

# **A Checkpoint Effect? Evidence from a Natural Experiment on Travel Restrictions in the West Bank**

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## **Abstract**

Does *non-violent* repression prompt subject groups to obey or rebel? By what mechanism does it do so? To address these questions we exploit a natural experiment based on a 2009 policy towards the ‘easement’ of checkpoints – non-violent impediments to movement – in the West Bank. We sample populations across 17 villages (n=599), beside one checkpoint slated for easement (treatment) and one that will undergo no change (control), before and after the intervention. We then pursue difference-in-difference estimation. This design is experimental, as easement was *orthogonal* to Palestinian attitudes; for robustness, we test our findings against an independent panel (n=1200). We find that easement makes subject populations *less likely* to support violence; we suggest *humiliation* as the mechanism bridging non-violent repression with militancy. This warrants rethinking Israeli security policy, as short-term concerns over Palestinian mobility may be compromising Israel’s long-term interests. By extension, checkpoint easement may positively affect peace negotiations.

**Key Words:** Checkpoints; Conflict Zone, Israel-Palestine; Natural Experiment; Repression; Dissent

**Word Count:** 8473

*“The checkpoints prevented hundreds of terrorist attacks against the Israeli population”*

– Israeli Ministry of Justice<sup>1</sup>

*“I don’t know how many terrorists decided to take up arms against Israel when they were standing in the sun for hours at checkpoints”*

– Peace Now<sup>2</sup>

Does *non-violent* repression prompt subject groups to obey or rebel? By what mechanism does it do so? Scholarly debate over repression and dissent is highly developed, but systematically overlooks non-violent institutions; further, to date most research is observational and inconclusive. In this paper we redress these shortcomings via a study of checkpoints – i.e. non-violent impediments to movement<sup>3</sup> – in the West Bank. According to the UN Office for the Coordination of Humanitarian Affairs (OCHA), as of 2012 there were 540 “obstacles blocking Palestinian movement” in the West Bank, including 59 staffed checkpoints and 455 un-staffed impediments to movement, such as roadblocks, gates, barriers and trenches (OCHA 2012b). We ask: *do checkpoints make Palestinians more likely to support diplomatic negotiation, or violence against Israel?* As the epigraph makes clear, contending local interests argue on the one hand that checkpoints suppress violence; on the other, that they perpetuate it. Which is it, and how can we tell?

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<sup>1</sup> Israeli Ministry of Justice website: <http://www.justice.gov.il> (last accessed May 2009).

<sup>2</sup> Hagit Ofran, cited in Hider 2007.

<sup>3</sup> Checkpoints are generally non-violent in nature, designed towards population management and control. Of course, they might be the *sites* of violence, but such events are exceptional. Indeed, most death or injury tolls at checkpoints relate to lack of access to medical care – such as, for example, because ambulances are prevented passage (WHO 2013). Otherwise, violence in the West Bank is quite limited and checkpoints represent but a tiny portion of total incidents (OCHA 2008c).

We confront this problem by exploiting a natural experiment based on a policy intervention by the Tony Blair-led Quartet (US, UN, EU and Russia)<sup>4</sup> in May/June 2009 toward the *easement* of checkpoints as a means of opening up economic corridors in the West Bank. The ‘*Jenin First Initiative*’ was initiated as a pilot in the north, to be extended if deemed successful. This intervention presents the rare opportunity to draw causal inference on Palestinian political preferences, since while travel in the Jenin corridor was eased, restrictions outside of this area remained in place. To capture the effect of this initiative, we sampled populations (n=599) before and after the intervention – some in villages near the Za’atara checkpoint, slated for easement (treatment), others near the Wadi Nar checkpoint, which would undergo no change (control). We then pursue difference-in-difference estimation to ascertain the effect of easement on Palestinian political attitudes. This design is experimental because the policy is *as-if* exogenous – or *orthogonal* – to Palestinian attitudes, and because it occurred without corresponding changes to other institutions of repression. We are able to rule out rival explanations for the divergence in preferences outside of the treatment alone, thereby isolating a ‘checkpoint effect’.

This difference-in-difference design matches other natural experiments derived from administrative or jurisdictional boundaries (Asiwaju 1985; Miles and Rochefort 1991; Card and Krueger 1994; Miguel 2004; Krasno and Green 2005), using a village-based selection model akin to that employed by Posner (2004), with the noise of non-random assignment cleaned up via difference-in-difference estimation (Ashenfelter and

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<sup>4</sup> The Quartet is involved in mediating the Israeli-Palestinian peace process; it was established by former US President George W. Bush in 2002 as part of the Road Map.

Card 1985; Smith and Todd 2005). In doing so, it provides us unique insight into a centerpiece of the Israeli-Palestinian conflict, which remains grossly understudied due to issues of identification. In the West Bank it is almost impossible to distinguish one facet of control from others; as a result, existing studies of checkpoints have been un-systematic and incomplete (Brown 2004; Naaman 2006; Bornstein 2006; Kotef and Merav 2011), with econometric reports going so far as to claim that “the losses of internal closures are difficult to measure” (Aranki 2004), or that “quantifying the economic impact of current restrictions is difficult given the paucity of data” (World Bank 2008).<sup>5</sup> As with any natural experiment, it is incumbent upon us to prove that the treatment assignment can be considered *as-if* random, such that the expected value of treatment and control vary due to the intervention alone (Dunning 2008). We take extra care to address challenges to identification – a small price for such a unique research frame – drawing on extensive fieldwork to clarify our research design and support our claims.<sup>6</sup> Additionally,

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<sup>5</sup> Challenges to identification are a mainstay of conflicts across the Middle East (Romano 2006; Clark 2006; Tessler and Jamal 2006), making experimental research in the region evermore critical.

<sup>6</sup> There will always be some threat to *as-if* random when the distinction is spatially determined (as through jurisdictional borders) – a challenge faced by Brady and McNulty (2011), Posner (2004) and Card and Kruger (1994). This does not disqualify such studies; however, it raises the stakes for substantively qualifying identification. Indeed, the classic trade-off of natural experiments is the sacrifice of true randomization, for social and political relevance. After all, for many important questions in political science controlled experiments are “impractical or unethical” (Freedman 1999: 255) or are of limited range (Druckman et al 2006). As Dunning explains: “natural experiments can allow us to study phenomena [that] are not routinely amenable to true experimental manipulation ... they can contribute to causal inferences in ways that neither true experiments nor conventional observational studies may do” (Dunning 2012: 306).

we test the robustness of our findings against an independent panel (n=1200) conducted in four waves between 2007-2009, across a representative sample in the West Bank.

We find that Palestinians subject to checkpoint easement are significantly *less likely* to support violence against Israel, or the militant Islamist group, Hamas over the secular-nationalist Fatah. This confirms a positive relationship between *non-violent* repression and support for *violent* dissent, such that high levels of repression correspond to support for violence; as conditions of repression lessened so did the subject population's preference for dissent. We suggest one particular mechanism linking the experience of checkpoints and support for militancy – *humiliation*. We find that Palestinians who feel humiliated by the experience of checkpoints are more likely to support violence against Israel than, for example, those who express fear or have suffered financial loss. Together, these findings have considerable practical significance, indicating that checkpoint easement may have a positive effect on future Israeli-Palestinian peace negotiations. To this end, they suggest a rethinking of Israeli security policy, as short-term concerns over Palestinian movement may be compromising Israel's long-term interests – a Faustian bargain that would be devastating to perpetuate.

### **Checkpoints in the Context of Repression and Dissent**

The debate over whether institutions of repression prompt subject groups to obey or rebel is burgeoning, but far from reconciled. Some scholars find that increased repression diminishes dissent by weakening the opposition and convincing fence-sitters to remain

loyal to the regime, thereby raising the cost of collective action (Olson 1971; Ostrom 1998; Diamond 2002; McFaul 2002). Others posit that repression encourages rebellion by creating conditions so unbearable that non-combatants come to believe that insurrection is the only option (Tullock 1971; Gurr and Duvall 1973; Mason and Krane 1989; Francisco 1996; Wood 2003; Kalyvas 2006). Another set of authors claim the relationship is non-linear. Some suggest a U-shaped curve, such that rebellion is constrained at intermediate levels of state penetration, but low levels enable rebellion, and higher levels of repression prompt increased recruitment, as the cost becomes too high *not* to organize (Gurr 1970; Lichbach and Gurr 1981). By contrast, others suggest an inverted U-shaped curve, which peaks at intermediate levels of repression, as low government repression makes rebellion unnecessary, and high levels make it impossible (Lichbach 1987; Mason 1989; Moore 1998). Finally, numerous studies sever the one-to-one relationship between repression and dissent, or suggest the other direction of causal inference, such that dissent causes repression (Davis and Ward 1990; Davenport 1995; Gurr and Lichbach 1986).

This debate remains inconclusive largely because of the tools employed towards its measurement. Repression is infrequently a single act but rather a bundle of policies and tactics that must first be disaggregated to be understood. It is for this reason that the experimental method is so instructive. As articulated above with regard to identification, it is only through experimentation that we can distinguish *between* instruments of repression – in this case between checkpoints and, for example, settlements. This study presents a critical advance in this regard, as it isolates a particular repressive instrument over and above other machineries of obedience or dissent, thereby establishing *causal*

*direction* (Heckman 2000; Achen 2002; Sekhon 2009) and providing a benchmark against which prior observational research can be evaluated (Druckman et al 2006).

In addition, this literature suffers from a systematic neglect of *non-violent* forms of repression. Indeed, despite frequently relying on definitions of repression that putatively include non-violent forms, in nearly all existing studies, technologies of repression are violent. For example, while several authors place non-violent considerations in their models (Gurr 1970; Tullock 1971; Lichbach 1987), these ‘events’ poorly fit their specifications: non-violent means of repression might increase in number, but not magnitude, and cannot be measured on the same scale. Further, most accounts rely on rationalist explanations of resistance, *derivative of violence*, whereby people resist because the status quo is too dangerous to maintain, or obey because it has a lower cost than dissent (Lichbach 1987; Wood 2003; Kalyvas and Kocher 2007). How can these lessons apply to cases where *non-violent* repression produces a *violent* response? At present, we lack a persuasive narrative linking repression and dissent in the absence of a credible threat of violence.

This is at core a question of mechanism. Returning to the case at hand, what is it about a non-violent form of repression, such as a checkpoint, that might drive Palestinians to support violence? This is difficult to answer because it is not easy to identify the precise *harm* of checkpoints. Existing studies reveal a bevy of possibilities: some highlight economic losses, as checkpoints obstruct trade, increase shipping costs and interrupt farmland (Aranki 2004; Simpson 2007); others cite confrontations with soldiers (Brown 2004; Kotef and Amir 2011) or health concerns, with diminishing opportunities for health care leading to increasing infant mortality rates and diminishing

average life span (WHO 2013). But are these really sufficient reasons to support violence? Which factor *drives* such support?

We hypothesize that checkpoints prompt people to support violence due to *humiliation* – i.e. feeling unjustly demeaned, or subjugated by another, usually the result of asymmetric power relations (see e.g. Hartling and Luchetta 1999; Lindner 2002; Ginges and Atran 2008). There is a vast literature within psychology linking humiliation and violence; but this debate too is inconclusive. Some authors suggest a positive relationship, such that humiliation prompts support for violence (Frijda 1994; Saurette 2006; Walker 2006). Within political science, Thomas Scheff argues that humiliation is the mechanism for both individual rage and collective action within conflict (Scheff 1994: 69); Harkavy asserts a “causal nexus between humiliation and revenge” (Harkavy 2000: 350). Humiliation is also used to explain how grievances arise (Wood 2003), as when deprivation theorists use ‘frustration levels’ to predict political violence (Ellina and Moore 1990; Opp and Roehl 1990; Muller and Weede 1994). By contrast, other scholars suggest that humiliation might decrease support for violence, by cultivating inertia and a sense of hopelessness and inferiority. Politically, leaders of insurgent groups have long claimed the humiliation has suppressed support for rebellion (Ginges 1997); numerous psychological studies have found that humiliation has an inhibiting effect on behavior (Keltner et al. 1998; Holtgraves and Lasky 1999). Indeed, a recent study set in the West Bank found that humiliation actually suppresses violence (Ginges and Atran 2008).

This prompts a further theoretical question: does humiliation prompt support for violence or quell it? If the former, is the type of humiliation wrought by non-violent forms of repression sufficient to spur such radicalization? These matters are tested below.



## Checkpoints in the West Bank, 1967-2009

A “checkpoint” is defined by the United Nations as any staffed physical impediment to travel within a territory. In the Israeli-Palestinian context, this means travel *within* the West Bank, and *between* Palestinian communities. This *does not* include “crossings” which delimit the border between the Palestinian territories and Israel, or that separate Palestinian communities and Israeli settlements.<sup>7</sup> Checkpoints are a subset of a broader infrastructure of “closures” within the West Bank, which includes roadblocks, earth mounds, and gates [see (SI) A.1]. Certain restrictions on Palestinian travel in the West Bank have existed since 1967. However, the systematic introduction of closures came with the finalization of Oslo as an interim accord (Oslo II) at Taba in 1995, after which the West Bank was officially divided into sections of Israeli and Palestinian control.

