

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

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Does nonviolent 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 toward the “easement” of checkpoints—nonviolent 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 ($n = 1,200$). We find that easement makes subject populations less likely to support violence; we suggest humiliation as the mechanism bridging nonviolent 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.

The checkpoints prevented hundreds of terrorist attacks against the Israeli population.

—Israeli Ministry of Justice¹

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 Now²

Does nonviolent 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 it systematically overlooks nonviolent institutions; further, to date most research has been observational and inconclusive. In this article, we redress these shortcomings via a study of checkpoints—that is, nonviolent impediments to movement³—in the

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

²Hagit Ofra, cited in Hider (2007).

³Checkpoints are generally nonviolent in nature, designed toward 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—for example, because ambulances are prevented passage (World Health Organization [WHO] 2013). Otherwise, violence in the West Bank is quite limited, and checkpoints represent but a tiny portion of total incidents (OCHA 2008b).

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West Bank. According to the United Nations 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 unstaffed 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 and, on the other, that they perpetuate it. Which is it, and how can we tell?

We confront this problem by exploiting a natural experiment based on a policy intervention by the Tony Blair–led Quartet (i.e., the United States, the United Nations, the European Union, and Russia)⁴ 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; Card and Krueger 1994; Krasno and Green 2005; Miguel 2004; Miles and Rochefort 1991), using a village-based selection model akin to that employed by Posner (2004), with the noise of nonrandom assignment cleaned up via difference-in-difference estimation (Ashenfelter and Card 1985; Smith and Todd 2005). In doing so, it provides us with 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 unsystematic and incomplete (Brown 2004; Bornstein 2006; Kotef and Amir 2011; Naaman 2006), with econometric reports going so far as to claim that “the losses of internal closures are difficult to measure” (Aranki 2004, 28) or that “quantifying the economic impact of current restrictions is difficult given the paucity of data” (World Bank 2008, 1).⁵

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 values 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.⁶ Additionally, we test the robustness of our findings against an independent panel ($n = 1,200$) conducted in four waves between 2007 and 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 *nonviolent* 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

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

⁶There 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).

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

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 (Diamond 2002; McFaul 2002; Olson 1971; Ostrom 1998). Others posit that repression encourages rebellion by creating conditions so unbearable that noncombatants come to believe that insurrection is the only option (Francisco 1996; Gurr and Duvall 1973; Kalyvas 2006; Mason and Krane 1989; Tullock 1971; Wood 2003). Another set of authors claims the relationship is nonlinear. 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 (Davenport 1995; Davis and Ward 1990; Gurr and Lichbach 1986).

This debate remains inconclusive largely because of the tools employed toward 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 (Achen 2002; Heckman 2000; 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 nonviolent forms of repression. Indeed, despite frequently relying on definitions of repression that putatively include nonviolent forms, in nearly all existing studies, technologies of repression are violent. For example, while several authors place nonviolent considerations in their models (Gurr 1970; Lichbach 1987; Tullock 1971), these “events” poorly fit their specifications: Nonviolent 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 either resist because the status quo is too dangerous to maintain or obey because it has a lower cost than dissent (Kalyvas and Kocher 2007; Lichbach 1987; Wood 2003). How can these lessons apply to cases where *nonviolent* 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 nonviolent 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—that is, feeling unjustly demeaned, or subjugated by another, usually the result of asymmetric power relations (see, e.g., Ginges and Atran 2008; Hartling and Luchetta 1999; Lindner 2002). 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 (1994, 69); Harkavy asserts a “causal nexus between humiliation and revenge” (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; Muller and Weede 1994; Opp and Roehl 1990). 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 that humiliation has suppressed support for rebellion (Ginges 1997); numerous psychological studies have found that humiliation has an inhibiting effect on behavior (Holtgraves and Lasky 1999; Keltner et al. 1998). 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 nonviolent 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” that delimit the border between the Palestinian territories and Israel or that separate Palestinian communities and Israeli settlements.⁷ Checkpoints are a subset of a broader infrastructure of “closures” within the West Bank, which includes roadblocks, earth mounds, and gates (see A.1 in the online supporting information [SI]). 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).

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 United Nations 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 United Nations 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 reintroduced 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

⁷Additionally 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.

referred to as a “credibility threshold.”⁸ 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 toward 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.

