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When Does Past Trauma Matter for Present Decision-making? Evidence from a Field Study on Myopia and Waiting Periods*

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Abstract

We document the relationship between prior exposure to violence and myopia using a field experiment in the Democratic Republic of the Congo. We endow study participants with an asset that grows over time, and find that direct exposure to violence during the Congolese wars strongly predicts redemption of the asset for its minimum value *only if participants can redeem their asset on the same day they receive it*. When we mandate an overnight waiting period before redemption is allowed (and before the asset begins to grow), there is no difference in myopic behavior by exposure to violence. Our results suggest that choice architecture is particularly important for vulnerable populations, even within a developing-country sample.

JEL Classification: C93, D14, F51

Keywords: impulsivity, deliberation, waiting period, violence, field experiments

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1 Introduction

Extensive work in both psychology and economics has documented that preferences are malleable. For example, they are affected by fleeting emotional states (Loewenstein, 1996), visceral factors such as hunger (Ashton, 2015) and intoxication (Schilbach, 2015), as well as exogenous events like natural (Eckel et al., 2009) and economic (Malmendier and Nagel, 2011) disasters. Major life events –typically disasters in the literature– can thus have long-lasting impacts on choice behavior.

There is substantially less work on the malleability of behavioral biases. If they respond to major life events as well, policy makers should be aware that the choice architecture faced by affected individuals is going to be particularly important for their outcomes. We examine how exposure to violence is associated with myopic choices in a field experiment. Additionally, by comparing choices made with and without a policy enforcing a mandatory waiting period, we show that prudent choice architecture can change the relationship between exposure to violence and myopia. We refer to this difference in behavior with and without the waiting period as *impulsivity*: impulsivity occurs when people make systematically different choices before and after they have had time to deliberate a choice.

We first document a significant positive relationship between past exposure to violence and myopia without a waiting period; people who had been previously exposed to violence are nearly twice as likely to make an extremely myopic choice than those who had not been exposed to violence. We then tested a simple intervention aimed at prompting more deliberative decision-making—a waiting period—in moderating this relationship (Imas et al., 2021; DeJarnette, 2020; Thakral and To, 2020). When the waiting period is in place, we find no link between exposure to violence and choices.

To access a population with variation in past exposure to violence, we worked with a local grocery store in Bukavu in the Democratic Republic Congo (DRC) to design an experiment to measure patience for obtaining a staple consumption good. Data for this study was collected in 2014, at the same time as the data reported on in Section 3 of Imas et al. (2021).[1] The grocery store was located in a region with a population with heterogeneous past exposure to violence.

[1]This means the data were collected prior to when the authors began the practice of preregistration. In this case, we hope that our decision to run this study in the Eastern DRC makes our ex-ante interest in studying heterogeneity by exposure to violence clear.

Customers arriving at the store during our study received a coupon that could be initially exchanged for 1 kilogram (kg) of cassava flour. For every day the coupon was saved rather than redeemed, it increased in value by another kg (up to a maximum of 5 kg). After agreeing to participate in the study, participants were randomized into two treatment groups. In the Immediate Treatment individuals could redeem their coupon immediately upon receiving it (or later the same day) for 1kg of flour. In the Waiting Period Treatment, the coupon could not be redeemed until the day after it was received (also for 1kg). Our outcome variable of interest is whether individuals redeem the coupon for its minimum value. This is because only one choice in our study is made immediately following news of the opportunity afforded by the coupon: whether to redeem it for 1kg in the Immediate treatment. Redeeming the coupon for its minimum value is costly; most individuals who do not redeem the coupon immediately for 1kg wait until it obtains a value of 4 or 5kg before redeeming.

We selected the study setting within Bukavu to minimize potential confounds such as uncertainty about the delivery of a future reward and transaction costs (Benhabib et al., 2010). Due to a lack of access to refrigeration, individuals in this neighborhood make visits to the store most mornings to buy food for the day. This helps to limit differential transaction costs associated with redeeming the coupon on the same day it was received versus a later date. Additionally, participants' frequent interactions with the store and its staff, who were on hand to run the study, minimized uncertainty that future payouts would in fact be delivered.