During the *al-Aqsa Intifada* in 2000-2005 – a violent Palestinian uprising – a comprehensive network of checkpoints emerged, such that “the total restricted area ... appears to be in excess of 50% of the land of the West Bank” (World Bank 2007). During this period, the number of Israeli settlers rose drastically, from 190,206 in 2000 to 247,514 in 2005 (Hareuveni 2010); Israel also began constructing the separation barrier between Israel and the West Bank, and the number of checkpoints within the West Bank ballooned to nearly 400 (OCHA 2010).

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<sup>7</sup> Additionally excluded are the 69 closures within the city of Hebron, as well as flying checkpoints, which are occasionally erected on an ad-hoc, temporary basis.

The end of the *al-Aqsa Intifada* resulted in a cease-fire and the signing of the Agreement on Movement and Access (AMA) on November 15, 2005, in which Israel pledged to “facilitate the movement of people and goods within the West Bank,” agreeing to a baseline of no more than 376 closures. However, settlements and closures continued to grow, with the UN declaring that “no” work was ongoing to “establish a plan to reduce obstacles” (OCHA 2006). Further, with the 2006 Palestinian elections bringing victory to Hamas, a militant Islamist party, hope of progress dissipated – as evidenced by the failed Annapolis Conference of 2007 (see e.g. Berman 2008).

In a show of reinvigorated commitment, in 2008 former British Prime Minister Tony Blair, the Quartet’s Envoy to the Middle East, alongside Israeli diplomats from the Economic Cooperation Forum (ECF) led by Ya’ir Hirschfeld, spearheaded a campaign – *The Jenin First Initiative* – calling on Israel to ease restrictions on the Palestinians as a means of spurring economic development. Despite diplomatic talk, however, there was little progress on the ground. Indeed, the number of checkpoints continued to rise such that by spring 2009 – the point of intervention – the UN recorded more than 600 closures within the West Bank – their highest level (OCHA 2009b) [see SI B.1].

#### *The Jenin First Initiative: May/June 2009*

The Jenin First Initiative was given public voice on May 13, 2008 in a speech by Tony Blair; however, this plan has its antecedents in 1999, with a local-level initiative between the Governor of Jenin and several Israeli regional councils to facilitate cross-border trade,

brokered by the ECF. The main outgrowth of the plan was the Jenin Industrial Park, which was designed to take place on 350 acres of land, and employ 10,000 Palestinians. This ambitious project began its development in 1999, but died out almost immediately with the onset of the second Intifada in 2000.

In 2008, Hirschfeld re-introduced his plan to bring about economic development in the West Bank by opening up ‘Economic Corridors’ for Palestinian traffic and trade. This process would begin in Jenin, due to the existence of the now long-defunct Industrial Park, and stimulate Palestinian economic activity “by attracting (foreign) investments and creating sustainable employment and income generation in the region of Jenin” (Blair, 2008). If successful, this initiative would serve as a pilot for future development across the West Bank, setting what Blair referred to as a ‘credibility threshold’.<sup>8</sup> Future corridors include the Tarqumya Industrial Park (by Hebron), and the Agro-Industrial Park (by Jericho) [See SI B.2].

*The Jenin First Initiative* was finally agreed upon in early 2009; however, before development on the Industrial Park could begin, an agreement was made towards the easement of several checkpoints – as unless business interests could transit unimpeded, no industry would be able to succeed (Giambi 2009). Easement began in the summer of 2009 on several checkpoints along the Jenin-Ramallah highway. This was the first step in the ECF’s plan; it also provided the necessary precondition for our study.

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<sup>8</sup> The ECF defines this plan as “an ‘area-by-area’ model for increased Palestinian security activity and increased Palestinian-Israeli security cooperation, which will begin in the Jenin area first ... [and] if successful, will be extended to other areas of the West Bank” (ECF 2008a: 3).

## **The Natural Experiment: Identifying a “Checkpoint Effect”**

Our research design is straightforward: Israel’s decision in the summer of 2009 to lift a set of checkpoints in the West Bank meant that civilian travel in the Jenin corridor was eased, whereas restrictions outside of this area remained the same. This policy intervention presented us with the rare opportunity to draw causal inference on Palestinian political preferences, since checkpoint easement (independent variable) occurred prior to the related change in political attitudes (dependent variable). As we will show, this discontinuity was not contingent on political factors, but can be considered *as-if exogenous*, such that variation between different populations in the West Bank was irrespective of the intervention itself. Further, since all other facets of Palestinian life remained the same, we were able to rule out rival explanations for the divergence in public opinion outside of the treatment alone.

### *Empirical Strategy*

Checkpoint easement began in May/June 2009; however, the empirical strategy we employ is based on field research conducted by the authors during the preceding summer (2008) in which discussions with international and Israeli political elites revealed that the easement of certain checkpoints was a real possibility. There was not much public

discussion at this point, as these talks took place under a Track II diplomatic setting – i.e. unattributed, unofficial conversations (ECF 2008a: 1). However, we were able to glean enough information to move ahead with the project.

In October 2008 we conducted an initial round of surveys, administered by Near East Consulting. The population sample is based on two population-clusters (matched pairs), a sample frame of 17 villages,<sup>9</sup> situated near two checkpoints – a candidate for easement, Za’atara (treatment), and a control, Wadi Nar. The first wave ( $t_0$ ) consisted of a random sample of 599 Palestinian adults, interviewed face-to-face – 297 at Za’atara and 302 at Wadi Nar. In November 2009 ( $t_1$ ), a new random sample was drawn,<sup>10</sup> with 504 interviews selected based on their proximity to treatment or control sites: Za’atara ( $n=219$ ) and Wadi Nar ( $n=234$ ). Within each group, subjects were chosen based on a household selection method with a pre-defined route; within household, respondents were selected according to the Kish method. Interviews were ‘double-blind’, as the enumerators were also unaware of the experimental design.

In Figure 1 we present the *ex ante* balance of our outcome variables and controls, beginning with standard demographic indicators, and including region-specific measures for religious attitudes and behavior. Our dependent variables measure attitudes towards militancy and peace negotiations, drawing from existing studies, including studies on the Israeli-Palestinian conflict (Huddy, Khatib, and Capelos 2002; Nelson and Milburn 1999;

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<sup>9</sup> *Wadi Nar*: al'Ubeidiya, al-Haddadiya, Dar Salah, Ash-Sheikh Sa'd. *Za'atara*: Yasuf, Yatma, Qabalan, As-Sawiya, Talfit, Iskaka, 'Einabus, 'Urif, Huwwara, Beita, Odala, Jamma'in, 'Awarta

<sup>10</sup> The second wave was intended to be a panel, with enumerators returning to the same households for a second round of interviews. However, a pre-sampling test revealed that our attrition rate would be too high – a common problem in conflict zones (see e.g. Romano 2006). Thus we conducted new random samples.

McAlister, Bandura, and Owen 2006), focusing on attitudes towards peace, violence and party affiliations (in this case, Fatah and Hamas) [For further explanations, see SI A.2].<sup>11</sup> As is evident, there is *ex ante* covariate imbalance – an expectation of our model – thus we cannot reject the null hypothesis that there is a difference in means across our baseline 2008 data, an issue we address through difference-in-difference estimation.

Figure 1: Balance Checks for Opinion Measures and Demographics

Variable	Index	Mean (ZA 2008)	Mean (WD 2008)	p-value ttest
Secular/ Non-Violent	1 to 8	5.425	4.697	0.037
Distrust of Israel	1 to 6	5.275	4.698	0
Militancy	1 to 6	3.863	3.567	0.003
Extremism	1 to 5	2.018	2.374	0.082
Two State Solution	1 to 6	1.990	2.057	0.387
Hamas (over Fatah)	0 to 1	0.248	0.293	0.404
Religion Ideology	1 to 6	4.326	3.879	0
Religion Behavior	1 to 6	4.638	3.978	0
Age	1 to 5	2.404	1.894	0
Wealth	1 to 5	2.397	2.776	0
Gender	1 to 2	1.616	1.694	0.045

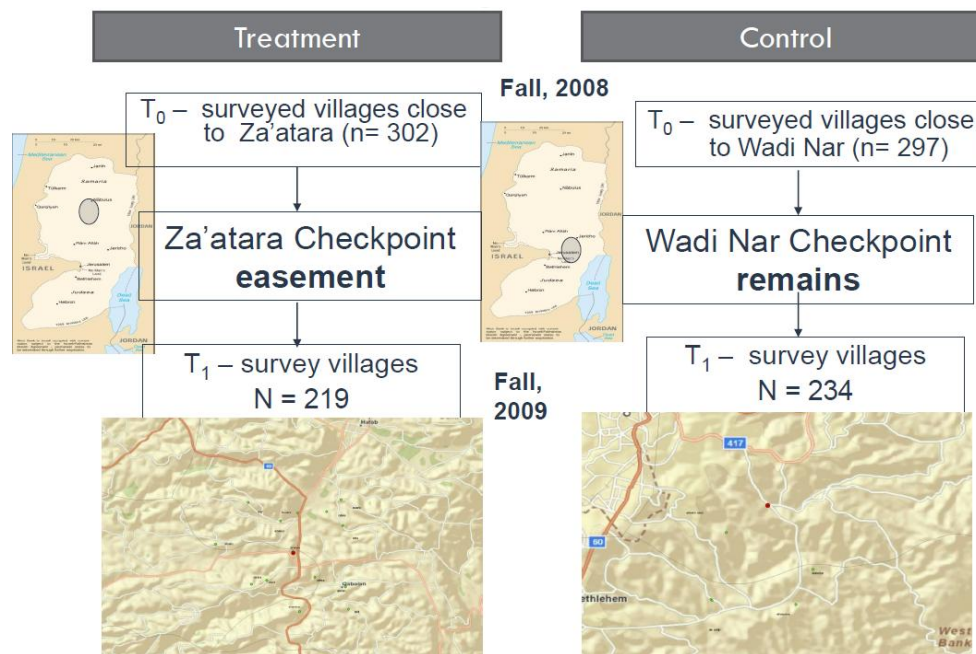
The two sites we use in our study, Za’atara (treatment) and Wadi Nar (control), were paired due to typological similarity, as they are considered to be equivalent both by UN classification [see SI A.1] as well as by the ECF (ECF 2008b). They are also both on the central artery of the West Bank, the Jenin-Hebron corridor [see Figure 2; see also SI

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<sup>11</sup> The variables used in this article constitute a subset of a larger questionnaire about Palestinian attitudes in the West Bank; all questions pertinent to this study are reported. The broader questionnaire of which this is a part is introduced in SI D, in our discussion of external validity.

B.1]. Za'atara,<sup>12</sup> regulates all traffic between Nablus and Ramallah; Wadi Nar, between Ramallah and Bethlehem. They are similar in form, function and reputation, with a substantial physical structure (control rooms, walkways, and watchtowers) as well as long lines of cars, with waits of up to an hour.

Figure 2: Map of Za'atara and Wadi Nar Surveys



The essential point is that in the period leading up to and including our first

<sup>12</sup> In reality this represents two sites, Za'atara and Huwwara, which are proximate to each other and regulate the same passage, but are not physically adjoined. We do not distinguish between them, as they are not considered discrete checkpoints, since one is pedestrian (Huwwara) and one is automotive (Za'atara). We use the single name "Za'atara" for clarity.

survey sampling window, the sites were considered comparable – notwithstanding circumstantial differences like design, layout, dimensions – both by Palestinian and international observers (see e.g. Barsella 2007).<sup>13</sup> Most importantly, the two sites did not differ along any axes that might contaminate the study. Additionally, neither site was experiencing a reduction in capacity – in fact, up until the policy intervention, both were undergoing routine renovation. Here is the UN assessment as of mid-2009:

During this period, the Israeli authorities completed the expansion of two key checkpoints: Wadi Nar ... which controls movement through the main north-south transport artery, and Huwwara [Za'atara], which controls access to Nablus City from the south. Contrary to earlier statements issued by the IDF, none of these expansions have so far resulted in an improvement in the flow of traffic, and long delays continue to be observed at peak times (OCHA 2009b).

### *The Experimental Treatment: May/June 2009*

In May/June 2009, Israeli policy shifted radically, with the government deciding to open up the Jenin-Ramallah corridor, turning Za'atara into little more than a traffic circle. Sometimes members of the Israeli Defense Forces occupied a seat in the center of the crossing, but cars were not stopped. For all intents and purposes, the highway was open for civilian passage. What had been hours of waiting was reduced to minor traffic congestion. Here is the UN assessment of the changes to Za'atara in May/June 2009:

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<sup>13</sup> There of course were minor changes in the surrounding areas during the pre-experiment window, most notably the easement of Shave Shamron, a checkpoint nearby Za'atara. But if anything, this would bias results downward. Moreover, it is precisely because Za'atara remained that these small changes were largely superficial (OCHA 2008a).