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 upon political factors, but it 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, un-

⁸The 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 2008b, 3).

official conversations; ECF 2008b, 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,⁹ 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,¹⁰ 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 each household, respondents were selected according to the Kish method. Interviews were double-blind, as the enumerators were also unaware of the experimental design.

In Table 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 toward militancy and peace negotiations, drawing from existing studies, including studies on the Israeli-Palestinian conflict (Huddy, Khatib, and Capelos 2002; McAlister, Bandura, and Owen 2006; Nelson and Milburn 1999), focusing on attitudes toward peace, violence, and party affiliations (in this case, Fatah and Hamas; for further explanations, see SI A.2).¹¹ 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.

The two sites we use in our study, Za’atara (treatment) and Wadi Nar (control), were paired due to topological similarity, as they are considered to be equivalent both by UN classification (see SI A.1) as well as by the

⁹*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

¹⁰The second wave was intended to be a panel, with enumerators returning to the same households for a second round of interviews. However, a presampling 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.

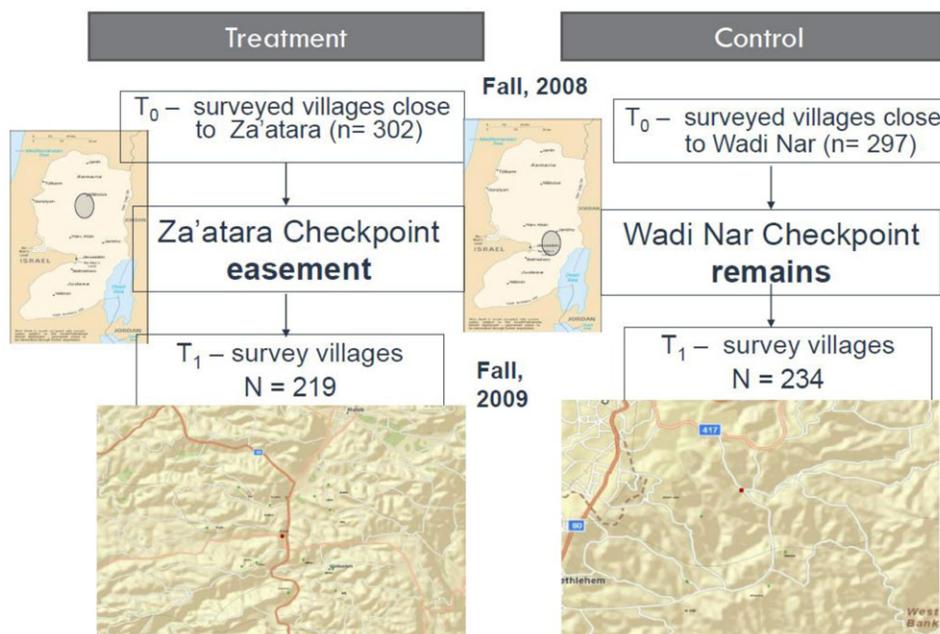
¹¹The variables used in this article constitute a subset of a larger questionnaire about Palestinian attitudes in the West Bank; all questions pertinent to this study are reported. The broader questionnaire of which this is a part is introduced in section D in the supporting information, in our discussion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

TABLE 1 Balance Checks for Opinion Measures and Demographics

Variable	Index	Mean (ZA 2008)	Mean (WD 2008)	p-value test
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

Note: ZA = Za’atara; WD = Wadi Nar.

FIGURE 1 Map of Za’atara and Wadi Nar Surveys



Note: ***p < 0.01, **p < 0.05, *p < 0.1.

ECF (2008a). They are also both on the central artery of the West Bank, the Jenin-Hebron corridor (see Figure 1; see also SI B.1). Za’atara¹² 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 (e.g., control

rooms, walkways, and watchtowers) as well as long lines of cars, with waits of up to an hour.

The essential point is that in the period leading up to and including our first survey sampling window, the sites were considered comparable—notwithstanding circumstantial differences like design, layout, and dimensions—both by Palestinian and international observers (see, e.g., Barsella 2007).¹³ Most importantly, the two sites did not

¹²In 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.

