.116)	(0.061)	(0.101)	
Unemployment 1999*Post	-0.0241	-0.515*	-0.0646	0.285	
	(0.170)	(0.268)	(0.150)	(0.222)	
Urban dummy*Post	-0.0535	-0.0819	0.0992*	0.143**	
	(0.035)	(0.068)	(0.056)	(0.063)	
Rural dummy *Post	-0.0607	-0.148**	0.0653	0.141**	
	(0.038)	(0.073)	(0.052)	(0.065)	
Share population below 16(1999)*Post	-0.0467	-0.111	-0.405	-0.607	
	(0.299)	(0.479)	(0.252)	(0.403)	
Share of waged empl. 1999*Post	0.0357	0.343*	0.0491	0.0811	
	(0.105)	(0.181)	(0.115)	(0.199)	
Gender*Sh1999*Post					-0.229***
					(0.085)
Year FE	YES	YES	YES	YES	
Locality FE	YES	YES	YES	YES	
Dist-by-Year FE	YES	YES	YES	YES	
Ind. controls	YES	YES	YES	YES	YES
Locality Trend	NO	YES	NO	YES	
Gender-by-Year FE					YES
Gender-by-Locality FE					YES
Locality-by-Year FE					YES
N	9239	9239	8231	8231	17470

The dependent variable equals one if a youth is a school dropout and zero otherwise. The Share of commuters 1999 denotes the share of male workers in Israel to the total male employment in 1999 by localities. Baseline localities-specific variables include the unemployment rate (Unemployment1999), share of population below 16 years of age, share of wage workers in total employment, and Rural and Urban dummies (the reference group is camps). $Post$ is a dummy that equals one for observations in years 2001–2006 and zero for 1999 and 2000.Individual controls are the years of education of both parents (household's heads), age dummy, number of household members, and the number of household members under 16 years of age. We exclude all observations with less than 8 years of education or/and those who never attended a school. 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.

Table 6: **The Effect of Commuting Restrictions on the College Enrollment of Students Aged 19–22 Years**

Dep. Variable: College Enrollement Aged 19-22			
	All	**Males**	**Females**
	(1)	(2)	(3)
Share of commuters 1999*Post	0.0546	0.186	-0.279
	(0.186)	(0.262)	(0.336)
Year FE	YES	YES	YES
Locality FE	YES	YES	YES
Dist-by-Year FE	YES	YES	YES
X1999Post	YES	YES	YES
Ind. controls	YES	YES	YES
Locality Trend	YES	YES	YES
N	5613	3072	2541

The dependent variable equals one if the student is in college or ever entered college and zero otherwise. The share of commuters 1999 denotes the share of male workers in Israel to total male employment in 1999 by localities. Baseline localities-specific variables include the unemployment rate (Unemployment1999), share of population below 16 years of age, share of wage workers in total employment, and Rural and Urban dummies (the reference group is camps). $Post$ is a dummy that equals one for observations in years 2001–2006 and zero for 1999 and 2000. Individual controls are the years of education of both parents (household's heads), age dummy, number of household members, and the number of household members under 16 years of age. We exclude all observations with less than 8 years of education or/and those who never attended a school. 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.

Table 7: **The Effect of Commuting Restrictions on the Youth School Dropout Rate: Binary Treatment**

	1 if Sh1999 $>$ median			**1 if Sh1999 $>$ dec70, 0 if Sh1999 $<$ dec30**		
	All	**Males**	**Females**	**All**	**Males**	**Females**
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment*Post	-0.0600***	-0.0987***	-0.0174	-0.0827***	-0.132***	-0.0271
	(0.019)	(0.027)	(0.025)	(0.027)	(0.042)	(0.034)
Year FE	YES	YES	YES	YES	YES	YES
Locality FE	YES	YES	YES	YES	YES	YES
Dist-by-Year FE	YES	YES	YES	YES	YES	YES
X1999Post	YES	YES	YES	YES	YES	YES
Ind. controls	YES	YES	YES	YES	YES	YES
N	17470	9239	8231	11155	5933	5222

The dependent variable equals one if a youth is a school dropout and zero otherwise. For columns (1–3), treatment equals 1 if the share of commuters in 1999 is larger than the median commuting share in 1999 and zero otherwise. In columns (4–6), Treatment equals one if the share of commuters is at the 70th percentile and zero if below the 30th percentile of the distribution of the commuting share in 1999, dropping all other localities. Baseline localities-specific variables in 1999, $X1999$, include the unemployment rate, share of population below 16 years of age, share of wage workers in total employment, and Rural and Urban dummies (the reference group is camps). $Post$ is a dummy that equals one for observations in years 2001–2006 and zero for 1999 and 2000. Individual controls are the years of education of the household's heads, dummies for age and gender, number of household, and the number of household members under 16 years of age. We exclude all observations with less than 8 years of education or/and those who never attended a school. 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.

Table 8: **Placebo: The Effect of Commuting Restrictions on the School Dropout Rates of Children and Adults**

	School dropout dummy aged 10-14			School dropout dummy aged 25-30		
	All	**Males**	**Females**	**All**	**Males**	**Females**
	(1)	(2)	(3)	(4)	(5)	(6)
Share of commuters 1999*Post	-0.0138	-0.0288	0.00251	-0.0787	-0.141	-0.0109
	(0.016)	(0.024)	(0.019)	(0.100)	(0.135)	(0.095)
Year FE	YES	YES	YES	YES	YES	YES
Locality FE	YES	YES	YES	YES	YES	YES
Dist-by-Year FE	YES	YES	YES	YES	YES	YES
X1999Post	YES	YES	YES	YES	YES	YES
Ind. controls	YES	YES	YES	YES	YES	YES
N	27193	14000	13193	12228	6244	5984

The dependent variable equals one if an individual is a school dropout and zero otherwise. The Share of commuters 1999 denotes the share of male workers in Israel to the total male employment by localities. Baseline localities-specific variables in 1999, $X1999$, include the unemployment rate; the share of population below 16 years of age, out of total population; the share of wage workers in total employment; and rural and urban dummies (the reference group is camps).$Post$ is a dummy that equals one for observations in years 2001–2006 and zero for 1999 and 2000. The individual variables for children include the age dummies, size of the household, number of household members below 16 years of age, and years of education of parents (household's heads). For adults, we control for the age and sex dummies, the household size, and the number of household members below 16 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.