Our findings imply that policies designed to help individuals and communities recover from violence may need to account for increased impulsivity, much as previous work has suggested they account for things like present bias (Camerer et al., 2003; Bernheim and Rangel, 2007). Unconditional cash transfers are common in the wake of humanitarian emergencies (ICRC, 2007; Jaspars et al., 2007), are gaining in popularity as economic development tools. Enforcing waiting periods, and perhaps encouraging the use of them for planning and budgeting, between the news of a transfer and consumption opportunities could help individuals avoid myopic choices. On the other hand, our results suggest that predatory lenders may consider areas affected by violence to be ripe targets for spontaneous demand for credit in response to solicitations. Waiting periods for credit could be a valuable part of consumer protection policy in such regions.

2 Literature and Hypotheses

Risk preferences are the most-studied measure in the economics literature on preferences and life events. Malmendier and Nagel (2011) demonstrate that experiencing the macroeconomic shock of the Great Depression significantly affected risk preferences for decades. Eckel et al. (2009), Bchir and Willinger (2013), and Hanaoka et al. (2018) show that people negatively impacted by Hurricane Katrina in New Orleans, mudslides in Arequipa, Peru, and the 2011 Japanese Earthquake, respectively, appear more risk seeking than those who were not impacted. Cameron and Shah (2013) find that individuals who suffered earthquakes and floods in Indonesia become more risk averse than otherwise similar groups in neighboring villages. Evidence on the effects of violence on risk preferences is similarly mixed. While Voors et al. (2012) find that people exposed to violence become more willing to take risks, Callen et al. (2014) find the opposite: that people become more risk averse via a higher certainty premium.[2]

There is less economic work on whether time preferences are affected by similar stimuli, and the evidence on whether there are medium-to-long-run consequences of prior events and experiences on time preferences is mixed.[3] Callen (2015) finds that individuals affected by a the 2004 Indian Ocean Tsunami in Sri Lanka become more patient over the long-run. On the other hand, living in an area where violence (Voors et al., 2012), or mudslides (Bchir and Willinger, 2013) occurred has a weak or insignificant effect on time preferences. These studies measured exposure to trauma at the community level. Additionally, they examined time preferences over outcomes that all lay in the future, with no variation in deliberation time. As such, they could not identify a relationship between violence on behavioral facets of discounting like present bias and impulsivity separately from exponential discounting.

While the Callen et al. (2014) paper studies the impact of violence on risk preference and the certainty premium rather than time preference, it is particularly relevant for our motivation. First, they demonstrate that rather than increasing risk aversion in general, exposure to a violent act exacerbates the certainty premium –the discontinuity in the valuation of a gamble that occurs

[2]Several important features distinguish the two studies, such as the fact that Voors et al. (2012) measure exposure to violence on a community level while Callen et al. (2014) measure exposure to violence on the individual level, and identify the impact of violence as an interaction between exposure and a prime to recall the exposure.

[3]Outside the major life event context, transient emotional states such as happiness (Ifcher and Zarghamee, 2011) and feelings of loss of control (Gneezy and Imas, 2014), as well as hunger (Ashton, 2015) and subsequent satiation (Kuhn et al., 2017) have been shown to have a significant effect on how people make intertemporal choice.

as the gamble approaches a certain payment (Kahneman and Tversky, 1979). The implication for time preferences, we believe, is that exposure to violence may be particularly important for immediate rewards.[4] Second, they find that only the combination of exposure to violence and a prime designed to elicit feelings of fear of anxiety increase the certainty premium. Our takeaway from this is that exposure to violence may be particularly relevant when emotional regulation is required for prudent decision-making. Indeed exposure to violence has been shown to negatively impact emotional regulation (Osofsky, 1995), which plays an important role in self-control and impulsivity (Loewenstein, 2000).