¹³There were, of course, minor changes in the surrounding areas during the preexperiment window, most notably the easement of Shave Shamron, a checkpoint near Za’atara. But if anything, this would bias results downward. Moreover, it is precisely because

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

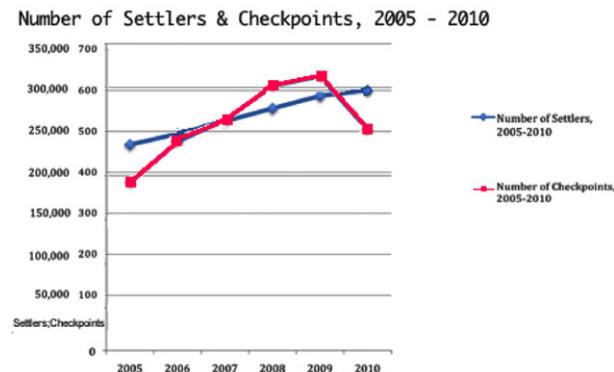
In May/June 2009, Israeli policy shifted radically, with the government deciding to open up the Jenin-Ramallah corridor, turning Za'atara into little more than a traffic circle. Sometimes members of the Israeli Defense Forces occupied a seat in the center of the crossing, but cars were not stopped. For 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:

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 (B'Tselem 2010; OCHA 2010). These official assessments were confirmed by the authors in person and in consultation with local analysts.

Za'atara remained that these small changes were largely superficial (OCHA 2008a).

FIGURE 2 Settlements and Checkpoints Compared, 2005–2010



Note: ***p < 0.01, **p < 0.05, *p < 0.1.

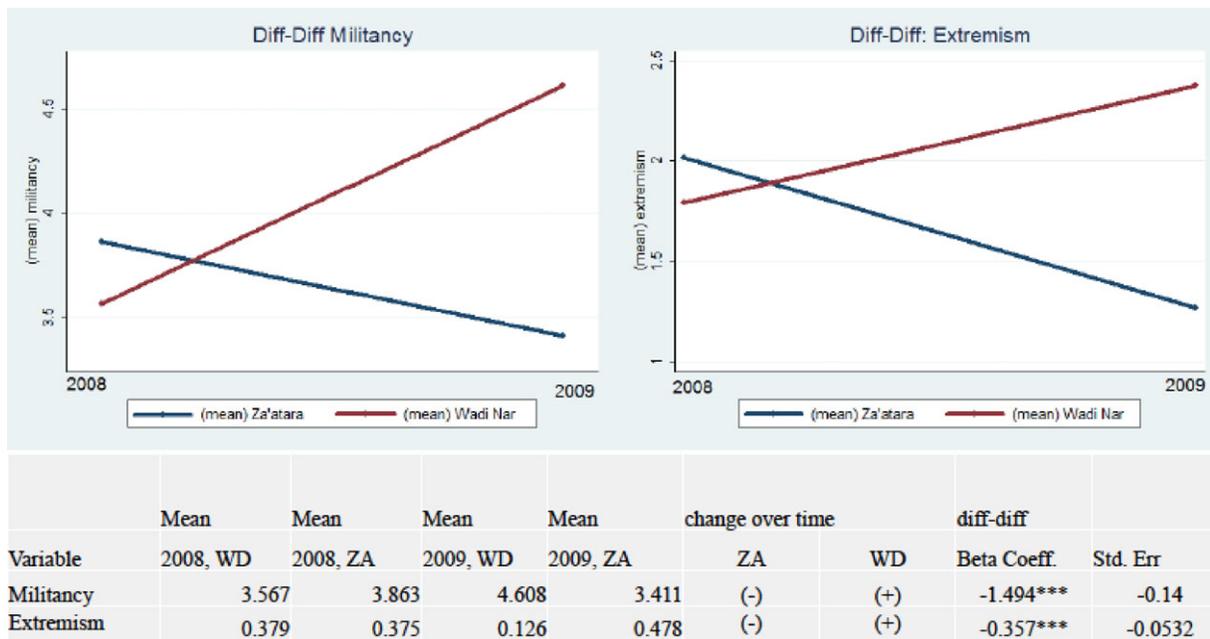
In addition, during this experimental window, there were no other significant changes to Israeli institutions in the West Bank (e.g., settlements, closed military zones) that might complicate identification. During the experimental window, settlements continued to develop at their normal clip, and access to land remained restricted (see Figure 2). According to the United Nations,

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 added)

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

FIGURE 3 Difference-in-Difference Estimation: *Militancy and Extremism*



Note: ***p < 0.01, **p < 0.05, *p < 0.1.

