Exposure to Violence Predicts Impulsivity in Time Preference: Evidence from The Democratic Republic of Congo

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Abstract

We document the relationship between exposure to violence and impulsivity in intertemporal choice. Using data from a field experiment in the Democratic Republic of Congo, we find that direct exposure to violence during the Congolese wars predicts impulsive intertemporal choice, but does not predict discounting in general. The distinction between discounting and impulsivity comes from comparing intertemporal choices behavior with and without a mandatory one-day waiting period after the coupons are disbursed. Thus, while violence may have harmful behavioral consequences, a waiting period that encourages deliberation or commitment-seeking may effectively mitigate them.

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1 Introduction

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, suboptimal decision-making long after the negative event has passed.[1] 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 examine the relationship between exposure to violence and impulsivity in intertemporal choice. By impulsivity, we refer to impatient choices made when the ability to make a choice immediately follows the news of a new choice set, as opposed to choices made when significant time elapses between news about a choice and the ability to make a choice. In psychological terms, choices made without a waiting period are made in an impulsive "hot" state. The wait serves as a cooling-off period and leads to choices made in a deliberate "cold" state (Loewenstein, 2000; Kahneman, 2011). Differences in decision-making with and without the waiting period allow us to identify impulsivity.

We use data collected for related work on the general impact of waiting periods on intertemporal choice (Imas et al., 2018). 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. The grocery store was located in a region with a population heterogenous in their exposure to violence. Customers arriving at the store during our study received a coupon that could be initially exchanged for 1 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, they were randomized into two treatment groups. In the 'Immediate' Treatment (IT for short) individuals could redeem their coupon immediately for 1kg of flour. In the 'Waiting Period' Treatment (WPT for short), the coupon could not be redeemed until the day after

[1]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.

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. This is also because redeeming the coupon for its minimum value is costly; most individuals who do not redeem the coupon immediately wait until it obtains a value of 4 or 5kg before redeeming.

Our study setting was chosen to access a population with heterogeneous exposure to violence and 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.

Exposure to violence has been shown to be associated with lower impulse control (Osofsky, 1995), and greater myopia (Voors et al., 2012) and propensity to exhibit behavioral biases such as the "certainty effect" (Callen et al., 2014). We find that, in line with prior work, individuals who were exposed to violence were substantially more likely to redeem the coupon on the earliest possible date relative to those who were not exposed. However, this difference only exists in IT. There was no difference between the groups when both experienced a waiting period in WPT.

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, which are common in the wake of humanitarian emergencies (ICRC, 2007; Jaspars et al., 2007), offer flexibility and low delivery costs but also great scope for temptation. Enforcing waiting periods (and perhaps encouraging the use of self-imposed waiting periods for planning and budgeting) between transfer news and consumption opportunities could allow impulsivity-prone individuals to make better choices. Additionally, our results suggest that predatory lenders may consider areas affected by violence to be ripe targets. These regions would stand to derive greater benefits from government monitoring and intervention.

2 Trauma and Behavioral Change

Work in both psychology and economics has documented that preferences are malleable. They are affected by fleeting emotional states (Loewenstein, 1996), visceral factors such as hunger (Danziger et al., 2011) and intoxication (Schilbach, 2015), as well as exogenous events like natural disasters (Eckel et al., 2009). Major life events –typically disasters in the literature– can sometimes have long-lasting impacts.

Risk preferences are the most studied behavioral measure in this literature. Malmendier and Nagel (2011) demonstrate that experiencing the macroeconomic 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 work on whether time preferences are affected by similar stimuli. Transient emotional states such as happiness (Ifcher and Zarghamee, 2011) and feelings of loss of control (Gneezy and Imas, 2014), as well as hunger (Kuhn et al., 2014) have been shown to have a significant effect on how people make intertemporal choice. However, evidence on the medium to long run consequences of prior events and experiences on time preferences is mixed. 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

[2]Several potentially 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.

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.