Our study design and hypotheses follow from these two points; the Immediate and Waiting Period treatments reveal how exposure to violence relates to the likelihood that an immediate reward is selected without and with a mandatory cooling-off period that allows time for deliberation. Our first hypothesis is that without the waiting period, individuals exposed to violence will be more likely to make the highly myopic decision to redeem the coupon for its minimum value as soon as possible. Second, we hypothesize that with the waiting period in place, exposure to violence will not matter for this decision. Putting these two hypotheses together, it should be that the treatment effect of the waiting period is larger for individuals exposed to violence, making them more impulsive by our definition. As the previous literature is somewhat inconclusive, we make no hypothesis regarding whether exposure to violence predicts when the coupon is redeemed entirely within the waiting period treatment.

3 Procedures

Our study was conducted at a local grocery store in a residential area in Bukavu, a city on the Eastern border of the Democratic Republic Congo (DRC). The city is near a current active combat zone and our population comprised of people that differed in their exposure to violence. We measure exposure to violence at the individual level using a variety of survey questions. A total of 258 customers participated in the study, and within our sample, 34% identified as being directly exposed to violence ("personally injured during the war") while 66% were either indirectly exposed

[4]Another motivation for our focus on immediate rewards is Blumenstock et al. (2014), who find that exposure to violence significantly decreases the propensity to use mobile money over cash. The authors suggest this is due to the greater perceived certainty associated with holding physical money.

to violence (e.g. members of family injured) or not exposed at all. Our measures of exposure to violence are all highly correlated with one another, so we choose one measure –direct personal exposure– to use throughout, although results are similar for other measures.

The store sells everyday goods and simple foodstuffs like rice, water, and milk. Because the store has access to electricity and refrigeration, which is lacking in most homes, many people in the area visit the store every day. This is important because it suggests that differences in the imposed transaction costs of the study were likely small across days. The store ran as usual during the study and was staffed by the family that has owned and operated it for the past decade. We hoped this would avoid disrupting customers' familiarity with the store and reduce uncertainty related to follow-through on offered future rewards. One of the authors supervised all aspects of the procedures for the entire length of the experiment.

3.1 Implementation

Upon arriving at the store and agreeing to participate, all customers completed a detailed survey on their exposure to violence and other demographic measures. Participants who were illiterate or had difficulty completing the survey on their own were helped by a research assistant who was blind to the hypotheses. The survey was available in both Swahili and French and the participant chose whichever was more convenient for them. The survey took about 30 minutes to complete and recruitment spanned a number of days. This means that treatment status did not systematically affect the days of the week on which the coupons could be redeemed for certain values.

Participants were then randomly assigned to one of two treatments. In both, they received a coupon that could be exchanged for varying amounts of cassava flour depending on when it was redeemed.[5] Cassava flour is used to make daily breads in the Eastern DRC. Cassava products in general are single largest diet item for 80% of the population, with consumption of about 0.4kg per-capita daily, mostly from bread (Harvest Plus, 2010). The coupons were were redeemable for flour that the store typically sold, not a new product.

In the Immediate Treatment, the coupon could be redeemed right away for one bag of flour (approximately 1kg), the next day for two bags of flour, and so on, up until five bags of flour. In the Waiting Period Treatment, we shifted the redemption schedule by one day: the first time the

[5]Each coupon had an ID matching it with a questionnaire, a date of issue and a code signifying the treatment.

coupon could be redeemed was on the next day for one bag of flour, and so on, up until five bags of flour.[6] Table 1 shows the value of the coupon on each day of the study by treatment.

Table 1 about here

Although we anecdotally observed many individuals visit the store daily, an important concern is whether transaction costs of coming back to the store motivated individuals in the Immediate treatment to redeem their coupon immediately. This motive is not present in the Waiting Period treatment. To test this, we regress an indicator variable for immediate redemption on a measure of how far individuals in the Immediate treatment live from the store (in the city center). The relationship is not large or statistically significant.[7] The same is true of the difference in the relationship between distance and as-soon-as-possible redemption across treatments.[8]

3.2 Sample Balance and Identification

Of the 258 participants, 136 were assigned to IT and 122 to WPT. Table 2, Panel A shows that key demographic and preference variables are uncorrelated with treatment assignment.[9] The frequency of significant differences is consistent with randomness (two of 22 measures differ at the 10% level).