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 easement of the Za’atara checkpoint. Our model is as follows:

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

The dependent variable, Y, represents the survey questions on political responses. X₁ is the time variable indicating whether the year is 2008 (before treatment) or 2009 (after treatment). The X₂ variable denotes the location, Za’atara, which was treated in 2009, or Wadi Nar, which was never treated. X₁*X₂ is the interaction term, computed by multiplying X₁ (time) and X₂ (place). The difference-in-difference model includes the individual effects of variables X₁ and X₂ in order to test for their individual significance and not to confound the interpretation of the β₃ coefficient. Thus, the β₃ coefficient may be interpreted as the relative change in attitudes of the treatment group compared to the control, brought about by treatment.

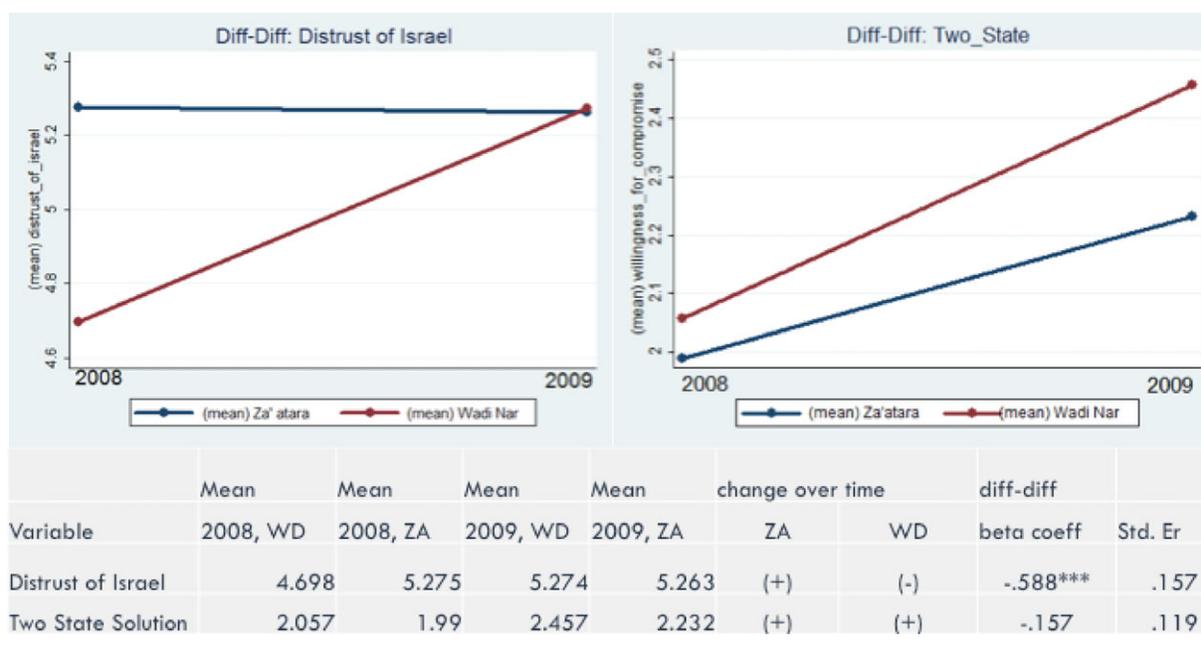
Findings

As a result of the checkpoint easement, public opinion in the treatment group shifted significantly and consistently toward less militant views. Our principle variable,

Militancy, taken as the mean of responses to a scale of questions regarding attitudes toward 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 3). This finding held true to a high degree of statistical significance (p > .01) and engendered more than a full point change (on a 6-point scale) away from violent attitudes toward 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 delocalized variable, *Extremism*, which measures Palestinian reaction to global acts of terrorism (Tessler et al. 2007).

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 section C in the supporting information).

FIGURE 4 Difference-in-Difference Estimation: *Distrust of Israel and Two-State Solution*

Note: ***p < 0.01, **p < 0.05, *p < 0.1.

