Technical appendix to Hard Traveling

Histograms of dependent and independent variables

Figure 1 depicts the histograms of the primary dependent and independent variables used in the paper's main regressions. The reader will note a fairly healthy variation across all variables, implying the regression results are unlikely to be driven by outliers. The role of outliers is further explored in the tables below.

Figure 2 depicts the spatial histogram of percentage-point change in employment across the West Bank. The figure should assuage readers' concerns that the findings of this paper are driven by a general trend of economic relocation from peripheral to central areas of the West Bank. On the contrary, employment gains and losses seem to be scattered across the map, without obvious concentrations.

Robustness checks

This section contains extra robustness checks for the main regressions of the paper.

Robustness: alternative channels

What if the paper's main results are actually driven by migration flows of Palestinian laborers between the urban core and periphery? Or by differential rates in job creation across these areas? In Table 1 I reprint the main result in Column 1, then show results in

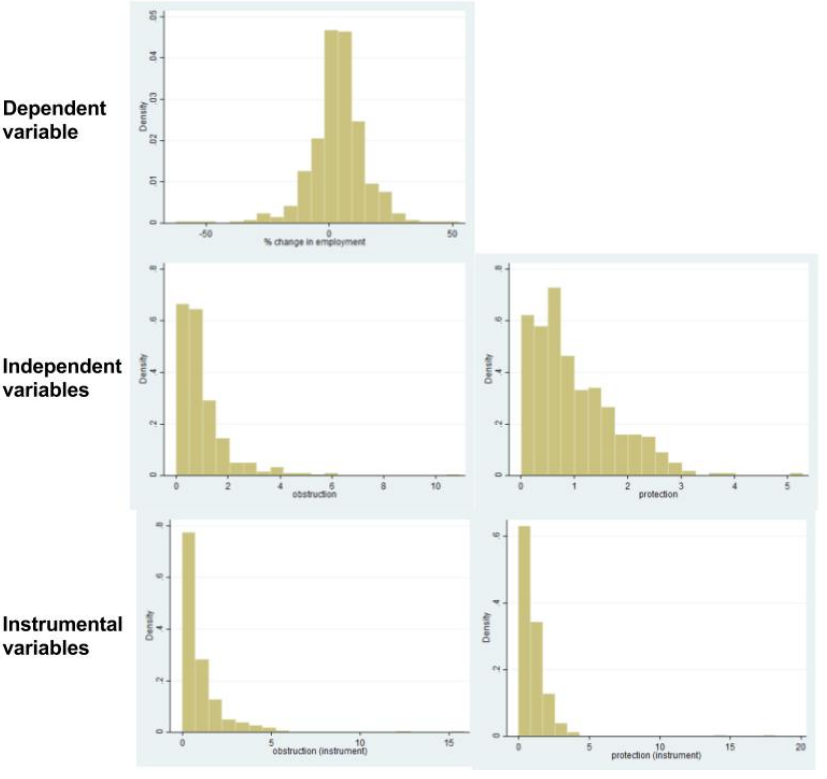

Figure 1. Histograms of the key dependent and independent variables

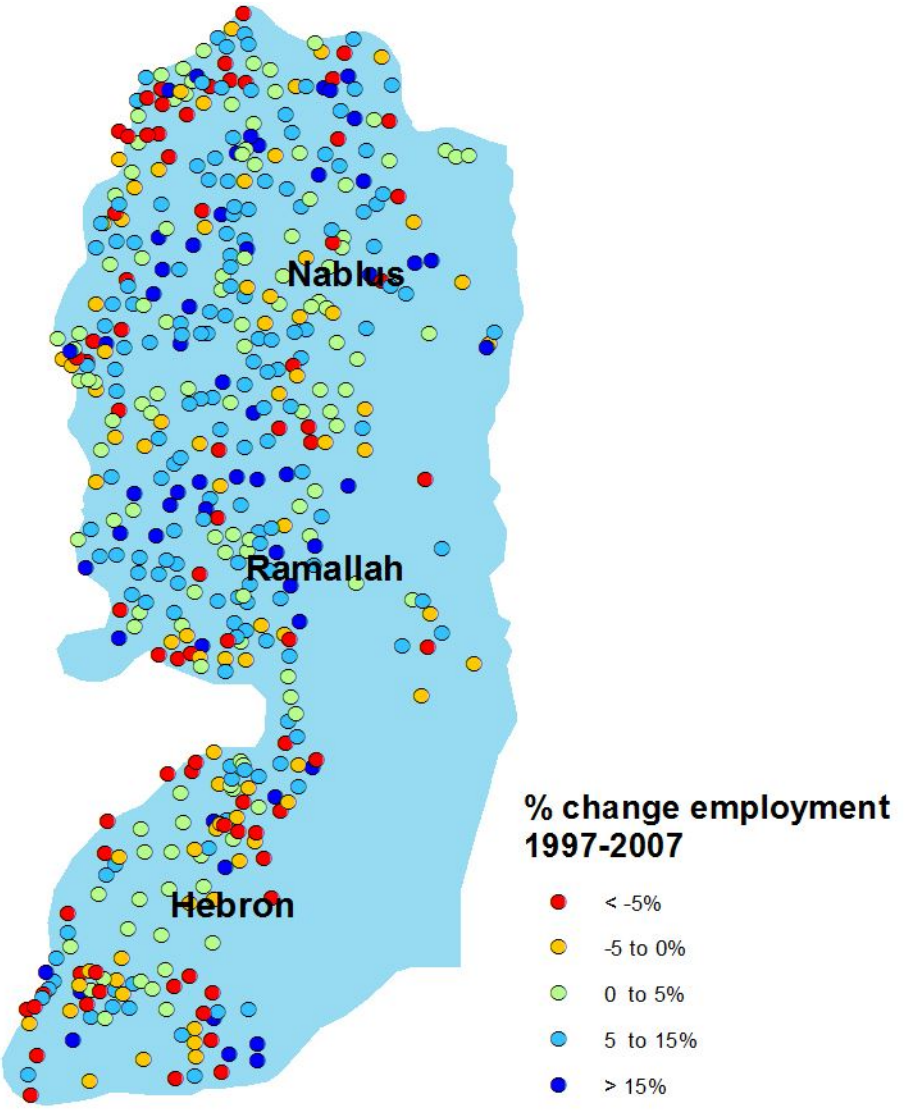

Figure 2. Percentage-point change in employment (△%employ) across West Bank Palestinian neighborhoods, 1997-2007. It appears employment rates did not obviously trend higher among core neighborhoods relative to peripheral neighborhoods over this time period.

TABLE 1 *Robustness to additional RHS covariates*

	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) 2SLS
$\triangle obstruction$	-3.756***	-3.774***	-3.676***	-3.979***	-3.901***
	(1.005)	(1.018)	(1.043)	(1.031)	(1.065)
$\triangle protection$	3.507**	3.472**	3.411**	3.645**	3.453**
	(1.560)	(1.577)	(1.560)	(1.571)	(1.596)