Table 9: **IV estimation: The effect of commuting restrictions on the school dropout rates**

Dep. Variable: School dropout dummy for Youth aged 15-19					
	(1)	(2)	(3)	(4)	(5)
	IV	Reduced	OLS	IV-male	IV-female
Share of commuters 1999*Post	-0.372**		-0.162***	-0.549**	-0.124
	(0.190)		(0.055)	(0.238)	(0.291)
Distance*Post		-1.698**	-0.960		
		(0.657)	(0.721)		
Year FE	YES	YES	YES	YES	YES
Locality FE	YES	YES	YES	YES	YES
Dist-by-Year FE	YES	YES	YES	YES	YES
X1999Post	YES	YES	YES	YES	YES
Ind. controls	YES	YES	YES	YES	YES
N	17219	17219	17219	9124	8095
First Stage Statistics					
Distance*Post	4.56**				
	(1.87)				
Kleibergen-Paap weak identification Wald F statistic	5.92				

The dependent variable equals one if a youth is a school dropout and zero otherwise. The Share of commuters 1999 denotes the share of male workers in Israel to the total male employment in 1999 by localities. Distance denotes the inverse of the log distance. Baseline localities-specific variables, $X1999$, include the unemployment rate (Unemployment1999), share of population below 16 years of age, share of wage workers in total employment, and Rural and Urban dummies (the reference group is camps). $Post$ is a dummy that equals one for observations in years 2001–2006 and zero for 1999 and 2000. Individual controls are the years of education of both parents (household's heads), age and gender dummies, number of household members, and the number of household members under 16 years of age. We exclude all observations with less than 8 years of education or/and those who never attended a school. 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. Stock and Yogo (2005) present a weak identification test for critical values (under i.i.d assumption) ranging from 5.53 to 16.36

Table 10: **Causal Mechanism (Employment Potential): Unemployment of Youth Aged Between 15 and 19 Years**

dependent var.	**Unemployment dummy** (dropout)		**Dis. unemp. dummy** (dropout)			**Unemp.dummmy**
	All	**Males**	**All**	**Males**	**Females**	**Degree**
	(1)	(2)	(3)	(4)	(5)	(6)
Share of commuters 1999*Post	0.553***	0.577***	0.158*	0.350***	-0.0465	-0.100
	(0.134)	(0.134)	(0.089)	(0.130)	(0.073)	(0.980)
Year FE	YES	YES	YES	YES	YES	YES
Locality FE	YES	YES	YES	YES	YES	YES
Dist-by-Year FE	YES	YES	YES	YES	YES	YES
X1999Post	YES	YES	YES	YES	YES	YES
Ind. controls	YES	YES	YES	YES	YES	YES
N	2288	2170	4189	2607	1582	342

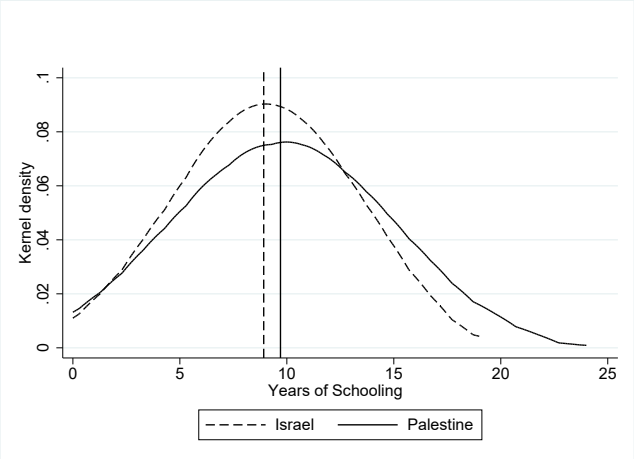
The dependent variable is an unemployment dummy that equals one in the case of unemployed youth dropouts, and zero in the case of employed youth dropouts (columns 1 and 2). The discouraged unemployment dummy equals one for unemployed youth dropouts and out of the labor force and zero for employed for youth dropouts (columns 3-5). Column 6 only includes youths with some educational degree; additionally, the dependent variable is as defined in columns 1 and 2. We do not include the regression for female youth dropouts, with unemployment as dependent variable, due to a small number of observation (low labor force participation). The Share of commuters 1999 denotes the share of male workers in Israel to the total male employment by localities. Baseline localities-specific variables in 1999, $X1999$, include the unemployment rate, share of population below 16 years of age, share of wage workers in total employment, and Rural and Urban dummies (the reference group is camps). $Post$ is a dummy that equals one for observations in years 2001–2006 and zero for 1999 and 2000. Individual controls are the years of education of the household head's and age dummies. We exclude all observations with less than 8 years of education or/and those who never attended a school. 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.

Table 11: **Causal Mechanism (Skill-wage Premium)**

	Dep. Variable: log wage						
	All sample		**Males**		**Females**		**Youth Dropout**
	unskilled	skilled	unskilled	skilled	unskilled	skilled	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Share of commuters 1999*Post	-0.129**	0.0204	-0.115**	0.0492	0.0922	0.0515	0.185
	(0.055)	(0.071)	(0.057)	(0.098)	(0.376)	(0.217)	(0.218)
Year FE	YES	YES	YES	YES	YES	YES	YES
Locality FE	YES	YES	YES	YES	YES	YES	YES
Dist-by-Year FE	YES	YES	YES	YES	YES	YES	YES
X1999Post	YES	YES	YES	YES	YES	YES	YES
Ind. controls	YES	YES	YES	YES	YES	YES	YES
N	13304	6007	11996	3813	1308	2194	885