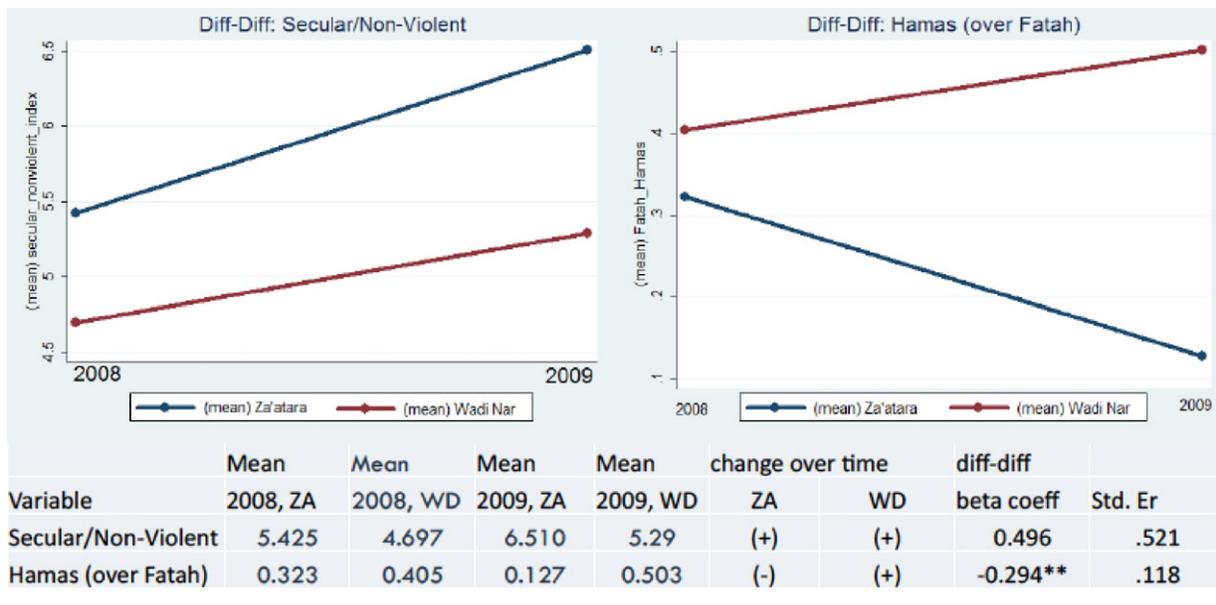
These militancy variables are complemented by two variables concerned with diplomacy (see Figure 4), although these findings offer considerably less clarity. *Distrust of Israel* asks whether Israel can be considered a trustworthy partner for peace (Nadler and Liviatan 2006). With *Distrust of Israel*, Palestinians in the treatment group were *less* inclined to express negative attitudes toward Israel after the checkpoint easement. 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,” that is, the diplomatic agreement in which Israeli and Palestinian states live side by side. Here, findings were not significant and were substantively ambiguous. While the population beside Za’atara did increase support for the two-state solution, support also increased within the control. This counterintuitive 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.

Our strong findings on militancy are echoed in support for Palestinian political parties (see Figure 5), drawn from the list of parties on the Palestinian Legislative Council ballot of 2006 and the candidates for the 2005 presidential elections. *Secular/Nonviolent* looks at the entire spectrum of Palestinian political parties, placed

on a linear index from “secular-nonviolent” 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 toward 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 and vice versa for Hamas).

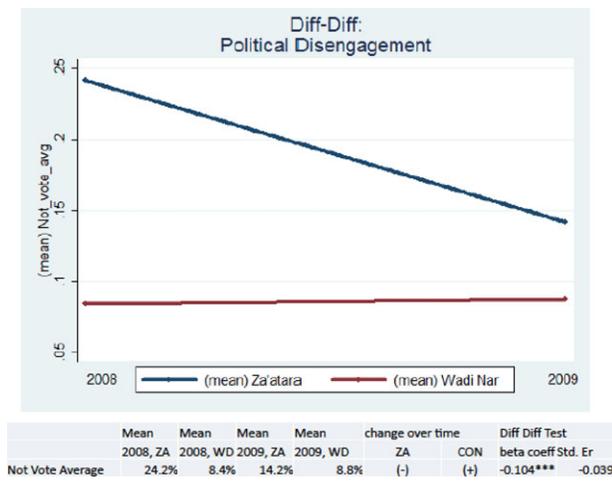
In addition to our principal variables, we offer two auxiliary findings. First, in addition to support for less militant political parties and ideologies as a result of checkpoint easement, our variable *Political Disengagement* reveals a statistically significant increase in willingness to participate in elections among respondents in Za’atara, as compared to Wadi Nar (see Figure 6; for additional information on this variable, see section C in the supporting information). This highlights the

FIGURE 5 Difference-in-Difference Estimation: Secular/Nonviolent and Hamas (over Fatah)



Note: ***p < 0.01, **p < 0.05, *p < 0.1.