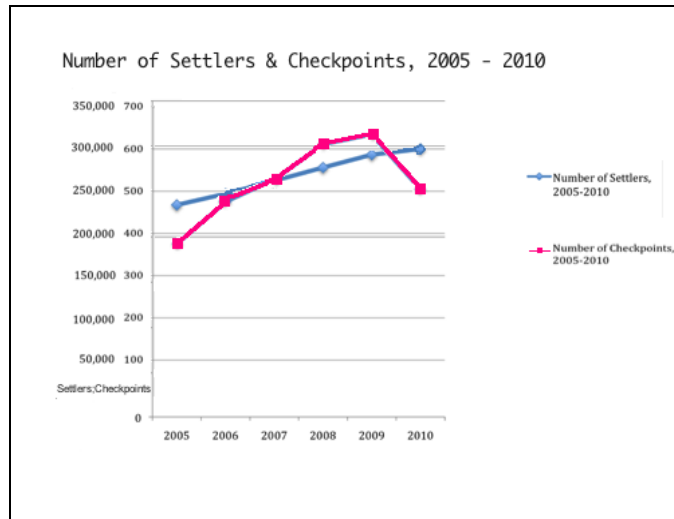
The Israeli authorities implemented a series of measures that improved the freedom of movement of Palestinians between most urban centres, particularly in the north. These measures included the removal of obstacles [and] the relaxation of controls at some permanent checkpoints ... As a result, large segments of the Palestinian population enjoy better access to services, places of work and markets. The total number of closure obstacles documented by OCHA at the end of the reporting period stood at 505, down from 626 on March 2009 (a 19 percent decrease) (OCHA 2010).

By contrast, in the same time period, business as usual continued in Wadi Nar (OCHA 2010; B'Tselem 2010a). These official assessments were confirmed by the authors in person and in consultation with local analysts.

In addition, during this experimental window, there were no other significant changes to Israeli institutions in the West Bank – settlements, closed military zones, etc – which might complicate identification. During the experimental window, settlements continued to develop at their normal clip and access to land remained restricted [see Figure 3]. According to the UN:

Over the course of the past six months (May-October 2009), the Israeli authorities continued to implement measures that increased the freedom of movement of Palestinians ... *However, during the same period, there has been no significant improvement when it comes to access to land and use of space by Palestinians* (OCHA 2009b; emphasis ours).

Figure 3: Settlements & Checkpoints Compared, 2005-2010



### *Estimation*

We pursue a difference-in-difference design comparing the mean values of the two sites – Za’atara (treatment) and Wadi Nar (control) – across two time periods, 2008 ( $t_0$ ) and 2009 ( $t_1$ ). This model mitigates covariate imbalance, by subtracting one set of means from the other, and produces a linear relationship such that the outcome variables incorporate the combined effects of the site easement and the change in time period from 2008 to 2009. Essentially a multiple time series design (Campbell and Stanley 1963), this estimator enables us to make causal claims about temporal changes by adding a control and thereby eliminating alternative explanations for temporal change outside of the treatment itself (Ashenfelter and Card 1985). If properly specified, the outcome of the model can be attributed to the experimental intervention alone. In our case, this means that these findings represent how the easement of the Za’atara checkpoint produced attitudinal shifts within the local population, taking into account the prior views of both

sites (in 2008) as well as changes that may have occurred over time that are unrelated to the easing of the Za'atara checkpoint. Our model is as follows:

$$Y = \alpha + \beta_1 X_1 + \beta_2 X_2 + \beta_3 (X_1 * X_2) + \varepsilon$$

The dependent variable, Y, represents the survey questions on political responses. X<sub>1</sub> is the time variable indicating whether the year is 2008 (before treatment) or 2009 (after treatment). X<sub>2</sub> variable denotes the location, Za'atara which was treated in 2009 or Wadi Nar which was never treated. (X<sub>1</sub>\*X<sub>2</sub>) is the interaction term, computed by multiplying X<sub>1</sub> (time) and X<sub>2</sub> (place). The difference-in-difference model includes the individual effects of variables X<sub>1</sub> and X<sub>2</sub>, in order to test for their individual significance and not to confound the interpretation of the β<sub>3</sub> coefficient. Thus the β<sub>3</sub> coefficient may be interpreted as the relative change in attitudes of the treatment group compared to the control, brought about by treatment.

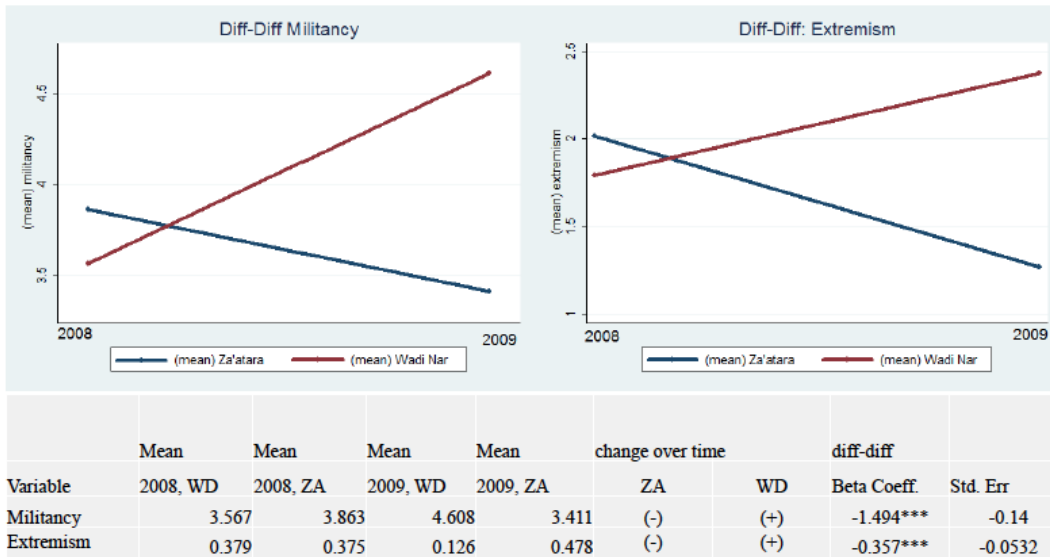
### *Findings*

As a result of the checkpoint easing, public opinion in the treatment group shifted significantly and consistently towards less militant views. Our principle variable, *Militancy*, taken as the mean of responses to a scale of questions regarding attitudes towards violence within the Israeli/Palestinian context, produced an extremely clear outcome: as a result of the checkpoint being lifted, the population sample around Za'atara became much *less* likely to support violence against Israel [see Figure 4]. This finding held true to a high degree of statistical significance (p>0.01), and engendered more than a

full point change (on a six-point scale) away from violent attitudes towards Israel. Importantly, those living adjacent to Wadi Nar also became *more* militant. While this confirms the general scope of our findings it underscores concerns over the contamination of the control – considered at length below in the section on identification.

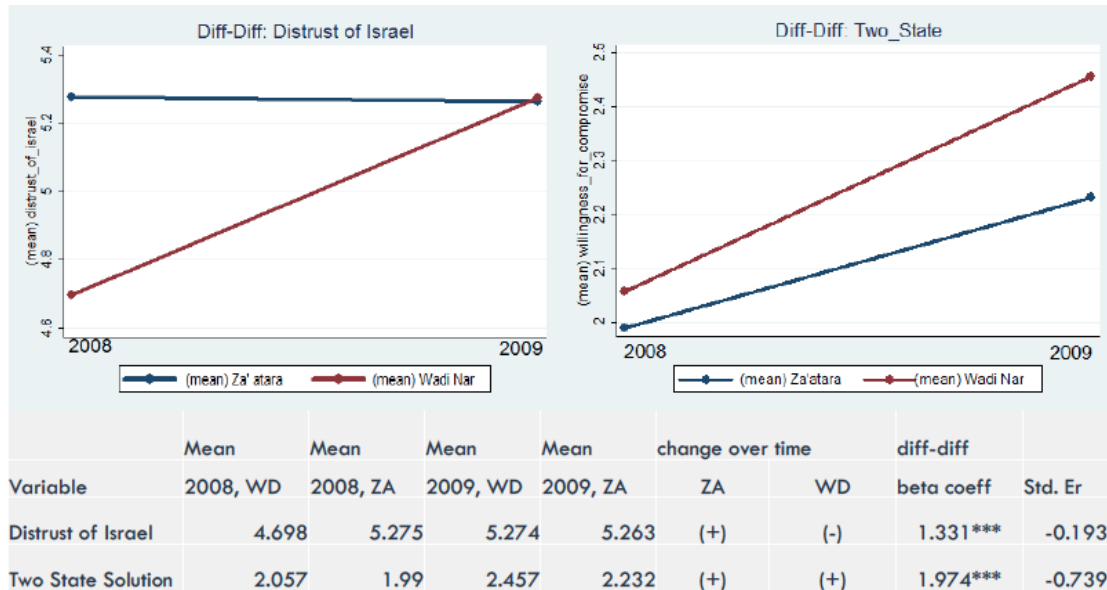
This variable is complemented by a de-localized variable, *Extremism*, which measures Palestinian reaction to global acts of terrorism (Tessler et al 2006). This variable is designed to differentiate support for violence against Israel from Islamic radicalism more broadly. *Extremism* produced the same strong finding, with respondents in Za’atara more likely to accept that violent acts against civilians are acts of terror – evidence of diminished radicalism – also significant to the highest degree. Because this variable is derived from a set of questions about events outside of the West Bank, there are many non-responses (‘don’t know’/‘no opinion’); the findings we cite take the mean of responses reported. [For additional iterations, as well as an evaluation of non-responses, see SI C].

Figure 4: Diff-in-Diff Estimation: *Militancy* and *Extremism*



These militancy variables are complimented by two variables concerned with diplomacy [See Figure 5], although these findings offer considerably less clarity. *Distrust of Israel* asks whether Israel can be considered a trustworthy partner for peace (Nadler and Liviatan 2006). With *Distrust of Israel*, Palestinians in the treatment group were *less* inclined to express negative attitudes towards Israel after the checkpoint easing. This confirms our general findings, but the shift in treatment was minor; instead, the statistical significance was driven by the control group, which became *more* likely to distrust Israel. *Two State Solution* looks at support for the “Two-State Solution” – i.e. the diplomatic agreement in which Israeli and Palestinian states live side-by-side. Here too, findings were significant but perhaps substantively ambiguous. While the population beside Za’atara did increase support for the two-state solution, support also increased within the control. This counter-intuitive finding may be borne of ambiguity within the question, as the “Two State Solution” is a diplomatic endgame that may be attainable as much through violence as through diplomatic means.

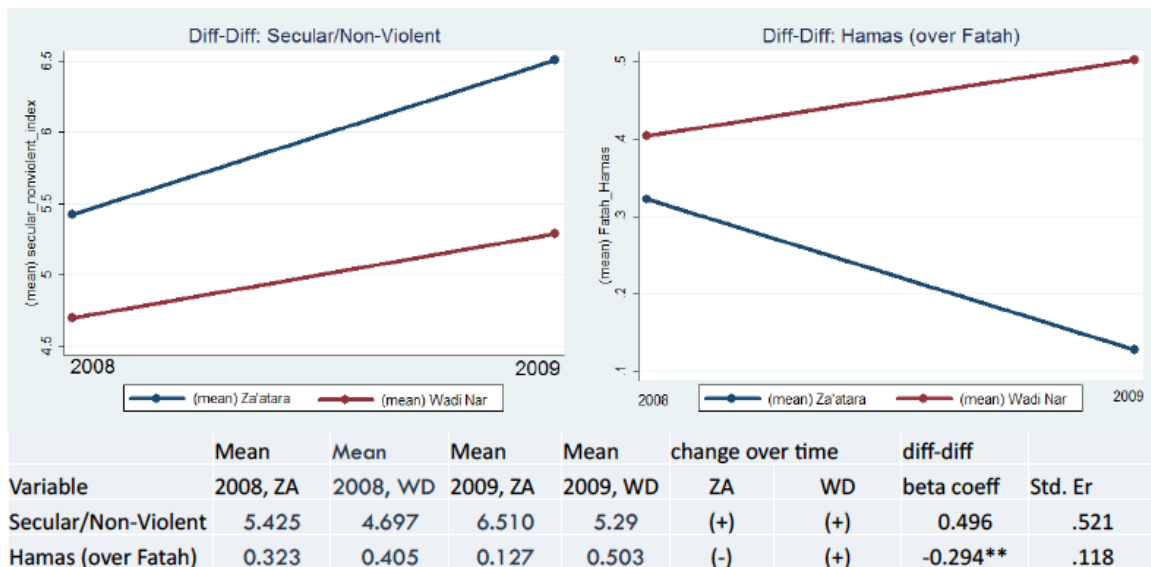
Figure 5: Diff-in-Diff Estimation: *Distrust of Israel* and *Two State Solution*



Our strong findings on militancy are echoed in support for Palestinian political parties [see Figure 6] – drawn from the list of parties on the Palestinian Legislative Council ballot of 2006 and the candidates for the 2005 presidential elections. *Secular/Non-Violent* looks at the entire spectrum of Palestinian political parties, placed on a linear index from “secular-non-violent” parties (e.g. Fatah) to “religious-violent” parties (e.g. Hamas). This index was designed to keep all parties in the study, but eliminate the noise of minor internecine fragmentation; however, this proved inconclusive. The second variable, *Hamas (over Fatah)*, is a categorical variable, looking just at support for one of the two principal parties – Fatah and Hamas – with incremental shifts representing a move in support towards Hamas, and away from Fatah. In this variable, responses for other parties were discarded as missing values. We find that as a

result of the treatment, respondents living near Za’atara were significantly more likely to put support behind Fatah and pull support away from Hamas. Party support is always inexact (e.g. McGreal 2006); however, these measures powerfully comment on the plausibility of peace or violence in the event that elections are held (with support for Fatah indicating that peace outcomes are more likely; vice-versa for Hamas).