$\triangle \%net\_immigr$		-0.125	-0.108	-0.144	-0.151
		(0.122)	(0.125)	(0.127)	(0.126)
$\triangle net\_opening\_firms$		-0.0366	-0.0389	-0.0484	-0.0454
		(0.0654)	(0.0654)	(0.0666)	(0.0679)
$\triangle \%rent$		-0.0643	-0.0779	-0.0759	-0.0808
		(0.0561)	(0.0580)	(0.0587)	(0.0582)
$\triangle \%public\_employ$			0.107	0.0893	0.108
			(0.0931)	(0.0942)	(0.0926)
$\triangle \%work\_settlement$				0.175**	0.159**
				(0.0742)	(0.0768)
Governorate trends	Yes	Yes	Yes	Yes	Yes
Obst. proximity	Yes	Yes	Yes	Yes	Yes
Settle. proximity	Yes	Yes	Yes	Yes	Yes
Plstn. fatal. 2000-07	No	No	No	No	Yes
Observations	480	480	480	480	480
R-squared	0.523	0.525	0.528	0.528	0.556

Robust standard errors are clustered using 310 clusters determined by poverty mapping teams at the World Bank and PCBS. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Column 2 of adding the percentage net-immigration, percentage net-openings of firms[1]. According to PCBS officials, some laborers may have reported their home neighborhood as their neighborhood of origin despite living most of the working week near their workplace, and vise versa. To account for this possibility, I add to the regression the percentage change in incidence of renting, since these weekly commuters apparently tended to rent apartments in the towns where they worked. Comparing Column 1 with Column 2, the marginal effects of $\triangle obstruction$ and $\triangle protection$ are statistically unchanged, suggesting these alternative explanations do not interfere with the story.

What if the results are driven by PA employment decisions? Perhaps the PA expanded public sector jobs in a Keynesian effort to stimulate the economy. If public sector job opportunities grew more in core areas like Ramallah, this might affect the results. In Column 3 I add to Column 2's regression the percentage-point change in public employment $\%\triangle public$. Again, the point-estimates of interest remain unaffected.