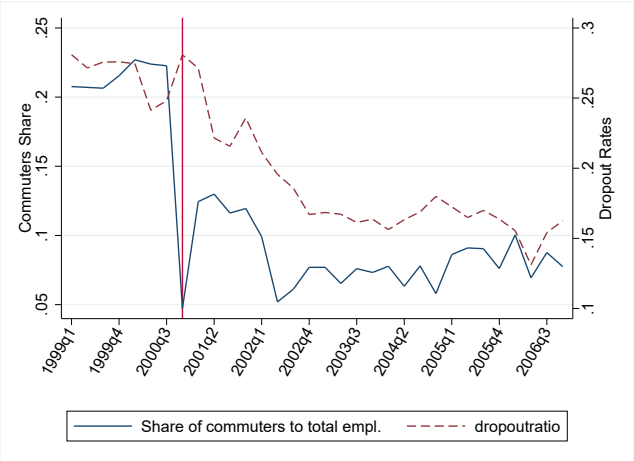
Dependent variable is the logarithm of daily wages. The Share of commuters 1999 denotes the share of male workers in Israel to the total male employment by localities. Baseline localities-specific variables, $X1999$, include the unemployment rate; the share of population below 16 years of age, out of the total population; 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 years 2001–2006 and zero for 1999 and 2000. Individual controls are the years of education, age, sex, and industry dummies. We exclude all observations with less than 8 years of education or/and those who never attended a school. We drop observations at the top 1 and bottom 1 percentile of wage distribution. 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.

Figure 1: **The Distribution of Years of Schooling for Male Workers by the Place of Work in 1999**



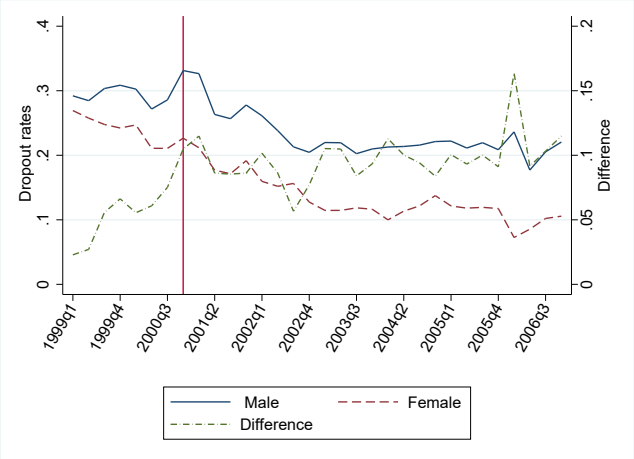
Note: The distributions of years of schooling for males working in Israel and the West Bank in 1999 are shown by the dashed- and solid distributions, respectively. The dashed and solid vertical lines denote mean years of education for workers in Israel and West Bank, respectively. Data source: PCBS: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

Figure 2: **The Share of Commuters and Dropout Ratios (West Bank)**



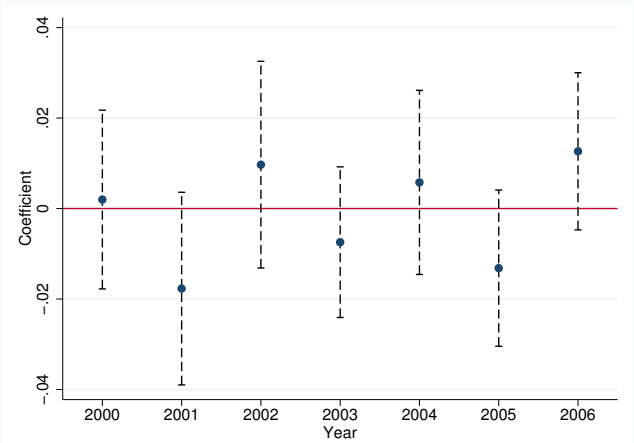
Notes: The solid and dashed lines show the quarterly share of commuters in the total employment and the ratio of school dropouts for youths aged between 15 and 19 years, respectively. The vertical line indicates the beginning of the Second Intifada. Data source: Palestinian Labor Force Survey (PLFS).

Figure 3: **Youth High School Dropouts by Gender**



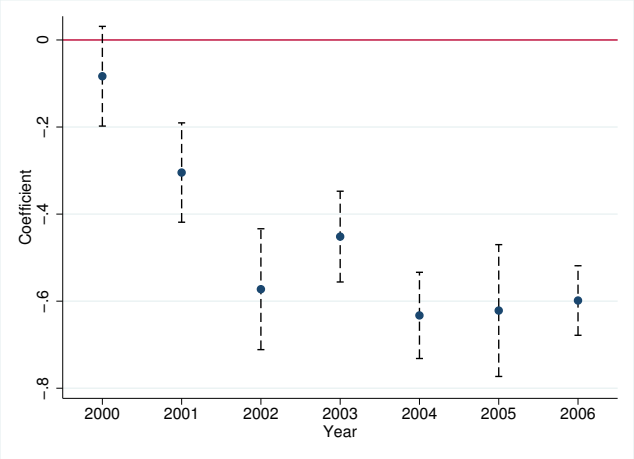
Note: The solid and dashed lines show school dropout rates of boys and girls aged between 15 and 19 years, respectively. The third line traces the educational attendance gap between females and males. The vertical line indicates the beginning of the Second Intifada. Data source: Palestinian Labor Force Survey (PLFS).

Figure 4: **The Impact of Closure on Predicted High School Dropout**



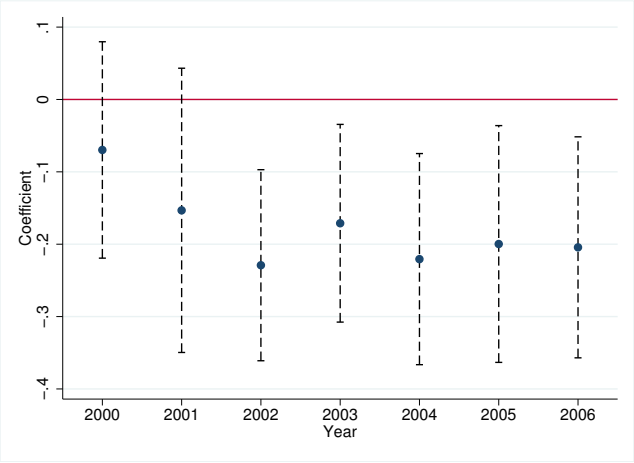
Notes: The dependent variable is the predicted school dropout dummy. The graph plots the coefficients of the interaction of the shares of commuters in the total employment, $S1999$, with the corresponding year's dummy from the regression that uses locality and district-by-year dummies, locality baseline characteristics in 1999 interacted with the corresponding year's dummy, and individual characteristics. The vertical lines represent the 95% confidence interval of each of the estimates. The coefficient of the interaction of $S1999$ and Year 1999 dummy is normalized to zero to identify the model. The probability weights provided by PCBS are used in the regression.