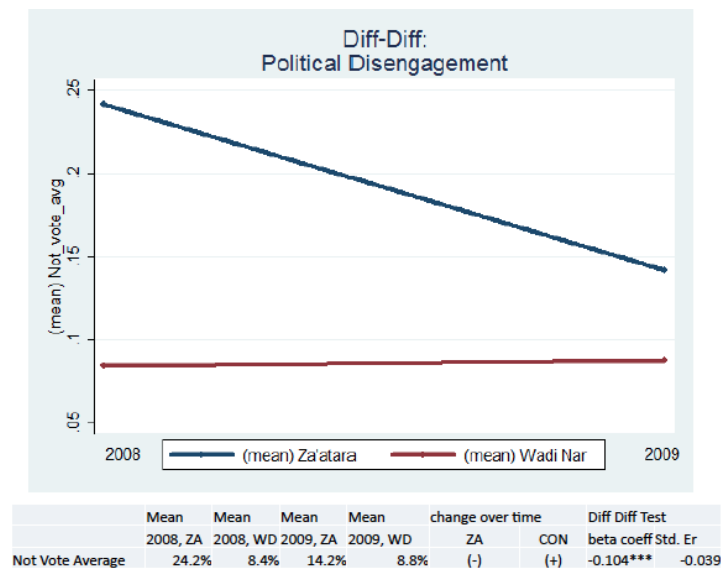
Figure 6: Diff-in-Diff Estimation: *Secular/Non-Violent* and *Hamas (over Fatah)*



In addition to our principle variables, we offer two auxiliary findings. First, in addition to support for less militant political parties and ideologies as a result of checkpoint easement, our variable *Political Disengagement* reveals a statistically significant increase in willingness to participate in elections amongst respondents in Za’atara, as compared to Wadi Nar [see Figure 7; for additional information on this variable see SI C]. This highlights the wide-reaching effect that checkpoint easement has

on the West Bank, as respondents beside Za’atara are both more likely to support less militant parties, *and* more like to vote – compounding and solidifying the move away from support for violence.

Figure 7: Diff-in-Diff Estimation: *Political Disengagement*



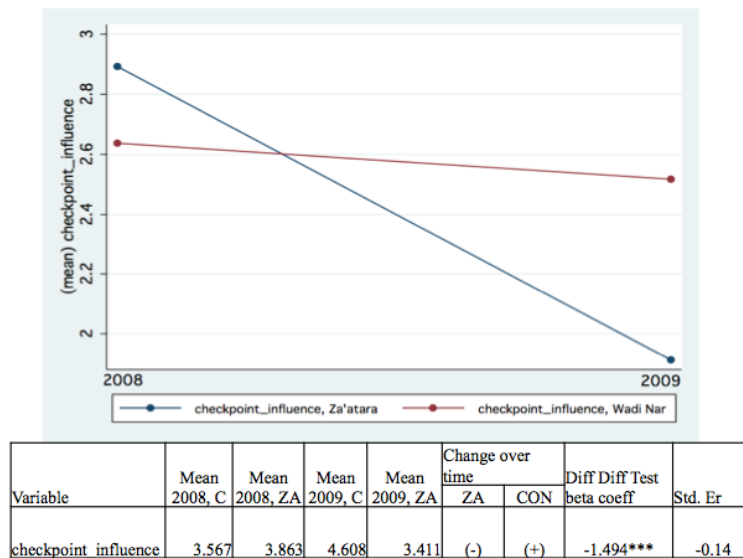
A second auxiliary finding derives from an explicit question in our questionnaire, *Direct Checkpoint Influence*, which asked respondents to reflect on their own exposure to checkpoints and how it has impacted their political attitudes.<sup>14</sup> This variable reveals that as a result of easement, respondents in Za’atara (treatment) were far more likely than in

<sup>14</sup> This is a leading question, which, taken independently, would produce weak findings; however, it is a valuable supporting measure. Importantly, it was asked *after* all the other questions raised in this study, and thus can in no way bias the previous inquiries.



Wadi Nar (control) to support resolving the conflict using peaceful means rather than violent ones [see Figure 8].

Figure 8: Diff-in-Diff Estimation, *Direct Checkpoint Influence*



### Identification & Concerns Addressed

A natural experiment is an intervention in ‘nature’ that approximates the qualities of a controlled experiment, with an exogenous force creating a facsimile of random assignment (Robinson, McNulty, and Krasno 2009: 342). This design enables causal claims in complex regions, but places onus on the authors to prove experimental conditions obtained. In this section we address challenges to identification [For reference to the CONSORT checklist, see SI A.3].

The first set of concerns pertains to research design. There are two issues worth treating here: endogeneity and blinding. First, was the shock (policy change) really exogenous, or were the particular checkpoints slated for easement selected for reasons that might contaminate our study? We contend that the Jenin First Initiative was *as-if* random – or *orthogonal* to our survey – because the decision to ease checkpoints in Jenin was not made due to underlying characteristics within the local Palestinian populations. As articulated above, the Jenin First Initiative was motivated primarily by economics. The Jenin district was home to a pre-existing Industrial Park, which, while defunct, had the greatest potential for hasty renovation. Thus, the point of policy discontinuity is based on the geographic locations of a business district, rather than on pre-existing historical, ethnic or political lines.<sup>15</sup> Concerns over endogeneity – i.e. that Jenin was chosen because the local populations were less militant, thereby confounding identification – are further mitigated empirically, as the area around Jenin, including the Jenin-Nablus and Jenin-Tulkarm corridors, has faced some of the *most* terrorist-related activity in the West Bank in the years leading up to the initiative (Stack 2003; Issacharoff 2007). This is made further evident by our baseline checks on opinion and demographic variables [see Figure 1], which demonstrate that the population surrounding Za’atara was *ex ante* more militant than Wadi Nar. Had the selection of checkpoint easement been based on security concerns, rather than the strict geographic criteria of business sectors, then Wadi Nar would have been eased rather than Za’atara. On both accounts it is clear that the Quartet-initiative can be considered *orthogonal* to attitudes about violence, militancy or peace –

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<sup>15</sup> Further, because the Initiative was a pilot, it was discontinued ‘arbitrarily’ – driven by outside considerations – leaving all checkpoints in the Jenin corridor “treated” and those outside “untreated”.

i.e. it is *as-if exogenous*.<sup>16</sup>

The second concern regarding research design pertains to *blinding*. Double-blind experiments are the gold-standard of scientific research design, and are not just achievable in lab settings. An experiment is said to be double-blind if neither the individuals studied nor the researchers collecting data are aware of the treatment assignment. Our research procedures meet these standards. First, while there was diplomatic discussion about checkpoint easement, there was *little or no* knowledge amongst Palestinians about these talks, and certainly no belief that such a policy would transpire. This is essential, because if Palestinians were aware that checkpoints were to be eased their attitudes might shift accordingly – thereby violating the exclusion restriction that experimental outcomes vary solely as a result of treatment, as opposed to the *expectation* of treatment. To pre-empt this concern, we ran a pre-sample and focus group in each site and found that not only was there no expectation of checkpoints being eased, there was *utter disbelief* that such a policy would be enacted. In addition, we reviewed major Palestinian newspapers in the three weeks prior to initial sampling, which revealed

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<sup>16</sup> Of course, one might contend that the *original* Industrial park (1999-2000) was situated in Jenin due to factors that correlate with local Palestinian attitudes. However, as this park was introduced in the late 90s – prior to the second *Intifada* – it is hard to imagine that the same preferences would obtain (or even that the population was the same) a decade later, rendering any correlation specious. Moreover, as Timothy Williams, Movement & Access Advisor, Office of the Quartet Representative, explained to us in an interview, this was very much a local initiative, borne of the personal relationship between the Mayors of Jenin and the Israeli Gilboa Regional Council – it was decidedly not part of an integrated West Bank strategy (Williams 2013), such that one region was chosen over another, thereby placing any argument about endogeneity on weak logical footing.

that no mention was made about easements.<sup>17</sup> Second, the survey enumerators were not informed of the study's experimental design. As the survey included questions on numerous topics, there was no indication of any emphasis on checkpoints or militancy.

An additional set of concerns pertains to outcomes – i.e. problems in the execution and results of our study. The most critical issue here is that of contamination, or that some members of the control group likely became aware of the easement in the treatment – i.e. the “John Henry Effect”, or the failure of the parallel trend assumption. Indeed, for several of our measures – including *militancy* and *extremism* – some of the significance of the result derives from the fact that while the treated group became *less* likely to support violence, the control group also became *more* likely to support violence. This is a serious concern, as it challenges the *experimental nature* of our study. Of course, every natural experiment faces potential contamination, as subject populations cannot be isolated as in a lab (Druckman et al 2006; Dunning 2008) – this is especially true in such a complex environment as the West Bank. However, we have reason to believe this concern is not especially damning to our study.

First, and most importantly, the fact that Wadi Nar changed values between  $t_0$  and  $t_1$  does not mean it must be discarded, or even that it shifted due to contamination. Rather, the fact that the control revealed an increase in attitudes towards militancy may simply be

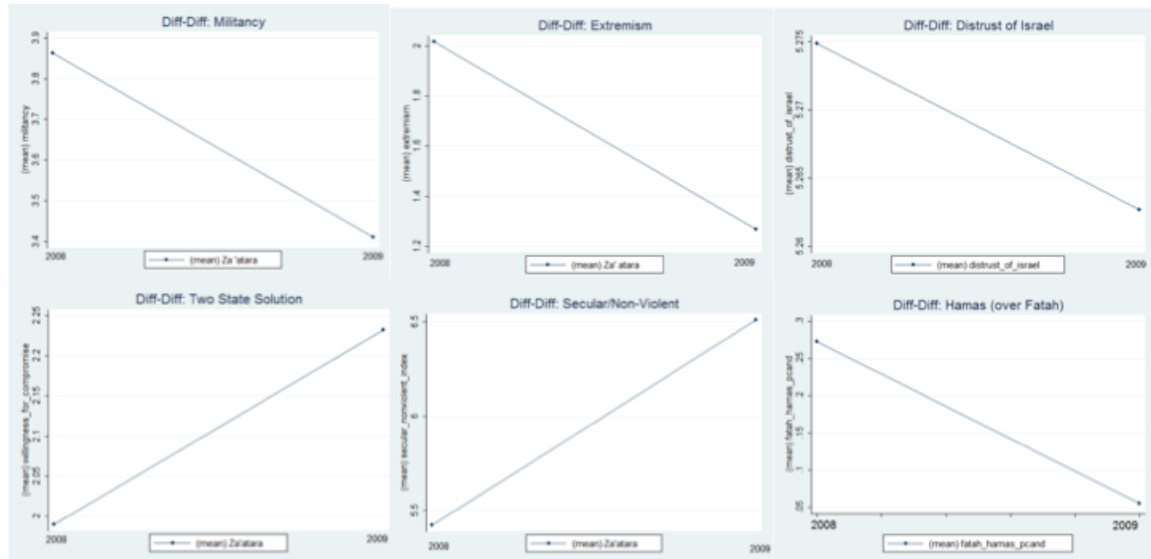
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<sup>17</sup> This confirms our priors, as checkpoint easements had been discussed by the Israeli government for many years prior without avail – to the point where such plans were labeled by Israeli human rights NGOs as “false promises” (Barsella 2007). As a final testament to how unfathomable the easement of checkpoints was, even the UN remarked on its impossibility as late as February 2009. In its *Humanitarian Monitor*, it decried that the Israeli checkpoint regime, originally justified as a temporary response to the Palestinian intifada, was evolving into “a more *permanent system* of control” (OCHA 2009a).

the expected trajectory of a site – any site – that is subject to adverse conditions, a finding common to deprivation theory (see e.g. Gurr 1970). In this case, we know that during our experimental window, settlements increased throughout the West Bank [See Figure 3], and there was no easement of checkpoints in the control area. Thus we might *expect* a heightened baseline between  $t_0$  and  $t_1$  as a result of natural trajectory alone. On this point, the fact that attitudes towards militancy might be expected to rise across the West Bank between  $t_0$  and  $t_1$ , only further strengthens our main findings – i.e. that militancy went down in Za’atara as a result of checkpoint easement.