What if the small fraction of Palestinian laborers who work on settlements (2.5%) is somehow responsible for the findings? In Column 4 I control for the percentage-point change in employment of Palestinian laborers on settlements. The point-estimates of interest rise slightly in magnitude and remain statistically significant.

Finally, despite the IV strategy, readers may retain concerns that violence directly affected employment trends (as in Miaari, Zussman, and Zussman (2014)), and that the spatial pattern of violence is strongly correlated with obstacle deployment, thus confounding the results. To assuage concerns, I access the same B'Tselem fatalities dataset used in Miaari, Zussman, and Zussman (2014) to obtain the per-capita count of Palestinian

[1]A firm census was conducted in 2004, so I obtain a count of open firms, and firms in the process of opening or closing. Per neighborhood I calculate percentage net-openings of firms as the number opening minus the number closing, divided by the sum of all firms

fatalities sustained by each neighborhood for each year since the start of the uprising until the post-uprising census (2000-2007). I include for each of these years the per-capita fatality counts in Column 5. As in Columns 1-4, the addition of these extra RHS variables does not appreciably reduce the magnitude or statistical significance of the point-estimates of interest.

Robustness: immobility of firms and laborers

Although I showed in 1 that the results are robust to firm openings and laborer relocations, it is interesting to know how obstacles affected these variables. To test for effects, I repeat Table 4's distributional checks, but change the dependent variable to be percentage net-immigration (Table 3) and percentage net-openings of firms (Table 2). While percentage net-immigration appears not to have responded obstruction from jobs, Table 3's results offer some weak evidence that laborers may have relocated away from protected neighborhoods. Since this neither matches the intuition of the theoretical framework, nor affects the point-estimates of interest in Table 1, I prefer not to speculate as to its interpretation.[2] Meanwhile, Table 2's results are even weaker, offering no consistent effect of obstacle deployment on percentage net-openings of firms. This result is completely consistent with the narrative, i.e. that the preexisting stock of unemployed laborers buffered firms from larger wage effects that obstacle deployment would have otherwise induced.

[2]The reader is further reminded that only 1.6% of laborers reported relocating during the entire 1997-2007 decade.

TABLE 2 *Dependent variable - % net firm openings*

	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) 2SLS	(6) 2SLS
$\triangle obstruction$	-0.659	-1.722**	-0.546	-0.761	-0.781	0.266
	(0.573)	(0.681)	(0.624)	(1.339)	(1.201)	(1.227)
$\triangle protection$	0.758	0.600	-1.430	2.357	2.404	1.471
	(1.127)	(1.389)	(1.425)	(1.512)	(1.487)	(2.261)
Gov. trends	Yes	Yes	Yes	Yes	Yes	Yes
Obst. proximity	Yes	Yes	Yes	Yes	Yes	Yes
Settle. proximity	Yes	Yes	Yes	Yes	Yes	Yes
Restrict sample	None None	10th-90th perc. 2007 labor force	10th-90th perc. $\triangle \%employ$	10th-90th perc. $\triangle obstruction$	10th-90th perc. $\triangle protection$	10th-90th perc. all together
Observations	480	382	236	364	370	132
R-squared	0.668	0.562	0.854	0.714	0.713	0.910

Robust standard errors are clustered using 310 clusters determined by poverty mapping teams at the World Bank and PCBS. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

TABLE 3 *Dependent variable - % net-immigration*

	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) 2SLS	(6) 2SLS
$\triangle obstruction$	-0.473	-0.237	0.0974	-0.251	-0.264	0.139
	(0.318)	(0.212)	(0.108)	(0.681)	(0.623)	(0.283)
$\triangle protection$	-1.112**	-0.476	-0.0769	-1.576**	-1.541**	-0.246
Gov. trends	Yes	Yes	Yes	Yes	Yes	Yes
Obst. proximity	Yes	Yes	Yes	Yes	Yes	Yes
Settle. proximity	Yes	Yes	Yes	Yes	Yes	Yes
Restrict sample	None None	10th-90th perc. 2007 labor force	10th-90th perc. $\triangle \%employ$	10th-90th perc. $\triangle obstruction$	10th-90th perc. $\triangle protection$	10th-90th perc. all together
Observations	480	382	384	364	369	227
R-squared	0.630	0.304	0.494	0.732	0.726	0.573

Robust standard errors are clustered using 310 clusters determined by poverty mapping teams at the World Bank and PCBS. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