Figure 5: **The Impact of Closure on Work in Israel**



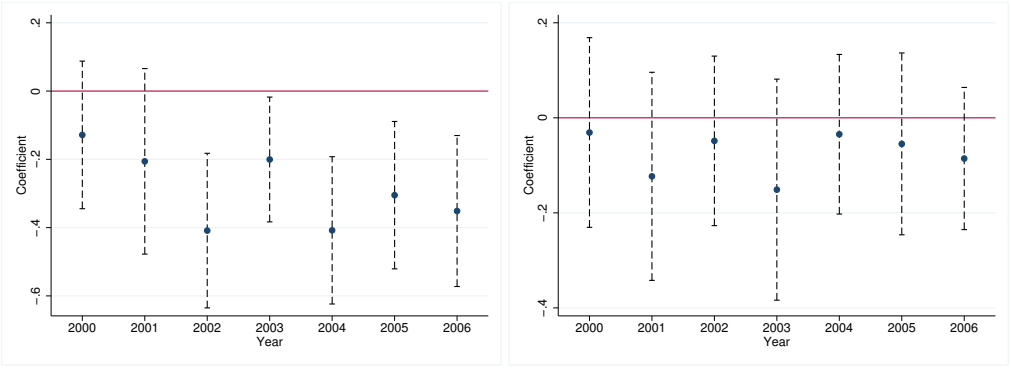
Notes: The dependent variable is the dummy for employment in Israel (1 for employed in Israel) for all males in the sample. The graph plots coefficients of the interaction of the shares of commuters in total employment, $S1999$, with the corresponding year's dummy from the regression that uses locality and district-by-year dummies, locality baseline characteristics in 1999 interacted with the corresponding year's dummy, and individual characteristics. The vertical lines represent the 95% confidence interval of each of the estimates. The coefficient of the interaction of $S1999$ and Year 1999 dummy is normalized to zero to identify the model. The probability weights provided by PCBS are used in the regression.

Figure 6: **The Impact of Closure on High School Dropout for Youth**



Notes: The dependent variable is school dropout dummy for youth (1 for dropout). The graph plots coefficients of the interaction of the shares of commuters in the total employment, $S1999$, with the corresponding year's dummy from the regression that uses locality and district-by-year dummies, locality baseline characteristics in 1999 interacted with the corresponding year's dummy, and individual characteristics. The vertical lines represent the 95% confidence interval of each of the estimates. The coefficient of the interaction of $S1999$ and Year 1999 dummy is normalized to zero to identify the model. The probability weights provided by PCBS are used in the regression.

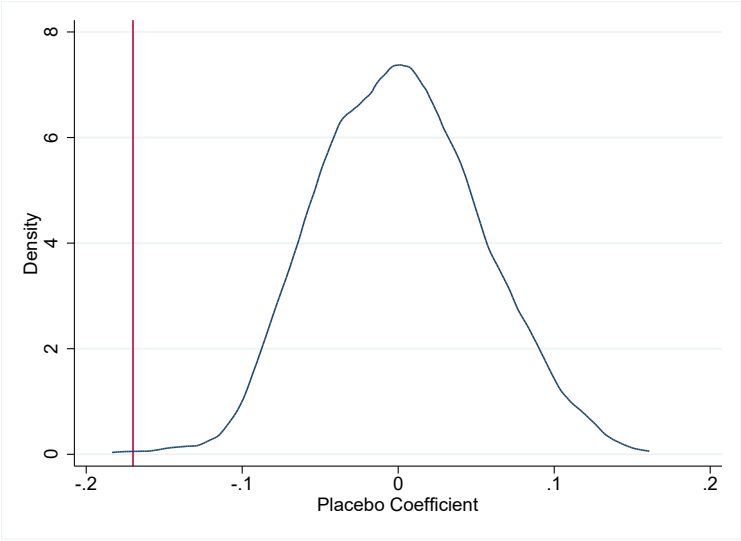
Figure 7: **The Impact of Closure on Youth Dropout by Gender**



(a) Males (b) Females

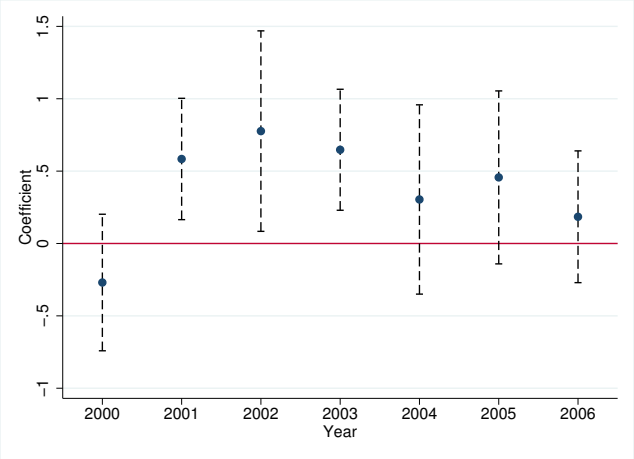
Notes: The graph (a) uses the male-observations, whereas the graph (b) uses only female-observations. The dependent variable is the school dropout dummy for youth (1 for dropout). The graphs plot coefficients of the interaction of the shares of commuters in total employment, $S1999$, with the corresponding year's dummy from the regression that uses locality and district-by-year dummies, locality baseline characteristics in 1999 interacted with the corresponding year's dummy, and individual characteristics. The vertical lines represent the 95% confidence interval of each of the estimates. The coefficient of the interaction of $S1999$ and Year 1999 dummy is normalized to zero to identify the model. The probability weights provided by PCBS are used in all regression.