FIGURE 6 Difference-in-Difference Estimation: Political Disengagement

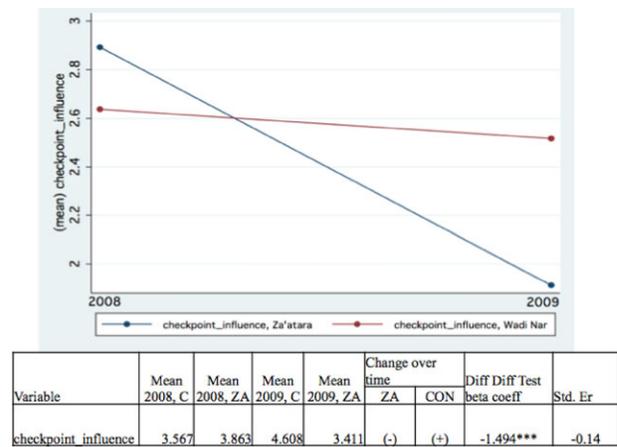


Note: ***p < 0.01, **p < 0.05, *p < 0.1.

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 likely to vote—compounding and solidifying the move away from support for violence.

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

FIGURE 7 Difference-in-Difference Estimation: Direct Checkpoint Influence



Note: ***p < 0.01, **p < 0.05, *p < 0.1.

political attitudes.¹⁴ This variable reveals that as a result of easement, respondents in Za'atara (treatment) were far more likely than those in Wadi Nar (control) to support resolving the conflict using peaceful means rather than violent ones (see Figure 7).

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

Identification and 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 it 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 preexisting 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 preexisting historical, ethnic, or political lines.¹⁵ 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 (Issacharoff 2007; Stack 2003). This is made further evident by our baseline checks on opinion and demographic variables (see Table 1), which demonstrate that the population surrounding Za’atara was *ex ante* more militant than that in 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 (i.e., it is *as-if* exogenous).¹⁶

¹⁵Further, 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.”

¹⁶Of 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 intro-

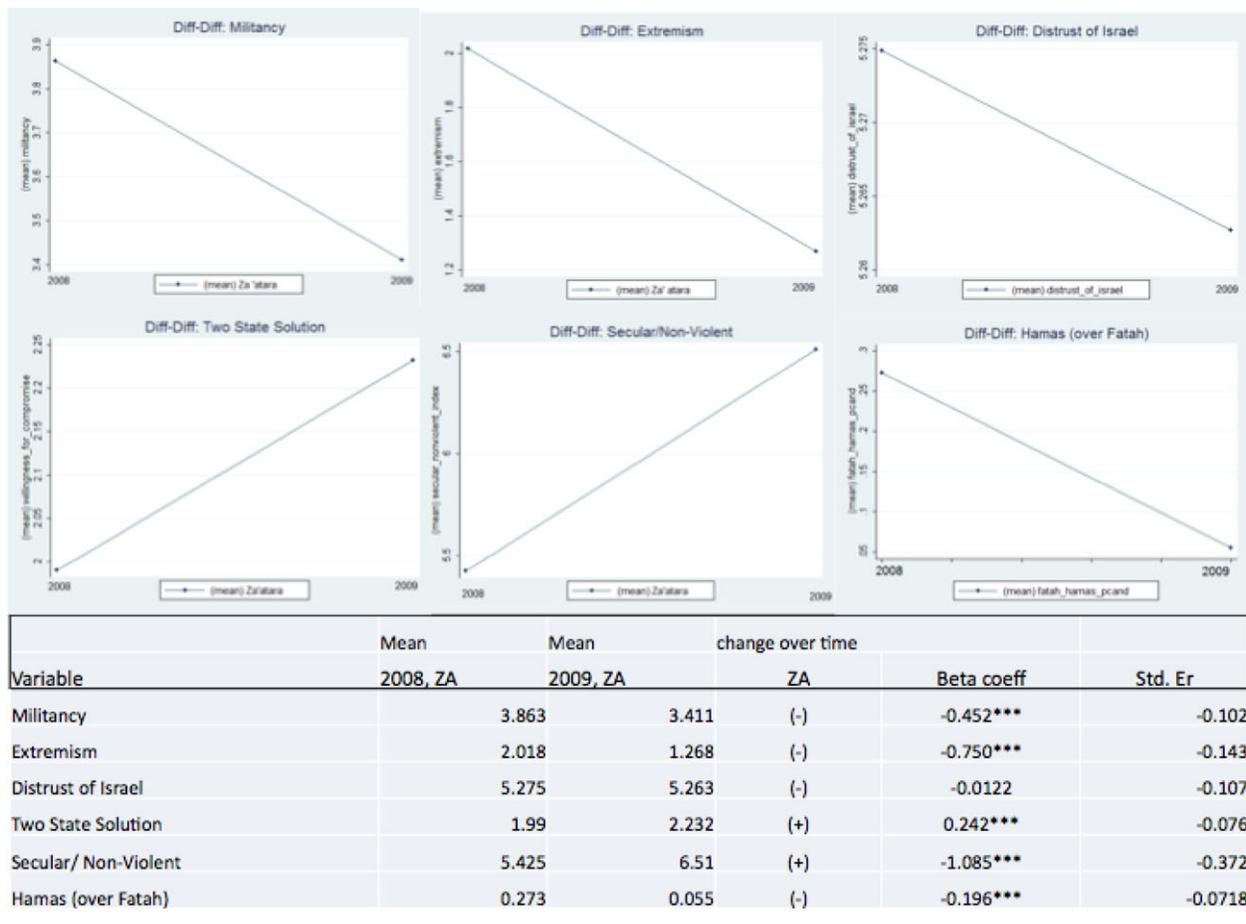
The second concern regarding research design pertains to blinding. Double-blind experiments are the gold standard of scientific research design and are not just achievable in lab settings. An experiment is said to be double-blind if neither the individuals studied nor the researchers collecting data 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 among 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 preempt this concern, we ran a presample 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 that no mention was made about easements.¹⁷ 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, that is, 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: 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