TABLE 4 *Roles of distribution tails in key variables*

	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) 2SLS	(6) 2SLS
$\triangle obstruction$	-3.756***	-4.736***	-4.069***	-4.084**	-4.023**	-2.683
	(1.005)	(1.185)	(0.648)	(2.054)	(1.910)	(2.605)
$\triangle protection$	3.507**	4.240*	2.426**	4.851**	4.562**	4.606
	(1.560)	(2.239)	(1.212)	(2.338)	(2.303)	(3.234)
Gov. trends	Yes	Yes	Yes	Yes	Yes	Yes
Obst. proximity	Yes	Yes	Yes	Yes	Yes	Yes
Settle. proximity	Yes	Yes	Yes	Yes	Yes	Yes
Restrict sample	None None	10th-90th perc. 2007 labor force	10th-90th perc. $\triangle \%employ$	10th-90th perc. $\triangle obstruction$	10th-90th perc. $\triangle protection$	10th-90th perc. all together
Observations	480	382	384	364	369	219
R-squared	0.523	0.433	0.677	0.570	0.575	0.645

Robust standard errors are clustered using 310 clusters determined by poverty mapping teams at the World Bank and PCBS. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Roles of distribution tails in key variables

Table 4 repeats the main 2SLS regression, but restricts the sample in various ways to understand the influence of distribution tails in each of the key variables of interest. For ease of comparison, Column 1 reprints the main result using the full sample. In Column 2, all neighborhoods whose labor force size falls outside the 10th-90th percentiles are excluded (at least 48 laborers and no more than 2,004 laborers). Sample size declines to 382 neighborhoods, the point-estimates on both treatment variables rise, though statistical significance declines for $\triangle protection$ to the 10% level. Closer investigation reveals that this has to do with the exclusion of larger neighborhoods. If the only restriction is that

labor force size should be at least 300, for example, the point-estimates remain statistically significant at the 5% level or better (similarly for lower restrictions like 200 or 100 laborers). If I additionally insist that neighborhoods have labor forces of less than 5,000 laborers, I find point-estimates −3.557 (1.158) and 4.238 (1.629) for $\triangle obstruction$ and $\triangle protection$, respectively. The results in Column 2, however, restrict neighborhoods to no more than 2,004 laborers, which begins to erode statistical significance of the protection effect. This makes perfect sense: the centrally located commercial destination neighborhoods, for which the protection effect dominates, tend to be larger, so if I systematically exclude them from the sample then the estimates on this effect, while not attenuating in magnitude, should become noisier. Thus the findings in Column 2 help us understand the main results better. In Column 3 I restrict the dependent variable $\triangle\%employ$ to its 10th to 90th percentiles. The sample reduces to 384 observations, but point-estimates of interest remain significant at the 5% level. In Column 4 I restrict $\triangle obstruction$ to its 10th to 90th percentiles, and in Column 5 I do the same for $\triangle protection$. Sample size falls to 384 and 364 observations, respectively, but in both cases the point-estimates of interest remain significant at the 5% level. In Column 6 I apply all restrictions of Columns 2-5. Sample size falls to 219 observations and the point-estimates, while remaining large and correctly signed, fall out of statistical significance. Closer investigation reveals suggest this is not driven primarily by the restrictions on labor force sizes, but rather the inter-dependence of other variables' tail observations.

Heterogeneities: Informality, gender, cross-border commuting

In Table 5 I explore the role of informal employment in the findings. Columns 1 and 2 define the treatment variables using lights for m_k as in Table **??**. Column 1 re-prints

TABLE 5 *Main results, netting out non-wage employees*

	Using Lights		Using Firms	
	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) 2SLS
$\triangle obstruction$	-3.756***	-3.621***	-2.450***	-2.400***
	(1.005)	(0.858)	(0.657)	(0.557)
$\triangle protection$	3.507**	1.636	2.485*	0.612
	(1.560)	(1.511)	(1.473)	(1.441)
$\triangle \%nonwage\_employment$		0.414***		0.419***
		(0.0634)		(0.0630)
Gov. trends	Yes	Yes	Yes	Yes
Obst. proximity	Yes	Yes	Yes	Yes
Settle. proximity	Yes	Yes	Yes	Yes
Observations	480	480	480	480
R-squared	0.523	0.593	0.524	0.597