Figure 8: **Falsification Test**



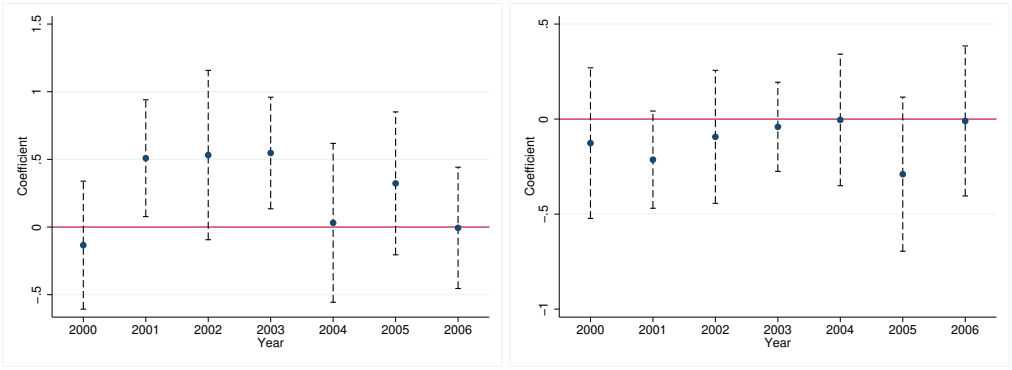
Notes: The figure plots the probability density function of the coefficients of the interaction term $Share1999^{placebo} \times Post$, which is obtained by estimating the Eq.(1) with placebo commuting shares in 1999, $Share1999^{placebo}$; this estimation is conducted 2,000 times. The vertical line indicates the point estimate reported in Table.(4), column (5).

Figure 9: **The Impact of Closure on Unemployment of Youth (Males)**



Notes: The dependent variable is an unemployment dummy (one for unemployed) for male dropouts. The graphs plot coefficients of the interaction of the shares of commuters in the total employment, $S1999$, with the corresponding year's dummy from the regression that uses locality and district-by-year dummies, locality baseline characteristics in 1999 interacted with the corresponding year's dummy, and individual characteristics. The vertical lines represent the 95% confidence interval of each of the estimates. The coefficient of the interaction of $S1999$ and Year 1999 dummy is normalized to zero to identify the model. The probability weights provided by PCBS are used in all regressions.

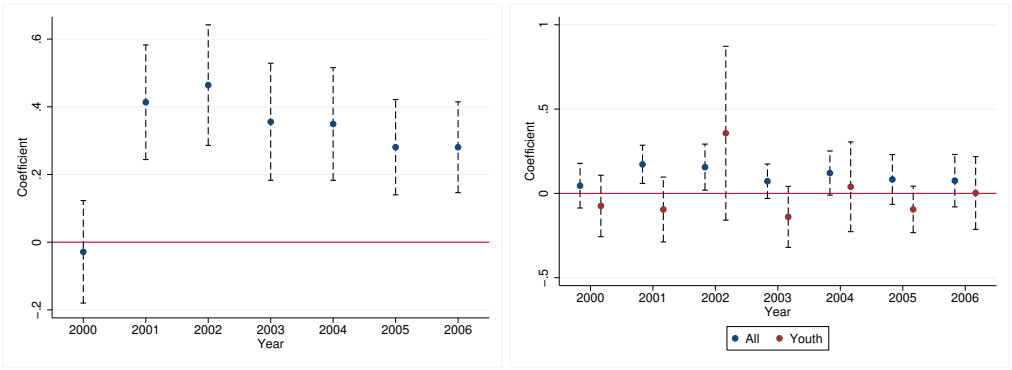
Figure 10: **The Impact of Closure on Discouraged Unemployment for Youth**



(a) Males (b) Females

Notes: The Graph (a) is for male dropout, whereas graph (b) is for females. The dependent variable is a discouraged unemployment dummy (one stands for unemployed or out of labor force and zero for employed). The graphs plot coefficients of the interaction of the shares of commuters in the total employment,$S1999$, with the corresponding year's dummy from the regression that uses locality and district-by-year dummies, locality baseline characteristics in 1999 interacted with the corresponding year's dummy, and individual characteristics. The vertical lines represent the 95% confidence interval of each of the estimates. The coefficient of the interaction of $S1999$ and Year 1999 dummy is normalized to zero to identify the model. The probability weights provided by PCBS are used in all regressions.

Figure 11: **The Impact of Closure on Father's Unemployment and Public Sector**



(a) Father unemployment (b) Public employment

Notes: The dependent variable in the graph (a) denotes the father's unemployment dummy (one for unemployed). In figure (b), the dependent variable denotes the public sector employment (one for employed in public sector). The graphs plot coefficients of the interaction of the shares of commuters in total employment, $S1999$, with the corresponding year's dummy from the regression that uses locality and district-by-year dummies, locality baseline characteristics in 1999 interacted with corresponding year's dummy, and individual characteristics. The vertical lines represent the 95% confidence interval of each of the estimates. The coefficient of the interaction of $S1999$ and Year 1999 dummy is normalized to zero to identify the model. The probability weights provided by PCBS are used in all regressions.

Online Appendix

A Additional Tables and Graphs

Table A.1: **Mean Daily Wages of Commuters in 1999 by Education Level**

Educational Attainment	**Daily wage**
Illiterate (3%)	87
Can read (10%)	94
Elementary (31.5%)	97
Secondary (36.5%)	100
highschool (13%)	110
Associate degree (4%)	110
Bachelor (1.5%)	100
Total (100%)	100

The table shows the average daily wages of male commuters in 1999 by the level of education. The percentage next to the educational level indicates the percentage of commuters with the corresponding educational level. Data source: PCBS Labor Force Survey (PLFS).