Second, in the event that there was contamination of the control, while this affects the degree to which our study is plausibly experimental, it does not implicate the substantive thrust of our findings. If in fact, Wadi Nar is not to be considered a control and our study is purely observational, we still reveal decreasing support for militancy in Za’atara as a result of easement. The question is whether pre/post measures in Za’atara produce significant findings *over and above the shift in Wadi Nar*. In fact they do, as rendered in Figure 9 – with the exception of *Distrust of Israel*, which mirrors the null finding above. This reveals that even given the shift in control, this was not *driving* our findings; rather, they stand on their own. Thus, there are good reasons to believe the veracity of this study, even in the event of contamination.

Figure 9: Pre-/Post- Study of Checkpoint Easement around Za’atara



Variable	Mean 2008, ZA	Mean 2009, ZA	change over time ZA	Beta coeff	Std. Er
Militancy		3.863	3.411 (-)	-0.452***	-0.102
Extremism		2.018	1.268 (-)	-0.750***	-0.143
Distrust of Israel		5.275	5.263 (-)	-0.0122	-0.107
Two State Solution		1.99	2.232 (+)	0.242***	-0.076
Secular/ Non-Violent		5.425	6.51 (+)	-1.085***	-0.372
Hamas (over Fatah)		0.273	0.055 (-)	-0.196***	-0.0718

In addition to the John Henry Effect, two smaller concerns about the results of this study warrant mention. The first pertains to covariate imbalance. The data used in this project are based on village-clusters; as such, there is no universal random assignment, only *within*-village randomization. As the data show, there are significant differences between Za'atara and Wadi Nar. Therefore we cannot reject the null hypothesis that there is a difference in means. We don't consider this to be a significant issue, however, as covariate imbalance is addressed explicitly through difference-in-difference estimation. But beyond this point, we have substantive reason to believe that this imbalance is not especially damning. First, Za'atara, our treatment site, was *ex ante* more militant and distrusting of Israel, which if anything *would bias our results downward*. Second, these sites match up well against a representative sample of the West Bank, mitigating

concerns that either site is exceptional in any de-stabilizing way. [For a more detailed discussion of external validity using additional data, see SI D].

Another concern pertains to confounding factors. One of the central problems natural experiments face is that “the rival hypothesis exists that ... some more or less simultaneous event produced the shift” (Campbell and Stanley 1963: 39) or that the treatment is “confounded with a nearly infinite number of alternative causal mechanisms that undermine any purported ‘experimentally valid’ inference” (Robinson, McNulty, and Krasno 2009: 348). That there were no major policy changes during the experimental window helps us avoid this problem of alternative explanations. But how can we be certain that attitudinal changes in the northern West Bank were derived from checkpoint easement, rather than other economic improvements as part of the Jenin First plan? We offer two answers. First, we know that checkpoint easement was the first step in the Initiative and so, since we sampled only a few months after this easement, there was not sufficient time for derivative economic improvement. Second, independent analysis a year later found the immediate economic benefits of the ECF plan to be minimal (Giambi 2009). Thus, at least as regards our study-window, it is fair to say that the changing attitudes came as a result of the easement of checkpoints alone.

### **Humiliation as a Mechanism**

Returning to the central motivating question of this paper, we seek not merely to show *whether* non-violent forms of repression motivate obeisance or dissent, but *why* they

might do so. Thus far we have provided a window into how checkpoint easement affects Palestinian political preferences. In this section, we explore the *mechanism* that underlies this relationship. What is it about a non-violent form of repression, such as a checkpoint, that drives Palestinians to support violence? In the introduction we suggested that non-violent repression might spur feelings of humiliation, which in turn would prompt support for political violence. We can test this hypothesis, as our experimental survey included the question: “Which of the following aspects do you find *most troubling* about checkpoints?” (Responses: ‘length of delay’; ‘humiliation of the experience’; ‘violence’; ‘uncertainty of opening times and locations’; ‘financial loss’). Of these, a little more than one in three (37%) chose humiliation [For descriptive statistics see SI E.1].

This data confirms the first part of this equation – i.e. that the experience of checkpoints can be considered humiliating. We also find support for this claim qualitatively, through our field-research. For example, Abu Hashhash, a Palestinian field officer for the Israeli human rights organization B’Tselem, explained to us:

The point [of checkpoints] is humiliation. It is to harden your life, to make your life hell ... Humiliation can be as simple as being made to stand – to stand, just looking, knowing nothing about when you will be released ... You are not under arrest. But they can do whatever they want. They can forbid you from smoking, or from talking to your friend next to you ... you have to hold things on your shoulder, waiting for gates to be open, in the sun or sometimes in the rain. This is humiliation ... And all of this can be avoided – it just takes a second to check in the computer to see if someone can pass or not pass. But instead they stop people for two or three hours for nothing, and then after they are completely tired, they let them go (authors’ interview, Hebron 2009).

Similar evidence exists in anthropological accounts. For example, Avram S. Bornstein describes the harm of checkpoints as their capacity to “harass” (Bornstein 2002: 204; see also Efrat 2006: 85; Hammami 2004: 26). In addition, a prominent psychology study found that of the many repressive institutions in the West Bank, Palestinians considered



checkpoints the most humiliating – above, for example, settlements, land grabs and house demolitions (Ginges and Atran 2008: 285).

Even some quarters of the Israeli military decry the humiliation at checkpoints as excessive and counterproductive. For example, Judge Advocate General of the Israeli Defense Forces (IDF) Menachem Finkelstein, admitted that “there were many – too many – complaints that soldiers manning checkpoints abuse and humiliate Palestinians and that the large number of complaints ‘lit a red light’ for him” (Ha’aretz 2003). He was speaking from a strategic vantage, concerned with the potential danger this might cause Israelis down the road.

These accounts suggest a link between checkpoints and humiliation. But what can this tell us about militancy? Returning to our data, we can use the same question cited above as a mediating variable between exposure to checkpoints and *militancy*, to ascertain which type of harm (if any) predicts support for violence. We find a direct correlation between those who selected “humiliation” and support for violence against Israel. Indeed, it is the only response with a significant, positive correlation [Figure 10].<sup>18</sup>

Figure 10: Testing for Heterogeneous Treatment Effects

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<sup>18</sup> The non-response option (“Don’t know/No opinion”) also proved significant, but there are not enough observations (17 total) to glean much from this.

<b>Humiliation and Militancy</b>						
<b>Checkpoint Effect Model</b>						
Diff-Diff Beta	-1.486***	-1.533***	-1.496***	-1.473***	-1.498***	-1.476***
	-0.141	-0.142	-0.141	-0.14	-0.141	-0.14
Place: ZA vs. WD	0.304***	0.288***	0.296***	0.254***	0.300***	0.297***
	-0.0921	-0.0921	-0.0935	-0.0923	-0.0923	-0.0917
Year: 08 v09	1.041***	1.078***	1.041***	1.065***	1.042***	1.037***
	-0.0981	-0.0999	-0.0983	-0.0978	-0.0983	-0.0979
<b>"Harms" of Checkpoints</b>						
"Length of Delay" = 37%	-0.094					
	-0.0726					
"Uncertainty" = 10%		-0.229*				
		-0.121				
"Violence" = 10%			-0.0239			
			-0.116			
"Humiliation" = 37%				0.266***		
				-0.073		
"Financial Loss" = 2%					0.0153	
					-0.228	
Don't Know/ No Opinion = 3%						-0.559***
						-0.195
Constant	3.597***	3.586***	3.571***	3.475***	3.566***	3.583***
	-0.0693	-0.066	-0.0683	-0.0696	-0.066	-0.0653
Total Observations = 1,048						

This correlation between Palestinians who feel humiliated by the experience and support for violence maintains across robustness checks [see SI E]. This analysis reveals that militancy maintains its direction regardless of response-type, and that the coefficient for humiliation is larger and more significant than any other choices.

These findings suggest humiliation as a mechanism linking checkpoints to violence – affirming the central thrust of literature on humiliation, cited above, and helping contextualize our experimental results. Further, they offer insight into how, absent classical rationalist explanations of violence or economic loss, non-violent repressive institutions like checkpoints might nonetheless prompt subject populations to support militancy – a mechanism largely absent from the literature cited above.

## Conclusion

*“I always wished that the American commanders who set up these checkpoints could drive through themselves, in a civilian car, so they could see what the experience was like ... Is there a way to do checkpoints right? Perhaps, perhaps not. But it seems that the checkpoint experience perfectly encapsulates the contradictions and miseries and misunderstandings of everyone's common experience – both Iraqis and Americans – in Iraq.”*

– Annia Ciezadlo, *Christian Science Monitor*, 2005

Repression has many faces, as does dissent. In this paper we demonstrate how even non-violent, seemingly banal forms of repression have a considerable impact on the attitudes of subject populations. Looking specifically at checkpoints in the West Bank, we find that Palestinian populations subject to easement were significantly *less likely* to support violence against Israel, or the militant Islamist group, Hamas, as opposed to the secular-nationalist Fatah (considered the party of peace negotiations). This confirms a positive relationship between *non-violent* repression (impediments to mobility) and *violent* dissent (support for militancy). Additionally we have suggested a mechanism linking this non-violent form of repression with support for a violent response: namely, *humiliation*, which mediates the treatment effect on Palestinian attitudes, over and above more common explanations such economic loss or threats of violence. Together, these points constitute an important contribution to a political science literature focused almost exclusively on violent forms of repression and rationalist explanations for rebellion.

This paper also contributes to important debates within the policymaking community. There is little question that checkpoints are an impediment on the lives of Palestinians; but it is our noteworthy finding that they might be a detriment to Israeli and regional security as well by making Palestinians more likely to support violence and

radicalism, rather than diplomatic negotiation. Thus, *at best* the Israeli state is trading off long-term risk for short-term safety. In this way, we provide scientific support for what some in the policy community have long argued – namely that checkpoints are damaging to long-term security interests. For example, Eli Berman writes:

The current Israeli Defense Force (IDF) movement and closure regime in the West Bank is effectively designed to provide tactical security against terrorist attacks ... However, *the current approach to security exacts a considerable price* in terms of Israel's long-term security and political interests (Berman 2008: 4).

This is also the precise point made by the ECF, which places checkpoint easement at the center of its long-term security strategy in the Middle East (ECF 2008a: 2). Indeed, our findings indicate that a more expansive policy of checkpoint easement could have a considerable positive effect on Israeli-Palestinian peace negotiations. Of course, expectations must be tempered. Checkpoint easement alone, without corollary economic development or improvements to Palestinian daily life are likely insufficient to maintain long-term gain. Nonetheless, this research reveals how such a policy might contribute towards negotiations – a small step in a considerably rocky path.

Looking further afield, the subject of impediments to movement is especially pertinent now, given the recent experience of American-led administrations in Iraq and Afghanistan, replete with their own comprehensive regimes of internal transportation restriction, much of which remains intact. Indeed, there is ample journalistic evidence of subject populations' anger over checkpoints in Afghanistan (Oppel Jr 2010) and Iraq (Hussein 2008) – as evinced in the epigraph, above. Additionally, impediments to movement have made brief cameos in two recent academic works, the findings of which are in concert with our own. Berman et al (2011) argues that checkpoints designed to

suppress insurgent violence in Iraq and Afghanistan have had the countervailing effect of driving up unemployment, a predicate for violence; Condra et al (2010) places checkpoints in the rubric of combat in Iraq, as a non-violent encounter that produces a violent response, or what they term the “escalation of force”.

The point is not to disavow all checkpoints; rather, it is to inform a better understanding of how these institutions impact the populations they contain. At first glance, as non-violent means of social control, checkpoints seem banal. However, they perhaps deserve more attention than we currently pay them. Indeed, our study illustrates the broader point that in political science we are driven to study ‘events’; however, in doing so we often overlook the quotidian happenings of daily life – the empty spaces between those events. Such a miscue is unfortunate, as at its core politics is an everyday phenomenon. It is about micro-processes, even those not explicitly political – such as traveling from one point to another, unmolested by institutional fetters. By and large, we remain blind to these matters, perhaps the central loci of contestation.