Robust standard errors are clustered using 310 clusters determined by poverty mapping teams at the World Bank and PCBS. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

the main findings for ease of comparison. In Column 2 I control for percentage-point change in non-wage employment. This includes self-employed laborers, family members who participate in the family business without formal compensation, and self-identified business owners. These non-wage laborers accounted for 26% of employed laborers in 2007. Controlling for their contribution has no appreciable effect on the point-estimate for $\triangle obstruction$, but greatly attenuates the point-estimate on $\triangle protection$. Similarly in Columns 3 and 4, where the treatment variables are defined using open firms for m_k depreciates the point-estimate on $\triangle obstruction$ remains stable while the point-estimate on $\triangle protection$ attenuates. This suggests that those who replaced wage-earning commuters were often remunerated off-the-books. For example, when a wage-earning commuter from out of town could no longer reach his job, a nephew dwelling nearby might be hired to take his place, with familial connections obviating formal contracting. In Table 6 I explore how the results vary by gender and with regard to cross-border commuting into Israel. For ease of comparison, Column 1 repeats the main regression, where male and female laborers are pooled together. In Columns 2 and 3 I separate out by female and male laborers, respectively. A comparison of Column 2 and 3 suggests that the effect of Israeli obstacles was felt entirely by Palestinian male laborers. Obstacles had no statistically significant effect on female employment rates one way or the other. To interpret this result, note firstly that, owing to the patriarchal nature of West Bank Palestinian society, and reflective of Arab society more generally, only 14% of the Palestinian West Bank 1997 labor force was female (14.8% in 2007), and only 11.6% of employees (14.6% in 2007). Among the few women who do overcome both the familial and professional hurdles to finding a job, very few of them commute far from their neighborhoods of residence. I can see this, for example, in the data on commuters to Israel and settlements: just 0.5% of employed female laborers, relative to 3.0% of employed male laborers, commuted to

TABLE 6 *Heterogeneity: gender, cross-border commuting to Israel*

	(1) $\triangle\%employ$	(2) $\triangle\%employ\ female$	(3) $\triangle\%employ\ male$	(4) $\triangle\%employ$	(5) $\triangle\%work\_israel$
	2SLS	2SLS	2SLS	2SLS	2SLS
$\triangle obstruction$	-3.756***	0.982	-4.120***	-2.338**	-2.798**
	(1.005)	(2.886)	(0.871)	(0.959)	(1.093)
$\triangle protection$	3.507**	1.229	3.350**	2.842**	0.790
	(1.560)	(4.839)	(1.375)	(1.411)	(1.481)
$\triangle\%work\_israel$				0.361***	
				(0.0536)	
$\triangle\%employ$					0.300***
					(0.0520)
Gov. trends	Yes	Yes	Yes	Yes	Yes
Obst. proximity	Yes	Yes	Yes	Yes	Yes
Settle. proximity	Yes	Yes	Yes	Yes	Yes
Observations	480	480	480	480	480
R-squared	0.523	0.595	0.463	0.585	0.790

Robust standard errors are clustered using 310 clusters determined by poverty mapping teams at the World Bank and PCBS. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

settlements in 1997, and just 3.3% of employed female laborers, relative to 23.9% of employed male laborers, commuted to Israel. Likewise in 2007, just 0.4% of employed female laborers, relative to 2.9% of employed male laborers, commuted to settlements to work, and just 1.1% of employed female laborers, relative to 15.0% of employed male laborers, commuted to Israel. It is no surprise, then, that the obstructive effect of obstacles has no impact on female employment rates, since few Palestinian women were commuting far enough to collide with them. On the other hand, it is interesting to note that obstacles had no significant protective effect for female employment rates. This implies that male commuters who lost their jobs were replaced by other males, not by females.

In Columns 4 and 5 I explore the influence of cross-border commuting to Israel on the findings. By construction, $\triangle obstruction$ incorporates the blockadedness of routes from Palestinian neighborhoods to Israeli border crossings, so part of the obstructive effect on employment that I are finding is driven by loss of access to Israeli border crossings, while the rest of the effect comes from loss of access to Palestinian commercial centers. In Column 4, I see that if I control for neighborhoods' percentage-point change of employment in Israel, the point-estimates of interest remain statistically significant, but their magnitudes fall somewhat (38% decline for $\triangle obstruction$, 19% decline for $\triangle protection$). In Column 4, I put percentage-point change in employment in Israel on the left-hand side and control for overall percentage-point change in employment. Predictably, the obstructive effect of internal obstacles does adversely affect employment rates as laborers struggle to reach border crossings. The protective effect is signed positively but not statistically significant. This make sense, since the additional hurdle of obtaining Israeli work permits (see Miaari, Zussman, and Zussman (2014)) likely made it harder for laborers to replace absentees.

In conclusion, the paper's main findings appear robust to all sensible alternative hypotheses, and support the thesis that adverse shocks to connectivity will have spatio-

distributional impacts but a very attenuated impact on aggregate unemployment in the urban economy as a whole.