Table A.2: **Average Locality Percentage of Workers Commuting by District in 1999**

District	Share (%)	Std. Dev.	No. localities
Jenin	31	0.18	24
Tubas	25	0.08	6
Tulkarem	28	0.15	14
Nablus	12	0.10	20
Qalqilia	24	0.11	12
Salfeet	38	0.09	13
Ramallah	22	0.20	18
Jericho	22	0.17	8
Bethlahim	22	0.16	19
Hebron	27	0.21	31
Total	25	0.17	165

Table A.3: **The Effect of Commuting Restrictions on The School Dropout Rates: Dropping Localities at the 1th and 99th Percentile of Commuters' Share in 1999**

Dep. Variable: School dropout dummy for youth aged 15-19						
	All		**Males**		**Females**	
	(1)	(2)	(3)	(4)	(5)	(6)
Share of commuters 1999*Post	-0.217***	-0.233**	-0.325***	-0.311**	-0.0952	-0.125
	(0.056)	(0.092)	(0.081)	(0.135)	(0.067)	(0.113)
Year FE	YES	YES	YES	YES	YES	YES
Locality FE	YES	YES	YES	YES	YES	YES
Dist-by-Year FE	YES	YES	YES	YES	YES	YES
Ind. controls	YES	YES	YES	YES	YES	YES
Locality trend	NO	YES	NO	YES	NO	YES
N	17019	17019	8993	8993	8026	8026

The Share of commuters 1999 denotes the share of male workers in Israel to the total male employment by localities. Baseline localities-specific variables, $X1999$, include the unemployment rate; share of population below 16 years of age, out of the total population; share of wage workers in total employment; and rural and urban dummies (the reference group is camps). The results are also robust to inclusion of more locality characteristics. $Post$ is a dummy that equals one for observations in years 2001–2006 and zero for 1999 and 2000. Individual controls are the years of education of both parents. We exclude all observations with less than 8 years of education or/and those who never attended a school. 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.

Table A.4: **The Effect of Commuting Restrictions on School Dropout Rates: Dropping Localities with Less than 100 Observations**

Dep. Variable: School dropout dummy for youth aged 15-19						
	All		**Males**		**Females**	
	(1)	(2)	(3)	(4)	(5)	(6)
Share of commuters 1999*Post	-0.182***	-0.212**	-0.263***	-0.347***	-0.109	-0.0479
	(0.058)	(0.084)	(0.088)	(0.131)	(0.077)	(0.149)
Year FE	YES	YES	YES	YES	YES	YES
Locality FE	YES	YES	YES	YES	YES	YES
Dist-by-Year FE	YES	YES	YES	YES	YES	YES
Ind. controls	YES	YES	YES	YES	YES	YES
Locality trend	NO	YES	NO	YES	NO	YES
N	13818	13818	7304	7304	6514	6514

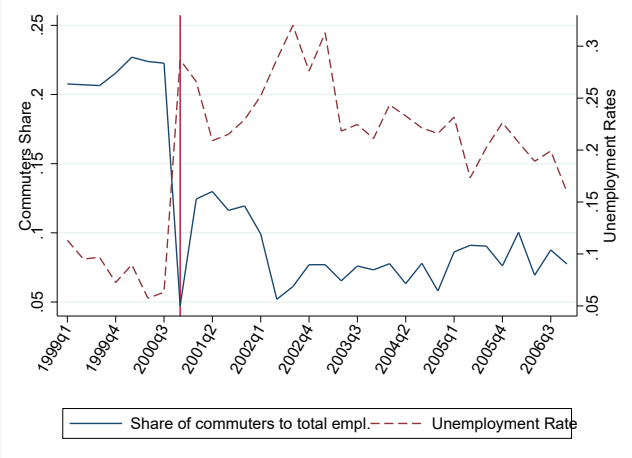
The Share of commuters 1999 denotes the share of male workers in Israel to the total male employment by localities. Baseline localities-specific variables, $X1999$, include the unemployment rate , share of population below 16 years of age, share of waged workers in total employment, and Rural and Urban dummies (the reference group is camps). $Post$ is a dummy that equals one for observations in years 2001-2006 and zero for 1999 and 2000. Individual controls are the years of education of both parents. We exclude all observations with less than 8 years of education or/and those who never attended a school. The probability weights provided by PCBS are used in all regressions. *p-value < 0.1; **p-value < 0.05; ***p-value< 0.01. Standard errors in parentheses are clustered at the locality level.

Table A.5: **The Effect of Commuting Restrictions on School Dropout Rates: Controlling for Locality-Year Variables**

Dep. Variable: School dropout dummy for youth aged 15-19				
	All		**Males**	**Females**
	(1)	(2)	(3)	(4)
Share of commuters 1999*Post	-0.235***	-0.162*	-0.286***	-0.105
	(0.085)	(0.089)	(0.076)	(0.116)
Year FE	YES	YES	YES	YES
Locality FE	YES	YES	YES	YES
Dist-by-Year FE	YES	YES	YES	YES
Ind. controls	YES	YES	YES	YES
Locality Controls	YES	YES	YES	YES
Locality trend	NO	YES	YES	YES
N	19285	17459	9232	8227

The Share of commuters 1999 denotes the share of male workers in Israel to the total male employment by localities. Baseline localities-specific variables, $X1999$, include the unemployment rate, share of population below 16 years of age, share of wage workers in total employment, and Rural and Urban dummies (the reference group is camps). $Post$ is a dummy that equals one for observations in years 2001–2006 and zero for 1999 and 2000. Individual controls are the years of education of the head of the household; it is reported in Column (1); additionally, the years of education of the household's head and his/her spouse in Columns (2)–(4). We exclude all observations with less than 8 years of education or/and those who never attended a school. The time-varying locality-specific controls include the locality's annual number of fatalities related to the Israeli-Palestinian conflict; the total population; the share of population, out of the labor force; and the unemployment level. The probability weights provided by PCBS are used in all regressions. *p-value < 0.1; **p-value < 0.05; ***p-value< 0.01. Standard errors in parentheses are clustered at the locality level.

Figure A.1: **The Share of Commuters and Unemployment Rates (West Bank)**



Notes: The solid and dashed lines show the quarterly share of commuters in the total employment and the unemployment rate, respectively. The vertical line indicates the beginning of the Second Intifada. Data source: PCBD: Labor Force Survey (PLFS).