We do not expect observables to balance across exposure to violence. Because the conflict in the DRC, and specifically the Great Lakes region of the eastern DRC, has been going on for many years, there is a span of time periods during which our participants could have experienced the reported exposure to violence. If exposure affects impulsivity, that difference in decision-making should translate into different circumstances over time. Table 2, Panel B shows this; those exposed to violence live further from the city center, and have poorer access to food, water and cell service.

Table 2 about here

[6] Only one person did not redeem their coupon by the last possible day. This individual was in the Waiting Period treatment.

[7] The coefficient on distance from city center (1-3 scale, 1 = "Live in city center", 2 = "In neighborhood outside city center", 3 = "In a village outside the city center") is 0.055 ($p = 0.32$).

[8] The coefficient on the interaction between distance from city center and an indicator for the Waiting Period treatment is -0.029 ($p = 0.65$).

[9] Full questionnaire available upon request.

Because of this imbalance it is unclear whether associations between exposure and impulsive choices are direct, or indirectly operate through post-exposure differences. This is often true in the literature on the long-term impact of exposure to violence, and we make two observations about this issue. First, if the initial exposure to violence was effectively randomly assigned, our study would still detect a long-run impact of exposure to violence on decision-making, potentially through an unobserved intermediary channel. For more than 20 years, the DRC has been facing an ongoing, complex militarized conflict. By 2008, the first and second Congo wars and their aftermath had killed 5.4 million people mostly in the East Congo (Coghlan et al., 2007) and random violence was widespread (Elbert et al., 2013; ECHACP, 2014). Despite the UN efforts, including the Goma peace agreements of 2008 and 2009, fighting among various armed groups continues to the present (AI, 2004, 2008a,b, 2012; MSF, 2013). According to a Human Rights Watch report regarding the conflict in the Bukavu region during the Congolese wars, "armed groups indiscriminately attacked civilians and burned houses" (Longman and Kippenberg, 2000). According to the reports from local and international NGOs and the US State Department (Mahecic, 2012; MSF, 2005; USDOS, 2014), the violence perpetrated by armed groups in the region was largely indiscriminate, including the shelling of populated areas like refugee camps and airports. The neighborhood in which we conducted this study is within mortar-fire range of the Rwandan border, and was invaded and occupied from across the border during the Second Congo War.

Second, while endogenous post-exposure to violence variables cannot be used as controls to recover a direct effect of exposure, under some assumptions, we can use them to asses whether they explain the predictive power of exposure (Acharya et al., 2016). When we hold fixed living location and food, water, and phone access, it may select for different parts of the distribution of exposed and unexposed individuals. Exposed individuals living close to the city center with good access to goods and services differ along unobservable dimensions from unexposed individuals in the same position. As exposure appears in Table 2 to be associated with uniformly worse outcomes, we make the assumption that by holding these post-exposure outcomes fixed, we compare exposed individuals with below-average impulsivity (within the exposed group), to unexposed individuals with above-average impulsivity (within the unexposed group). Therefore, when we add these controls, we expect to bias our estimate of the direct association between exposure and impulsivity towards zero. Also, to the degree that these post-exposure outcomes are due to the financial conse-

quences of exposure, rather than the direct consequences, we can control for whether individuals experienced property damage during the wars.

4 Results

We show the fraction of sample redeeming the coupon for its minimum value by exposure to violence and treatment in Figure 1. In the Immediate treatment, 16 of 85 (19%) of participants without personal exposure to violence redeemed the coupon for its minimum value, whereas 18 of 51 (36%) of participants with personal exposure to violence did so. In the Waiting Period treatment, 7 of 86 (8%) of participants without personal exposure to violence redeemed the coupon for its minimum value, whereas 4 of 36 (11%) of participants with personal exposure to violence did so.