duced in the late 1990s—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, born 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.

¹⁷This 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 nongovernmental organizations as “false promises” (Barsella 2007). As a final testament to how unfathomable the easement of checkpoints was, even the United Nations remarked on its impossibility as late as February 2009. In its *Humanitarian Monitor*, it decreed 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).

FIGURE 8 Pre/Post Study of Checkpoint Easement around Za’atara



Note: ***p < 0.01, **p < 0.05, *p < 0.1.

derives from the fact that while the treated group became *less* likely to support violence, the control group 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 toward militancy may simply be 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 2)], 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 toward 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 8—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.

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 do not 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 destabilizing way. (For a more detailed discussion of external validity using additional data, see section D in the supporting information).

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; 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 article, we seek not merely to show *whether* nonviolent 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 nonviolent form of repression, such as a checkpoint, that drives Palestinians to support violence? In the introduction, we suggested that nonviolent 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).

These data confirm the first part of this equation—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” (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

TABLE 2 Testing for Heterogeneous Treatment Effects

Humiliation and Militancy						
Checkpoint Effect Model						
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***	0.0254***	0.300***	0.297***
	−0.0921	−0.0921	−0.0935	−0.0923	−0.0923	−0.0917
Year: 08 v 09	1.041***	1.078***	1.041***	1.065***	1.042***	1.037***
	−0.0981	−0.0999	−0.0983	−0.0978	−0.0983	−0.0979
“Harms” of Checkpoints						
“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						

Note: ZA = Za’atara; WD = Wadi Nar.

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 (see Table 2).¹⁸

This correlation between Palestinians who feel humiliated by the experience and support for violence maintains across robustness checks (see section E in the supporting information). 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, nonviolent 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

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

common experience—both Iraqis and Americans—in Iraq.

—Annia Ciezadlo (2005)

Repression has many faces, as does dissent. In this article, we demonstrate how even nonviolent, 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 *nonviolent* repression (e.g., impediments to mobility) and *violent* dissent (e.g., support for militancy). Additionally, we have suggested a mechanism linking this nonviolent 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 as 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 article also contributes to important debates within the policymaking community. There is little question that checkpoints are an impediment in 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. (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 2008b, 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 toward 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 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) place checkpoints in the rubric of combat in Iraq, as a nonviolent 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 nonviolent 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

Additional Supporting Information may be found in the online version of this article at the publisher's website:

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