Figure A.2: **The Share of Commuters and Skill-Wage Premium (West Bank)**

Notes: The solid and dashed lines show the quarterly share of commuters in total employment and skill-wage premium, respectively. The vertical line indicates the beginning of the Second Intifada. Skill-wage premium is calculated as the log wage difference between workers with college degree and non-college workers. Data source: PCBD: Labor Force Survey (PLFS).

Figure A.3: **Map of the West Bank**

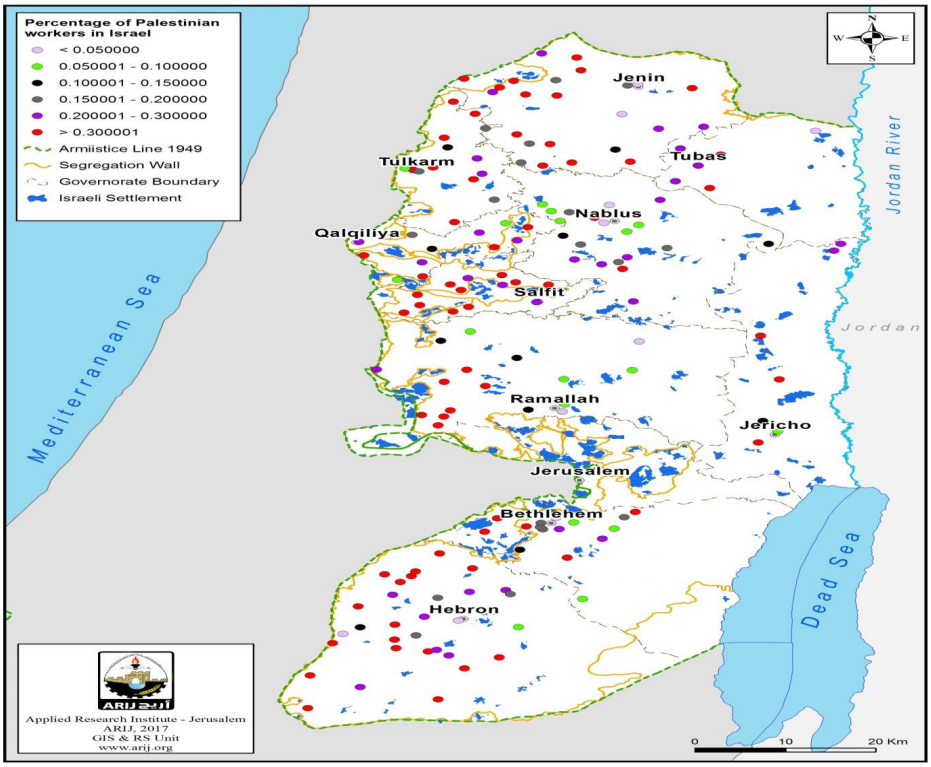
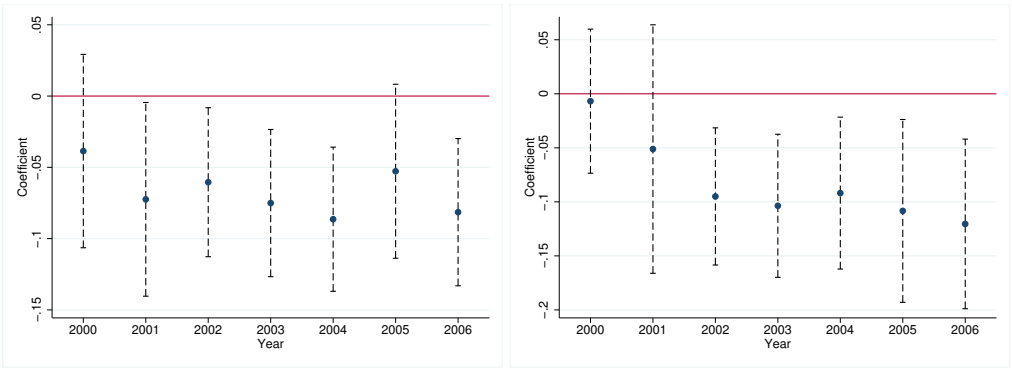


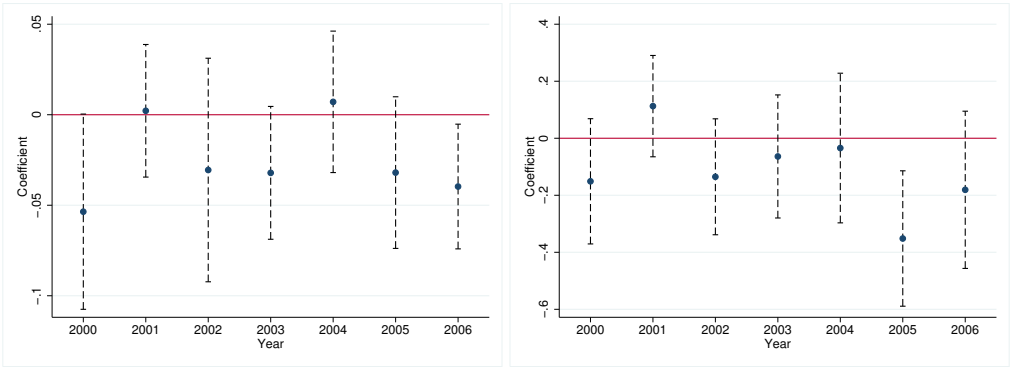
Figure A.4: **The Impact of Closure on High School Dropout: Binary Treatment**



(a) Median rule (b) 70th-30th rule

Notes: The dependent variable is the school dropout dummy of youth aged 15–19 years. The graph plots coefficients of the interaction of binary treatment dummy (median cutoff for figure (a) and above the 70th percentile - below 30th percentile treatment rule for figure b) with the corresponding year's dummy from the regression that uses locality and district-by-year dummies, locality baseline characteristics in 1999 interacted with corresponding year's dummy, and individual characteristics. The vertical lines represent the 95% confidence interval of each of the estimates obtained from clustered standard errors at the locality level. The coefficient of the interaction of $S1999$ and Year 1999 dummy is normalized to zero to identify the model. The probability weights provided by PCBS are used in the regression.