Figure 1 about here

We use linear probability models with heteroskedasticity-robust standard errors to asses the significance of these findings. Estimates are presented in Table 3. The first four columns of Table 3 show the within-treatment associations of exposure to violence and minimum value redemption, columns (5) and (6) show the within-exposure group treatments effects, and the last two columns show the difference-in-differences estimates of the treatment effect by exposure status. Columns (2), (4), and (8), replicate columns (1), (3), and (7) with the inclusion of our control variables (see the caution on the interpretation of these estimates in Section 3.2). Results are nearly identical with or without the controls.

Table 3 about here

Our first hypothesis is that exposure to violence is positively associated with minimum value redemption in the Immediate treatment. We find support for this hypothesis: columns (1) and (2) show that being personally exposed to violence is associated with a significant 17-18 percentage point increase in the likelihood of minimum value redemption ($p = 0.041$ and $p = 0.039$, respectively).

Our second hypothesis is that exposure to violence is not associated with minimum value redemption in the Waiting Period treatment. We fail to reject this hypothesis: columns (3) and (4) show that being personally exposed to violence is associated with an insignificant 3-6 percentage point increase in the likelihood of minimum value redemption ($p = 0.625$ and $p = 0.381$, respectively).

Putting these two hypotheses together, it should be that the treatment effect is larger for individuals exposed to violence, making them more impulsive than unexposed individuals. While we find directional support for this implication, we fail to reject the alternative. Columns (5) and (6) show that the treatment effect is statistically significant for both groups, meaning that both groups impulsive by our definition: exposed individuals are 24 percentage points less likely to redeem for the minimum value as a results of the waiting period ($p = 0.006$), and unexposed individuals are 11 percentage points less likely ($p = 0.041$). However, columns (7) and (8) show that the difference in treatment effect by exposure is not statistically significant. Despite the large difference-in-differences of 12-13 percentage points (in other terms, exposure increases the magnitude of the treatment effect by 114-126%) , we don't have the statistical power to reject that the interaction term is equal to zero ($p = 0.179$ and $p = 0.234$, without and with controls respectively).

Thus far, we have focused on minimum value redemption of the coupon because of our specific interest in impulsivity. We can consider the relationship between exposure to violence and a more general measure of patience by using only data from the Waiting Period treatment. Individuals with more present bias and higher discount rates should redeem the coupon sooner for a smaller reward (we cannot separately analyze these two dimensions in our study). We estimate OLS models of coupon redemption—which is measured in both days waited and kilograms redeemed—with heteroskedasticity-robust standard errors. Results are in Table 4.

Table 4 about here

Both with and without control variables, we find no significant association between exposure to violence and coupon redemption in the Waiting Period treatment (recall that we already found no association between minimum value redemption and exposure in this treatment in Table 3). Without controls, we find that exposure is associated with redeeming the coupon 0.08 days sooner (and thus for 0.08 fewer kg of flour), from a base of 3.30 days ($p = 0.706$, a 2.4% reduction). With

controls, we find that exposure is associated with redeeming the coupon 0.23 days sooner from a base of 2.98 days ($p = 0.283$, a 7.7% reduction). Thus, exposure to violence is only relevant in our study when individuals have the opportunity to redeem their coupon immediately after receiving it.

5 Conclusion

Calculations of the economic costs of war and violence have typically focused on the loss of existing capital, disruptions to future capital development, and human casualties (Stewart, 1993). However, for those who survive, exposure to violence and other trauma has been shown to affect behavior and lead to costly, sub-optimal decision-making long after the negative event has passed.[10] These second-order effects of exposure to violence are likely dwarfed by the direct effects in the immediate aftermath, but in the long run, they can play a key role in determining when and how recovery occurs. For example, if exposure to violence changes time and risk preferences, the investment and entrepreneurship needed for recovery may unfold differently than expected.