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## Supporting Information (SI)

### SI A: Additional Figures & Tables

#### A.1 Checkpoint Typology

	TYPOLOGICAL DISTINCTIONS (UN Descriptions)
CHECKPOINT	Checkpoints are composed of two elements. First, an infrastructure obstructing vehicular and pedestrian traffic. The second element is the permanent presence of Israeli security personnel (e.g. the IDF, the Border Police, the civil Police, a private security company). Security personnel check the documentation of persons crossing the checkpoint and conduct searches on their vehicles and their belongings.
PARTIAL CHECKPOINT	Partial Checkpoints are made up of similar infrastructure as checkpoints but are not permanently staffed. Frequently the partial checkpoint infrastructure is installed on roadsides and therefore does not directly obstruct the traffic. When staffed, partial checkpoints function as the full checkpoints described above. When unstaffed, the traffic may flow relatively freely along the route.
EARTH MOUND	Earth mounds are mounds of rubble, dirt and/or rocks put in place by IDF bulldozers to prevent vehicle movement along a road or track. Several mounds less than 50 meters apart, blocking the same route, are only counted as one closure. If a mound is pushed to the side (by IDF or Palestinians) or if a route around it is created and vehicle access is possible, the mound is not recorded as an obstacle. Earth mounds are often removed or circumvented and then re-built and/or enlarged. Therefore, some of them appear on one map, disappear from the next and then subsequently reappear.
ROADBLOCK	Roadblocks are constructed from one or more concrete blocks about one meter cubed and like earth mounds are used to prevent vehicle access to land or roads.
TRENCH	Trenches (or ditches) are dug across flat land or along the side of a road to prevent vehicles going around a closure obstacle on the road.
ROAD GATE	Road Gates are metal gates used to block access to a route. Many of them are permanently closed whilst others are mostly open and only closed from time to time by the IDF.
ROAD BARRIER	Road Barriers may be composed by a continuous earth wall, a fence or a concrete barrier running along the side of a road. To be classified as a road barrier, this type of infrastructure should not be safety related, should be greater than 100 meters in length and obstruct free passage of people, vehicles or livestock, onto, off or across the road.

Source: "Closure Update: Occupied Palestinian Territory," OCHA. September 11, 2008.

#### A.2: List of Demographic Measures and Outcome Variables

<i>Variable Name</i>	Definition/ Significance	Questionnaire Derivations	Scaling
<i>Demographic Measures</i>			

<i>Age</i>	Age grouping	“How old are you?”	1 to 5 (brackets: 18-24; 25-34; 35-44; 45-54; 55+)
<i>Wealth</i>	Average monthly income (household)	“The average monthly income per household in the West Bank and Gaza is currently 2,500 NIS. Is your total household income (of both spouses)”	1 to 5 (“much below average” to “much above average”)
<i>Gender</i>	Gender	“Gender”	1 to 2 (“male” to “female”)
<i>Religion (Behavior)</i>	The practice of religion	How often do you perform each of the following activities? “Visit a mosque/church”, “Pray or read the Koran/Bible”, “Listen to religious programs on the radio or watch them on TV (including sermons of religious leaders)”	1 to 6 (“never” to “almost every day”)
<i>Religion (Ideology)</i>	Religious beliefs	“To what extent do you agree or disagree with each of the following statements: “Religion is the only way through which Palestinians can obtain their rights”, “I generally prefer to support political parties with a strong religious orientation”, “In the future Palestinian state, there should be no separation between religious law and state law”	1 to 6 (“strongly oppose” to “strongly support”)
<i>Outcome Variables</i>			
<i>Militancy</i>	Support for violence against Israel	“In times of threat to the Palestinians, it is important to take significant military action, even if it means harming innocents on the opposing side”; “In times of threat to the Palestinians, it is necessary that we overpower the enemy or destroy it”: Only by using force can you achieve anything in the Middle East”.	1 to 6 (“strongly oppose” to “strongly support”)
<i>Extremism</i>	Recognition of acts of terrorism	“Do you think the following operations are terrorist operations or not?: Amman hotel explosions; Egyptian explosions (Sharm al Sheikh/Dahab); London underground explosions; Madrid train explosions?”	0 to 1 (1 signifies “yes”, 0 signifies “no”)
<i>Two State Solution</i>	Support for the two-state solution	“What is your opinion about signing a peace agreement with Israel based on a two-state formula: (a) “including mutual recognition of Israel as the state of the Jewish people and Palestine as the state of the Palestinians people”; (b) “while forgoing return of the refugees into the state of Israel?”; (c) “while forgoing Palestinian sovereignty over parts of Jerusalem?”.	1 to 6 (“strongly oppose” to “strongly support”)
<i>Distrust of Israel</i>	Trust in Israel as a partner for peace.	“To what extent do you agree or disagree with each of the following sentences: “I do not believe in the peaceful intentions of the Israelis”; “I do not believe that Israelis will adhere to peace agreements signed with them?”	1 to 6 (“strongly oppose” to “strongly support”)

<i>Secular/Non-Violent</i>	Palestinian political party index	Comprised of answers to the question: "If elections to the Palestinian Legislative Council were held today, for which of the following would you vote?"; "If presidential elections were held today, and the following were presidential candidates, who would you vote for?"	1 to 8 (based on 2005/2006 electoral lists) <sup>19</sup>
<i>Hamas (over Fatah)</i>	Support for Hamas as compared to Fatah.	Same as above, but with all non-Fatah or Hamas votes coded as missing values.	0 to 2, ("Fatah" to "Hamas")
<b>Auxiliary Outcome Variables</b>			
<i>Political Disengagement</i>	Lack of interest in voting	Percentage of those who responded "Would not Vote," when asked about participating in elections for Palestinian Legislative Council or Palestinian Presidency (averaged).	0 to 1 ("no" to "yes")
<i>Direct Checkpoint Influence</i>	Effect of checkpoints on attitudes (self-reported)	"Have your experiences at the checkpoints influenced your position regarding the conflict with Israel?"	1 to 5 ("support for resolving the conflict peacefully" to "support for violent attacks")

<sup>19</sup> The party choices were as follows: Hamas (Islamic Resistance Movement); Fatah (Liberation Movement of Palestine); The Popular Front for the Liberation of Palestine; Democratic Front for the Liberation of Palestine; Palestinian People's Party; Palestine Democratic Union; Palestinian National Initiative; Third Way; Palestinian Popular Struggle Front; Palestinian Arab Front; Palestine Liberation Front; Wa'ad (National Coalition for Justice and Democracy); Palestinian Justice. Presidential choices (party in parenthesis): Mahmoud Abbas (Fatah); Ismaeel Haniyeh (Hamas); Salam Fayyad (Third Way); Khaled Misha'el (Hamas); Marwan Barghouthi (Fatah); Mustafa Barghouthi (Palestinian National Initiative).

<sup>19</sup> This variable took into account both support for the parties as well as their leaders, Ismael Haniyeh (Hamas) and Mahmoud Abbas (Fatah).

### A.3: Checklist of Items for Reporting a Randomized Trial (CONSORT)\*

Section	Item	Standard CONSORT Description	Specific Information for Checkpoint Study	Page No.
<b>Title and abstract†</b>	1	How participants were allocated to interventions (e.g., “random allocation,” “randomized,” or “randomly assigned”)	Population adjacent to Wadi Nar and Za’atara were arbitrarily assigned to treatment or control in the course of ‘Jenin First Initiative’.	1-4
<b>Introduction</b>				
Background	2	Scientific background and explanation of rationale	Scientific and historical background is described in paper.	5-11
<b>Methods</b>				
Participants/Subjects†	3	Eligibility criteria for participants and the settings and locations where the data were collected	The natural experiment is based upon checkpoint easement. The checkpoints are the experimental subject, the participants are individuals who happen to be proximate to either treatment or control checkpoints. The data are collected via individual surveys at the residences of randomly selected participants.	12-16
Interventions†	4	Precise details of the interventions intended for each group and how and when they were actually administered	The ‘Jenin First Initiative’ is the intervention administered to treatment and control populations based upon an arbitrary point of policy discontinuation.	16-17
	4A	Description of the different components of the interventions and, when applicable, descriptions of the procedure for tailoring the interventions to individual participants	The intervention had the effect of reducing state imposed travel restrictions for the treatment group (Za’atara) and leaving the control group (Wadi Nar) unaltered.	
	4B	Details of how the interventions were standardized	The easement of Za’atara checkpoint is a single intervention and thus standardized.	
Objectives	5	Specific objectives and hypotheses	The objective of this study is to determine the influence of travel restrictions on public opinion. The hypothesis is that the continuation of checkpoints adversely related to public opinion, whereas the easement of checkpoints improves public opinion.	5-8; 11-13
Outcomes	6	Clearly defined primary and secondary outcome measures and, when applicable, any methods used to enhance the quality of measurements (e.g., multiple observations, training of assessors)	The primary outcome measures are the surveys on public opinion. The secondary outcome measures are the 4-wave panel study of public opinion that is used for comparison to the primary survey.	13-14; 19-25; SI A.2
Sample size†	7	How sample size was determined and, when applicable, explanation of any interim analyses and stopping rules	Sample size is determined by local population proportion.	13

Randomization– sequence generation†	8	Method used to generate the random allocation sequence, including details of any restriction (e.g., blocking, stratification)	Point of discontinuation at Za'atara checkpoint, southernmost checkpoint to be eased. Local populations were assigned to treatment and control groups based upon location of their residences. Within local assignment, participants were chosen via the Kish method.	13
Allocation concealment	9	Method used to implement the random allocation sequence (e.g., numbered containers or central telephone), clarifying whether the sequence was concealed until interventions were assigned	People in the two sites had no prior knowledge of the 'intervention itself, (Jenin First Plan.) or how it was to be allocated (treatment vs. control).	13
Implementation	10	Who generated the allocation sequence, who enrolled participants, and who assigned participants to their groups	The allocation of treatment was generated by the "Jenin First Plan," the enrolment of individual survey participants was determined by Jamil Rabah, Near East Consulting.	13
Blinding (masking)†	11A	Whether or not participants, those administering the interventions, and those assessing the outcomes were blinded to group assignment	Parties involved in administering the Jenin First Plan' were completely unrelated to this study and did not have public opinion in mind when administering the policy. The surveyors were not aware of the purpose of the study, and the survey was sufficiently long and broad enough to indicate an underlying purpose.	27-28
Statistical methods†	12	Statistical methods used to compare groups for primary outcome(s); methods for additional analyses, such as subgroup analyses and adjusted analyses	The statistical method for evaluating the primary outcome is difference in difference estimation. Subgroup analysis is also conducted to detect heterogenous treatment effects and mediating factors.	18-19
<b>Results</b>				
Participant flow†	13	Flow of participants through each stage (a diagram is strongly recommended)---specifically, for each group, report the numbers of participants randomly assigned, receiving intended treatment, completing the study protocol, and analyzed for the primary outcome; describe deviations from study as planned, together with reasons	Pre-treatment survey conducted in Fall 2008, (Treatment = 302; Control = 297) Post-treatment survey conducted in Fall 2009 (Treatment = 219; Control = 234)	13; 15
Implementation of intervention†	New item	Details of the experimental treatment and comparator as they were implemented	n/a	
Recruitment	14	Dates defining the periods of recruitment and follow-up	Pre-treatment survey occurred Fall 2008, policy intervention occurred May/June 2009, post treatment survey occurred Fall 2009.	13
Baseline data†	15	Baseline demographic and clinical characteristics of each group	Baseline demographics are presented in detail, employing both primary and secondary data sources.	13-14

Numbers analyzed	16	Number of participants (denominator) in each group included in each analysis and whether analysis was by “intention-to-treat”; state the results in absolute numbers when feasible (e.g., 10/20, not 50%)	Random samples Pre-treatment (T = 302; C= 297); Post-treatment (T = 219; C = 234) Intent to Treat (ITT) not applicable since those residing near treatment or control were fully treated or not by default.	13
Outcomes and estimation	17	For each primary and secondary outcome, a summary of results for each group and the estimated effect size and its precision (e.g., 95% confidence interval)	Primary and secondary results presented in Figures 7,8, and 13.	19-25
Ancillary analyses	18	Address multiplicity by reporting any other analyses performed, including subgroup analyses and adjusted analyses, indicating those pre-specified and those exploratory	Ancillary analyses are presented in a discussion on mediation	31-34; SI C; SI E
Adverse events	19	All important adverse events or side effects in each intervention group	n/a	
<b>Discussion</b>				
Interpretation†	20	Interpretation of the results, taking into account study hypotheses, sources of potential bias or imprecision, and the dangers associated with multiplicity of analyses and outcomes	We argue that the statistical results indicate that public opinion is sensitive to changes in travel restrictions, both in a positive and negative sense. We also address concerns to identification.	19-31
Generalizability†	21	Generalizability (external validity) of the trial findings	External validity is treated in a section of its own.	30-31; SI D
Overall evidence	22	General interpretation of the results in the context of current evidence	See conclusion	35-37

\* CONSORT = Consolidated Standards of Reporting Trials.