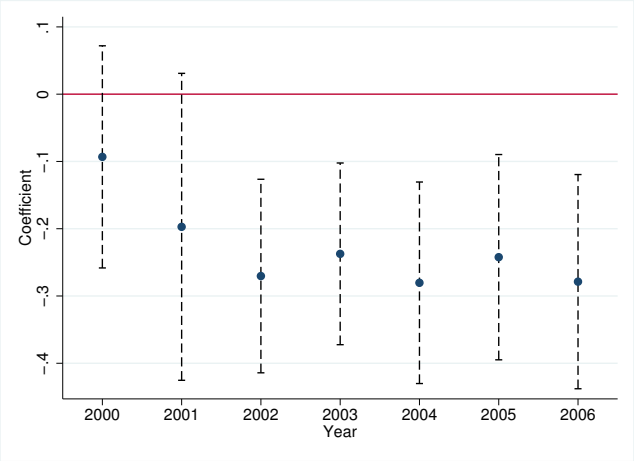
Figure A.5: **The Impact of Closure on Education: the Placebo Regressions**



(a) Children 10-14 (b) Adult aged 25-30

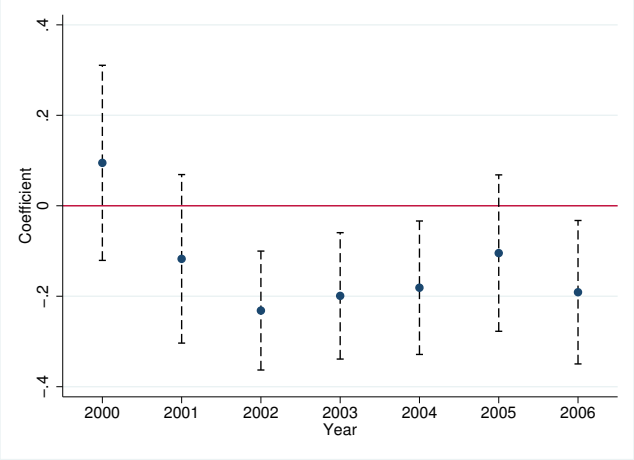
Notes: The dependent variable is the school dropout dummy for children aged 10–14 years for the figure (a) and figure(b)for adults aged 25–30 years. The graph plots coefficients of the interaction of the shares of commuters in total employment, $S1999$, with the corresponding year's dummy from the regression that uses locality and district-by-year dummies, locality baseline characteristics in 1999 interacted with corresponding year's dummy, and individual characteristics. The vertical lines represent the 95% confidence interval of each of the estimates obtained from clustered standard errors at the locality level. The coefficient of the interaction of $S1999$ and Year 1999 dummy is normalized to zero to identify the model. The probability weights provided by PCBS are used in the regression.

Figure A.6: **The Impact of Closure on Youth High School Dropout: Localities at the 1-99th Percentiles of Commuters Share Distribution**



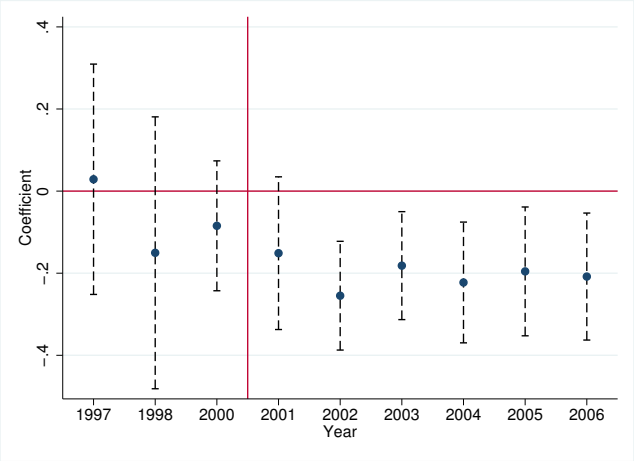
Notes: The dependent variable is the school dropout dummy for youth (1 for dropout). The graph plots coefficients of the interaction of the shares of commuters in the total employment, $S1999$, with the corresponding year's dummy from the regression that uses locality and district-by-year dummies, locality baseline characteristics in 1999 interacted with corresponding year's dummy, and individual characteristics. The regression only includes localities between the 1-99th percentiles of the distribution of commuters' share in 1999. The vertical lines represent the 95% confidence interval of each of the estimates. The coefficient of the interaction of $S1999$ and Year 1999 dummy is normalized to zero to identify the model. The probability weights provided by PCBS are used in the regression.

Figure A.7: **The Impact of Closure on Youth High School Dropout: Dropping Small Localities**



Notes: The dependent variable is a school dropout dummy for youth (1 for dropout). The graph plots coefficients of the interaction of the shares of commuters in total employment, $S1999$, with the corresponding year's dummy from regression that uses locality and district-by-year dummies, locality baseline characteristics in 1999 interacted with corresponding year's dummy, and individual characteristics. The regressions only include localities with more than 100 observations. The vertical lines represent the 95% confidence interval of each of the estimates. The coefficient of the interaction of $S1999$ and Year 1999 dummy is normalized to zero to identify the model. The probability weights provided by PCBS are used in the regression.

Figure A.8: **The Impact of Closure on Youth High School Dropout: Extended Datasets 1997–2006**



Notes: The dependent variable is a school dropout dummy for youth (1 for dropout). The graph plots coefficients of the interaction of the shares of commuters in total employment, $S1999$, with the corresponding year's dummy from regression that uses locality and district-by-year dummies, locality baseline characteristics in 1999 interacted with corresponding year's dummy, and individual characteristics. The vertical dashed lines represent the 95% confidence interval of each of the estimates. The coefficient of the interaction of $S1999$ and Year 1999 dummy is normalized to zero to identify the model. The solid vertical line separates the pre- and after-shock coefficients. The probability weights provided by PCBS are used in the regression.