In this study, we focused both identifying an association between exposure to violence and time preference, as well how that association changes with a simple adjustment to the choice architecture of our task: a waiting period. Indeed, we found a large association between exposure to violence and the likelihood that participants made a costly, myopic decision in the Immediate treatment. While our estimates were not precise enough to conclude that waiting periods were more effective for individuals exposed to violence, we can say that when the waiting period was in place, we found no significant association between exposure and highly myopic behavior. These findings are consistent with exposure to violence being closely associated with impulsive intertemporal choices, but not with deliberated intertemporal choices. Waiting periods have immense potential value as cheap, light-touch policy tools, but more work is needed to firm up estimates of the differential impact of waiting periods, and expand the set of vulnerable populations studied to determine where they may be particularly effective.

[10] Research in psychology has demonstrated that exposure to violence and other trauma (e.g. extreme poverty) has complex, deleterious long-run effects on both mental and physical health (Boscarino, 2006; Yehuda, 2006). Recent work has also shown that such experiences also affect economic decision-making: see Section 2 for a review of this literature.

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Tables and Figures

Table 1: Coupon Value (kg of Flour)
by Treatment and Date

Treatment:	Immediate	Waiting Period
Day of Receipt	1	0
1 Day after Receipt	2	1
2 Days after Receipt	3	2
3 Days after Receipt	4	3
4 Days after Receipt	5	4
5 Days after Receipt	0	5
6 Days after Receipt	0	0

This table appears as Table 4 in Imas et al. (2021).

Table 2: Observable Balance

Variable	*Panel A: By Treatment*			*Panel B: By Exposure*		
	Immediate	Waiting Period	Diff.	Exposed	Unexposed	Diff.
	(1)	(2)	(3)	(4)	(5)	(6)
Female	0.41	0.42	-0.01	0.45	0.40	0.04
Age	30.90	30.59	0.31	32.48	29.87	2.61*
Secondary education or beyond	0.79	0.77	0.02	0.73	0.81	-0.08
Has children	0.69	0.75	-0.05	0.76	0.70	0.06
Employed	0.44	0.39	0.06	0.33	0.46	-0.12*
Home distance from city-center (1-3 scale)	1.57	1.61	-0.04	1.72	1.52	0.20**
Feels safe at home (1-4 scale)	2.34	2.53	-0.20*	2.37	2.46	-0.09
Food access (1-4 scale)	2.39	2.39	0.00	2.15	2.51	-0.36***
Clean water access (1-4 scale)	2.40	2.29	0.11	2.17	2.44	-0.26**
Access to medical care (1-4 scale)	2.05	2.13	-0.08	1.94	2.15	-0.21*
Access to shelter (1-4 scale)	2.36	2.40	-0.04	2.23	2.46	-0.23*
Access to phone network (1-4 scale)	2.66	2.40	0.26*	2.32	2.65	-0.34**
Life got better last year (1-5 scale)	3.04	3.14	-0.10	2.93	3.17	-0.24*
Expects better life next year (1-5 scale)	3.72	3.73	-0.08	3.78	3.62	-0.16
Not afraid to take risks (1-4 scale)	3.03	3.12	-0.09	3.14	2.95	-0.18
Feels in control of life (1-4 scale)	2.32	2.23	0.08	2.33	2.25	0.08
Worries about future (1-4 scale)	2.74	2.88	-0.14	2.90	2.77	0.13
Plans for next week (1-4 scale)	3.10	3.13	-0.04	3.14	3.10	0.04
Trusts people (1-4 scale)	2.38	2.55	-0.17	2.45	2.47	0.01
Close to community (1-4 scale)	2.94	3.05	-0.11	3.13	2.92	0.21
Property damage during conflict	0.46	0.50	-0.04	0.63	0.40	0.22***
Direct exposure to violence during conflict	0.38	0.30	0.08			

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$.