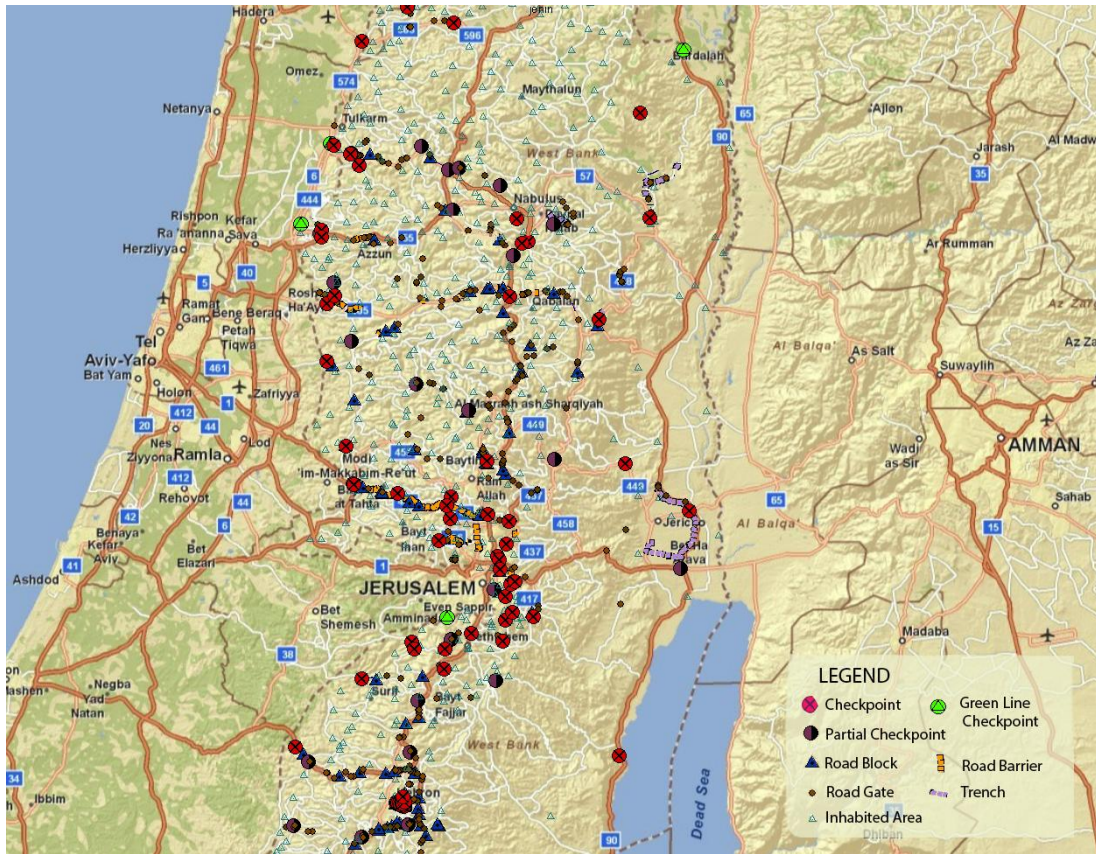
† This item was modified in the 2007 revised version of the CONSORT checklist.

## **SI B: Map Appendix**

The maps provided in this appendix are intended to qualify some of the substantive points in the text. Most specifically, they assist in the setting up of our research design and in identification. The first map, B.1 presents the full quantity of road closures in the West Bank, with full checkpoints denoted by red circles inset with black Xs. This map is illustrative in that it plots the checkpoints across the entirety of the West Bank, not simply our experimental sample, thereby establishing the political context to which our study contributes. Checkpoint placement also highlights the central thoroughfare that runs North to South (known as the Jenin-Hebron corridor) on which both of our experimental sites are located.

### **B.1 Simplified West Bank Map, with Closures (2009)**

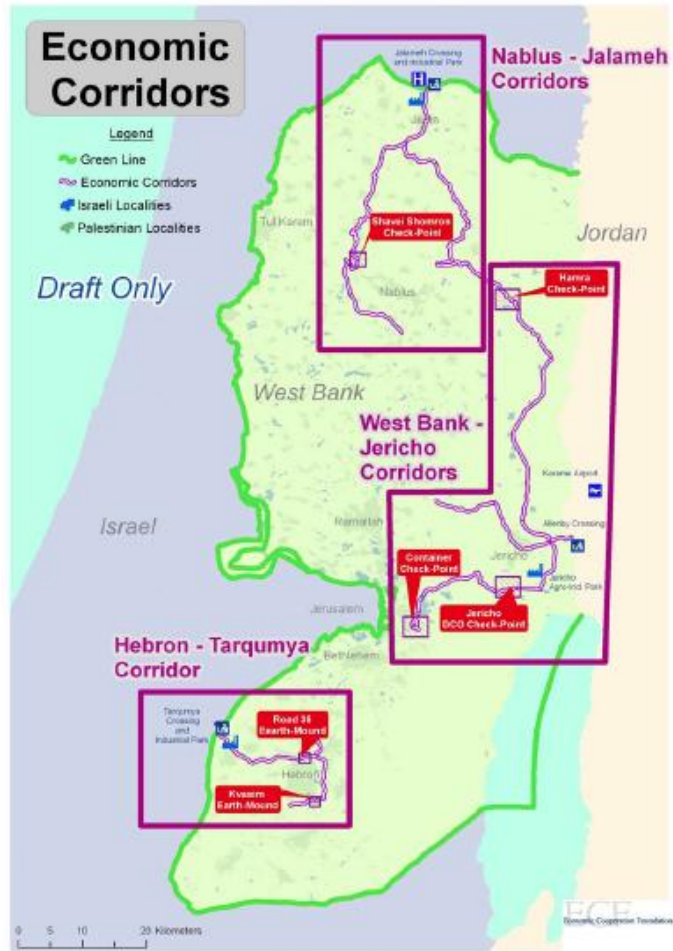




The second map in this appendix, B.2, looks specifically at three so-called “Economic Corridors” originally highlighted by the ECF as possible areas for economic development in 2009. Ultimately the Nablus-Jalameh Corridor (which passes through, and centers upon the development of, the city of Jenin) was selected to go first – i.e. the “Jenin First Plan”, with the others to follow.

B. 2: Simplified West Bank Map, with Economic Corridors (2009)<sup>20</sup>

<sup>20</sup> This Economic Cooperation Forum map was cited in Berman 2008: 42.



## SI C: Non-Response Appendix

All variables used in this study are coded on a set scale, based on responses recorded. No “Don’t know/no opinion” option was provided to respondents. In the event that a respondent chose not to respond, it was coded as a missing value and excluded from the data-set. By and large this was an unlikely outcome, and thus missing values are dropped from our analysis [For Descriptive Statistics See C.1]. However there are two notable exceptions to this: first, as regards the *Extremism* variable, and second as regards our two variables derived from party choice, *Secular/Non-Violent* and *Hamas (over Fatah)*. Larger quantities of non-response are understandable in these cases.

### C.1 Non-Response Rates by Outcome Variable

Variable	Full Sample						2008		2009	
	# obs	# missing	Mean	Std. Dev.	Scale	% missing	# obs	% missing	# obs	% missing
Militancy	1047	5	3.853	1.201	(1-6)	0.5%	598	0.2%	449	0.9%
Extremism	819	233	0.346	0.390	(0-1)	22.1%	491	18.0%	328	27.6%
Distrust of Israel	1049	3	5.109	1.282	(1-6)	0.3%	598	0.2%	451	0.4%
Two State Solution	1049	3	2.164	0.970	(1-6)	0.3%	597	0.3%	452	0.2%
Secular/Non-Violent	651	401	5.312	3.237	(0-8)	38.1%	364	39.2%	287	36.6%
Hamas (over Fatah)	561	491	0.364	0.694	(0-2)	46.7%	298	50.3%	263	41.9%

Full Sample = 1052, 2008 = 599, 2009 = 453

With regards to *Extremism*, this variable asked several questions about events that exist outside of the West Bank – including some in Europe, such as about the Madrid bombings. It is not surprising that fewer respondents will have heard of these cases, or

feel comfortable discussing them. The way that extremism was calculated was such that all answers were coded (0-1) and averaged. If anything, this means the extremism effect would be understated. The missing values are simply left out. This means of tabulations assumes that there is no information provided by those who chose not to respond – i.e. that they simply did not understand the question or feel familiar with the cases. The assumption that most missing values relate simply to the lack of knowledge of the question is confirmed by the question-by-question breakdown, which shows that respondents were more likely to not respond to the later questions (which pertain to Europe, more distant) than the preceding questions (which pertain to the Middle East, more proximate) [See C.2].

### C.2 Non-Response Relationship along Each *Extremism* Question

	Mean	Mean	Mean	Mean	change over time		Diff Diff Test	
	2008, ZA	2008, WD	2009, ZA	2009, WD	ZA	WD	beta coeff	Std. Er
Q1	0.294702	0.1010101	0.2785388	0.15812	(-)	(+)	-0.0733	-0.0494
Q2	0.3509934	0.1077441	0.3333333	0.200855	(-)	(+)	-0.111**	-0.0521
Q3	0.4569536	0.1952862	0.5022831	0.34188	(+)	(+)	-0.101*	-0.0582
Q4	0.4238411	0.2188552	0.4748858	0.457265	(+)	(+)	-0.187***	-0.0593
Any missing	0.5331126	0.3097643	0.5388128	0.534188	(+)	(+)	-0.219***	-0.061

However, these questions, which ask respondents to identify as terrorist numerous actual acts of violence, are also extremely politically sensitive. This might make respondents less likely to respond even if they have heard of the cases. However, if anything this will make respondents more likely to downplay extreme views out of fear of retribution – thereby understating our findings. Indeed, as revealed in Table C.3 below, non-responses

do move in the same direction as *Extremism*, which confirms our priors that there is more latent extremism than is present in our sample.

### C.3 Difference-in-Difference Between Non-Response and Extremism

	Q 1	Q2	Q3	Q4	Any missing
Diff-Diff Beta	-0.0733	-0.111**	-0.101*	-0.187***	-0.219***
	-0.0494	-0.0521	-0.0582	-0.0593	-0.061
Place: ZA v WD	0.194***	0.243***	0.262***	0.205***	0.223***
	-0.0324	-0.0342	-0.0382	-0.0389	-0.04
Year: 08 v 09	0.0571*	0.0931**	0.147***	0.238***	0.224***
	-0.0347	-0.0366	-0.0409	-0.0416	-0.0428
Constant	0.101***	0.108***	0.195***	0.219***	0.310***
	-0.023	-0.0243	-0.0271	-0.0276	-0.0284
Observations	1,052	1,052	1,052	1,052	1,052
R-squared	0.043	0.057	0.063	0.047	0.041

Finally, with regard to party choice, there are also numerous missing values. This is unsurprising, as missing values were part of the construct of the variable – especially in the case of *Hamas (over Fatah)* in which all smaller parties were excluded by design. Party choice is derived from two questions, first how respondents would vote for the Legislative Council, the other for Presidential candidate. However, because of the nature of the question, we can actually disaggregate responses, as non-responses had three enumerated types: “Would Not Vote,” “Undecided,” and “Refuse” [For Descriptive Statistics See Figure C.4].

### C.4 Non-Response by Party Question

Q1: Legislative Council

"Would Not Vote"	158	15.0%
"Undecided"	161	15.3%
"Refusal"	34	3.2%
Missing Value	48	4.6%
<b>Q2: Presidential Candidate</b>		
"Would Not Vote"	141	13.4%
"Undecided"	203	19.3%
"Refusal"	43	4.1%
Missing Value	24	2.3%

Because these non-responses cannot be coded either along secular/non-violent lines or by particular party, these are dropped from the reported variables. However, the fact that respondents did not answer the question does not mean that there is no information revealed in this data. For those that did not vote, we can once again expect latent extremism, as those respondents with more extreme views are less likely to express these opinion – thereby downplaying our findings. Indeed, this fact is borne out by the correlation between those who selected “would not vote” and support for militant views – both of which decreased in the treatment as a result of easement. This finding is reported in the main text [See Figure 7]; this relationship is further clarified in Figure C.5, below.

### C.5 *Political Disengagement*, Disaggregated

Variable	Mean	Mean	Mean	Mean	change over time		Diff Diff Test	
	ZA 2008	WD 2008	ZA 2009	WD 2009	ZA	WD	beta coeff	Std. Er
Would not Vote -Legislative	27.8%	7.4%	15.5%	7.7%	(-)	(+)	-0.126***	0.0433
Would not Vote -Presidential	20.5%	9.4%	12.8%	9.8%	(-)	(+)	-0.0815*	0.0421
Not Vote-Average	24.2%	8.4%	14.2%	8.8%	(-)	(+)	-0.104***	-0.039

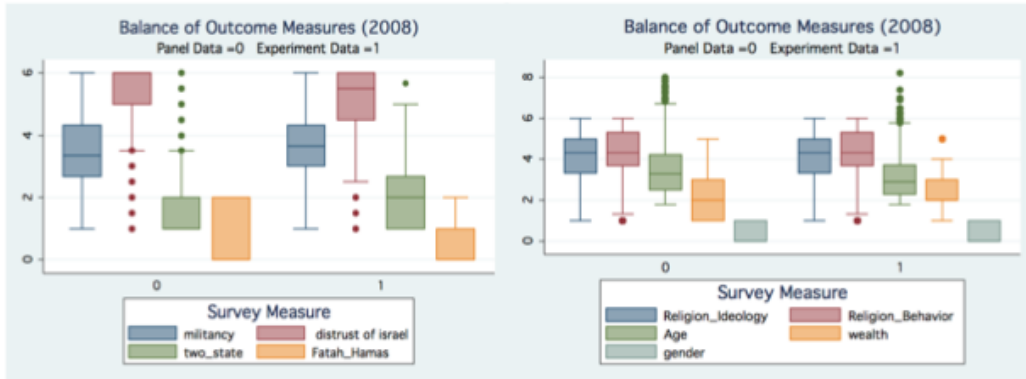
## SI D: Robustness Checks with Panel Data

In addition to challenges to identification, natural experiments also encounter concerns over external validity – i.e. do the findings produced by this study generalize to a broader population of units (see e.g. Campbell and Stanley 1963)? In this case, we might ask: what does the change in public sentiment at our experimental sites tell us about the impact of checkpoints across the whole of the West Bank? In this appendix we test the findings of the natural experiment against observational data, making use of a panel study (n=598), conducted by the Jerusalem Media and Communications Centre, which measures Palestinian attitudes in the West Bank across four time-periods, September 2007 – June 2009 (37.2% attrition rate).<sup>21</sup> This dataset is ideal for validating our experimental findings, as the questionnaires were designed in tandem; thus many of our variables – *Militancy*, *Two State Solution*, etc – are identical.

### D.1: Comparison of Experimental Sample and Representative Sample

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<sup>21</sup> Interviews were conducted face-to-face; randomization was effective, as the final sample parallels known population demographics in age, economic status, and sex (Palestinian Central Bureau of Statistics 2008).

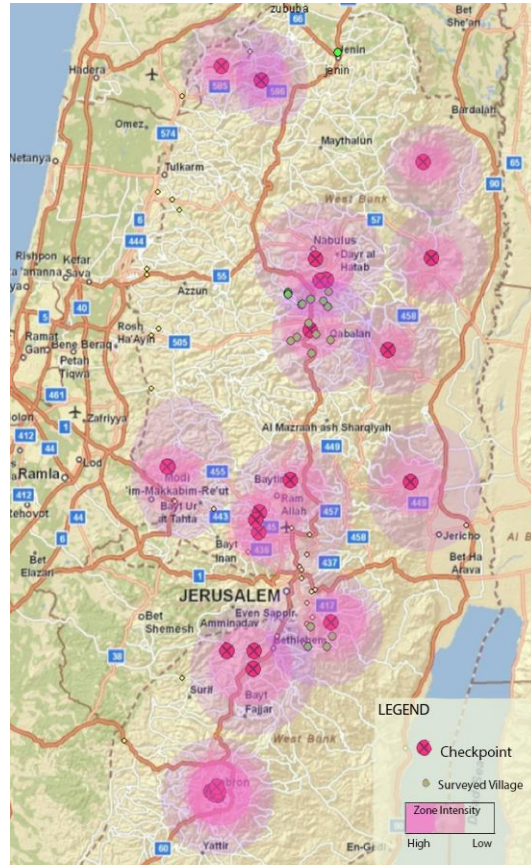


variable	Mean (panel)	Mean (exp data)	beta coeff
militancy	3.406	3.716	0.310***
distrust_of_israel	5.340	4.989	-0.351***
two_state	1.714	2.023	0.310***
Fatah_hamas	0.602	0.369	-0.233***
Religion_Ideology	4.228	4.108	-0.120*
Religion_Behavior	4.328	4.314	-0.0142
age	3.501	3.135	-0.365***
wealth	2.429	2.581	0.153**
gender	0.480	0.346	-0.134***

In D.1, we paired the two datasets against one another – using variables which can be considered directly comparable – finding significant imbalance. This was expected, as the experimental data focuses exclusively on checkpoint-impacted areas, whereas the representative data sampled the West Bank as a whole. In an effort to make the samples comparable, we placed our experimental sample alongside ‘checkpoint-impacted’ areas across the West Bank. To determine which areas were most impacted by checkpoints we measured the distances between villages and checkpoints in our representative sample and assigned an “impactedness” rating, based on the width of impacted areas on a three-point scale – a technique common to epidemiology [see D.2]. The subset of villages deemed “highly impacted” were then isolated and compared to our experimental sample.

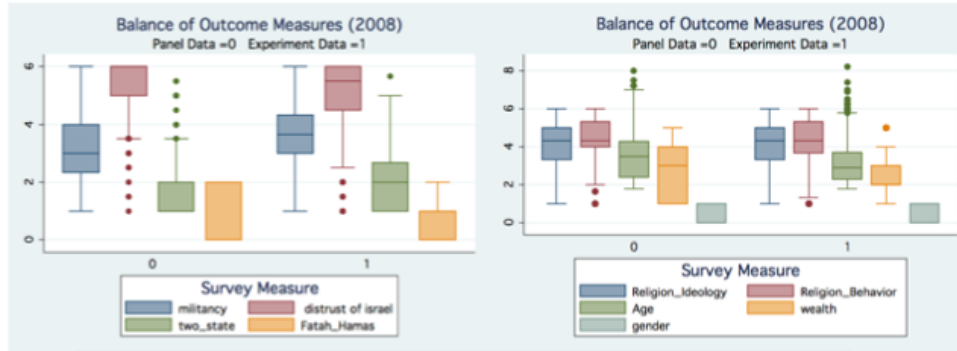
## D.2: ArcGIS Map of Checkpoint-Impacted Areas





When we look exclusively at areas within the representative dataset that are checkpoint-impacted, we find this imbalance considerably reduced [D.3]. This helps us counter concerns over external validity.

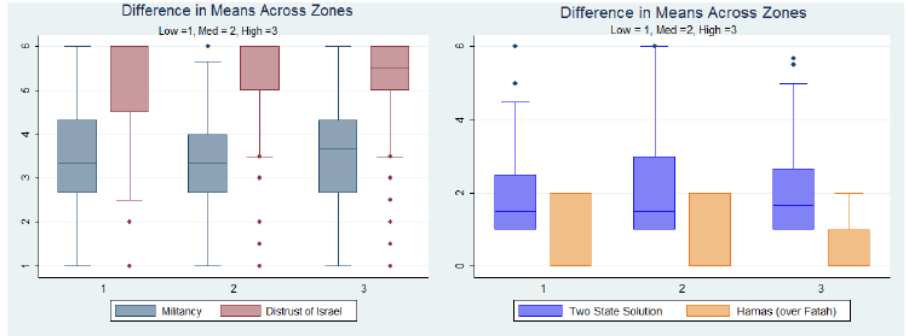
### D.3: Experimental and Representative Sample, Checkpoint-Impacted Areas Only



variable	Mean (panel)	Mean (exp data)	beta coeff
militancy	3.251	3.716	0.465***
distrust_of_israel	5.356	4.989	-0.367***
two_state	1.674	2.023	0.349***
Fatah_hamas	0.295	0.274	-0.0216
Religion_Ideology	4.195	4.108	-0.0876
Religion_Behavior	4.280	4.314	0.0343
age	3.585	3.135	-4.498***
wealth	2.565	2.581	0.0167
gender	0.475	0.346	-0.129***

Beyond merely asserting demographic parity, this data provides additional support for our findings, as we can use the 3-pronged ‘impactedness’ coding system to determine how Palestinian attitudes in highly impacted areas compare to more moderate levels. Figure D.4 reveals one significant finding, which is that respondents in the *high*-impactedness zones are less likely to support the “Two State Solution”. This finding corroborates our basic argument, in this case that checkpoints correlate with support for military solutions, rather than diplomatic ones – here replicated across the West Bank as a whole.

#### D.4: Difference in Means Testing Across Zone Intensity



Proximity	Low	Medium	High	B	Std. Err.
	Mean	Mean	Mean		
Militancy	3.413	3.349	3.583	-0.0846	-0.0691
Distrust of Israel	5.121	5.492	5.094	0.0668	-0.0583
Two State Solution	2.000	2.086	1.924	-0.211***	-0.0713
Hamas (over Fatah)	0.736	0.667	0.412	-0.101	-0.0771

## SI E: Humiliation Appendix

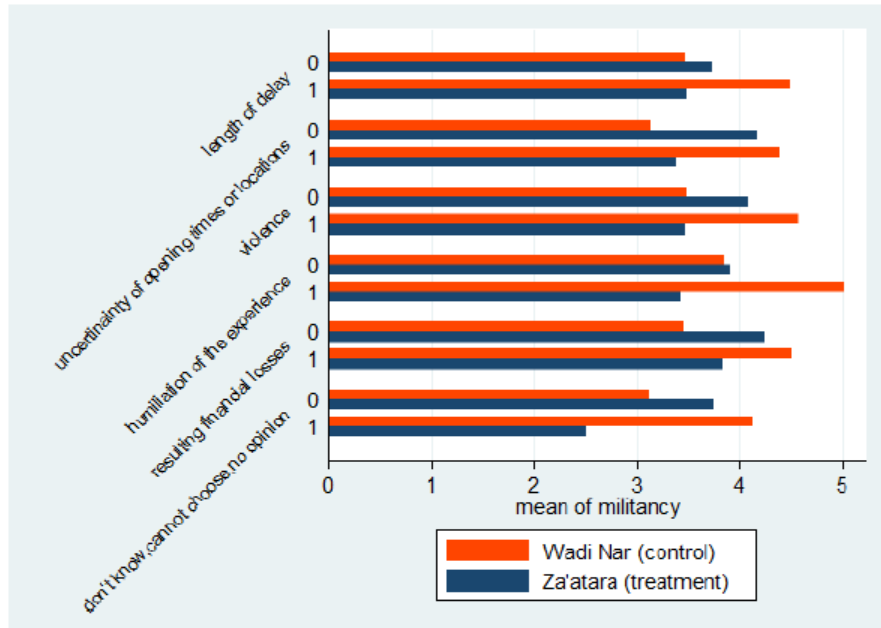
We suggest humiliation as the mechanism by which non-violent forms of repression produce support for violence. To support this point quantitatively, this appendix supplements this discussion with more data analysis. To begin, the basic descriptive statistics are provided in Figure E.1, taken from the sample as a whole.

### E.1 Descriptive Statistics

<b>Reported "Harm" Caused by Checkpoints</b>		
Length of delay	389	37.12%
Uncertainty of opening times or locations	103	9.83%
Violence	108	10.31%
Humiliation of the experience	389	37.12%
Resulting financial losses	25	2.39%
Don't know; Cannot choose; No opinion	34	3.24%
<b>Total</b>	<b>1,048</b>	

As this study is structured around a policy intervention, it is also helpful to view the basic changes in response, across site (Za'atara and Wadi Nar) and time period,  $t_0$  and  $t_1$ . Figure E.2 illustrates the relationships between the different responses and support for militancy over time.

### E.2 Humiliation and Militancy by Mediator: Graphic



Variable	Mean	Mean	Mean	Mean
	ZA 2008	WD 2008	ZA 2009	WD 2009
"Length of Delay" = 37%	3.745	3.474	3.490	4.493
"Uncertainty" = 10%	4.182	3.133	3.389	4.398
"Violence" = 10%	4.074	3.490	3.467	4.576
"Humiliation" = 37%	3.916	3.851	3.423	5.017
"Financial Loss" = 2%	4.350	3.462	3.833	4.500
Don't Know/ No Opinion = 3%	3.750	3.111	2.500	4.133

In the main paper, the results of this correlation between humiliation and support for militancy are broken down by question – i.e. taking each question to be its own model. Referring to Figure 10, we see humiliation stands out as having a significant, positive relationship with changes in militancy when applied as an indicator to our main model estimation. We did not include all of harms in a single model in Figure 10, since this would force us to arbitrarily omit one category. Instead, Figure 10 presented the results of six separate models each allowing a “harm” to be tested individually. The combined analysis was excluded from the paper; however, in this appendix there is space to perform a combined model to further highlight this finding [see Figure E.3]. Here we

present two iterations, with different dropped variables; the basic outcomes remain the same, with humiliation and militancy maintaining a significant, positive relationship.

### E.3 Humiliation and Militancy: Single Model

<b>Humiliation Militancy</b>		
<b>Checkpoint Effect Model</b>		
Diff-Diff Beta	-1.484***	-1.484***
	-0.141	-0.141
Place: ZA vs. WD	0.257***	0.257***
	-0.0937	-0.0937
Year: 08 v09	1.084***	1.084***
	-0.0994	-0.0994
<b>"Harms" of Checkpoints</b>		
"Length of Delay" = 37%		0.482**
		-0.199
"Uncertainty" = 10%	-0.154	0.328
	-0.129	-0.224
"Violence" = 10%	0.0255	0.508**
	-0.124	-0.221
"Humiliation" = 37%	{ 0.226***	{ 0.708*** }
	-0.0807	-0.2
"Financial Loss" = 2%	0.0678	0.550*
	-0.231	-0.295
Don't Know/ No Opinion = 3%	-0.482**	
	-0.199	
Constant	3.509***	3.027***
	-0.0816	-0.2
Total Observations = 1,043		