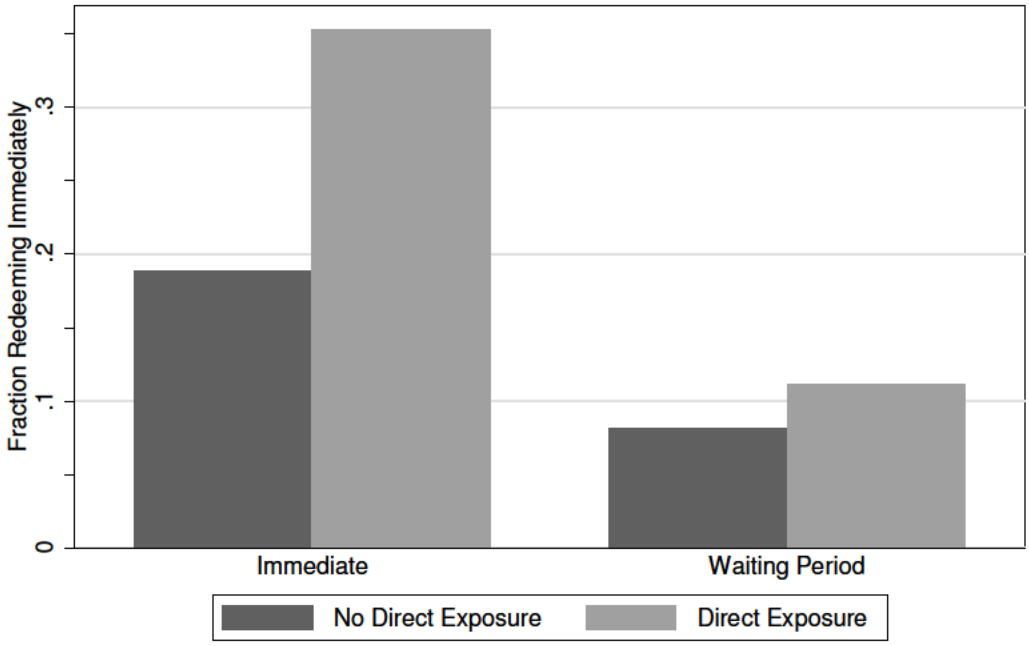

Figure 1: Immediate Redemption by Exposure to Violence and Treatment

Table 3: Minimum Value Coupon Redemption by Exposure to Violence and Treatment

Sample:	Immediate		Waiting Period		Exposed	Unexposed	All	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposure	0.165**	0.181**	0.030	0.057			0.165**	0.180**
	(0.080)	(0.087)	(0.061)	(0.065)			(0.080)	(0.083)
WPT					-0.242***	-0.107**	-0.107**	-0.106**
					(0.086)	(0.052)	(0.052)	(0.052)
Exposure × WPT							-0.135	-0.122
							(0.100)	(0.103)
Home distance from city center		0.020		0.037				0.028
		(0.056)		(0.035)				(0.033)
Food access		-0.024		0.004				-0.011
		(0.050)		(0.035)				(0.030)
Clean water access		0.020		0.004				0.014
		(0.036)		(0.030)				(0.025)
Phone network access		-0.014		-0.023				-0.017
		0.033		(0.027)				(0.021)
Property damage during conflict		-0.150*		-0.144**				-0.146***
		(0.080)		(0.062)				(0.050)
Constant	0.188	0.267	0.081	0.125	0.353	0.188	0.188	0.247
	(0.043)	(0.191)	(0.030)	(0.084)	(0.068)	(0.043)	(0.043)	(0.104)
Observations	136	132	122	119	87	171	258	251

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Robust standard errors in parentheses. Seven individuals did not provide complete information on our questionnaire (four in IT and three in WPT), which explains the difference in observation count when controls are included.

Table 4: Coupon Redemption by Exposure, Waiting Period Treatment

	(1)	(2)
Exposure	-0.080	-0.229
	(0.212)	(0.212)
Home distance from city center		-0.058
		(0.133)
Food access		0.017
		(0.111)
Clear water access		-0.134
		(0.106)
Phone network access		0.169**
		(0.085)
Property damage during conflict		0.625***
		(0.197)
Constant	3.302	2.982
	(0.109)	(0.375)
Observations	122	119

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$. Robust standard errors in parentheses. Three non-minimum value redeemers in the Waiting Period treatment did not provide complete information on our questionnaire, which explains the difference in observation count when controls are included.