Several lines of work suggest that violence should make individuals more likely to prefer immediate, certain rewards. Callen et al. (2014) 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 as the gamble approaches a certain payment (Kahneman and Tversky, 1979). Also, Blumenstock et al. (2014) find that exposure to violence significantly decreases the propensity to use mobile money over cash, which the authors suggest is due to the greater perceived certainty associated with holding physical money. Additionally, 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 hypotheses follow directly from this literature:

Hypothesis 1: In the Immediate treatment, individuals exposed to violence will be more likely to redeem their coupon for its minimum value than individuals who have not been exposed to violence.

Hypothesis 2: Individuals exposed to violence will be more likely to redeem their coupon for its minimum value in the Immediate treatment than the Waiting Period treatment.

Hypothesis 3: The difference in minimum-value redemption rates between exposed and unexposed individuals will be larger in the Immediate treatment than in the Waiting Period treatment.

Hypothesis 4: Among those who do not redeem their coupon for its minimum value, there will be no difference in redemption amount between exposed and unexposed individuals.

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. In our sample, 34% identified as being directly exposed to violence ("personally injured during the war") while 66% were either indirectly exposed 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 focus on in the paper.

The store sells everyday goods and simple foodstuffs like rice, water, and milk. A total of 258 customers participated in the study. 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 and day of the week on each study day are not co-linear.

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.[3] 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.[4]

In the Immediate Treatment (IT), the coupon could be redeemed right away for 1 bag of flour (approximately 1kg), the next day for 2 bags of flour, and so on, up until 5 bags of flour. In the

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

[4]The DRC is the global leader in cassava consumption, with cassava providing about 65% of daily calories (Harvest Plus, 2010).

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

Treatment:	Immediate	Waiting Period
	(1)	(2)
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 5 in Imas et al. (2018).

Waiting Period Treatment (WPT), we shifted the redemption schedule by one day: the first time the coupon could be redeemed was on the next day for 1 bag of flour, and so on, up until 5 bags of flour. The value of the coupon at the time of redemption serves as our measure of intertemporal choice.[5] Table 1 shows the value of the coupon on each day of the study by treatment.

Although many individuals visited the store daily, an important concern is whether transaction costs of coming back to the store motivated individuals in IT to redeem their coupon immediately. This motive is not present in WPT. To test this, we regress an indicator variable for immediate redemption on a measure of how far individuals in WPT live from the store (in the city center). The relationship is not large or significant.[6] The same is true of the difference in relationships between distance and as-soon-as-possible redemption across treatments.[7]

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.[8] The frequency of significant differences is consistent with randomness (two of 22 measures differ at the 10% level).

[5]Only one person did not redeem their coupon by the last possible day. This individual was in WPT.
[6]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 ($p = 0.32$).
[7]The coefficient on the interaction between distance from city center and WPT is -0.029 ($p = 0.65$).
[8]Full questionnaire available upon request.

We do not expect observables to balance across exposure to violence. Because the conflict in the DRC, and the Great Lakes region of the eastern DRC in general, 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.

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 variables cannot be used as controls to recover a direct effect of exposure, under strict assumptions, we can use them to asses whether they explain all of 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 exposed group). Therefore, when we add these controls, we expect to bias our estimate of the direct association between exposure and impulsivity downwards. Also, to the degree that these post-exposure outcomes are due to the financial consequences of exposure, rather than the direct consequences, we can control for whether individuals experienced property damage along with physical injury.

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			

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

4 Results

Treatment effects without regard to violence are discussed in Imas et al. (2018). 25% of individuals in the Immediate treatment redeem the coupon as soon as possible whereas only 9% do so in the Waiting Period treatment. The 16% difference is statistically significant ($p < 0.01$). This difference suggests that individuals in our sample are subject to impulsive behavior that can be mitigated by introducing a waiting period: when given the opportunity to think about their choice for a day, individuals are more likely to allow the coupon to acquire additional value. Instead of simply pushing back the onset of temptation and impulsivity by a day, the waiting period changed the decision problem such that individuals are able to attain the larger, later reward.

We show the fraction of sample redeeming the coupon for its minimum value by exposure to violence and treatment in Figure 1, with corresponding regression coefficients in columns (1), (3), (5), (6), and (7) of Table 3. Our first hypothesis is that within the Immediate treatment, individuals exposed to violence will be more likely to redeem the coupon for its minimum value. There is support for this hypothesis. 35% of individuals in the exposed group do so, and 19% in the unexposed group do so ($p = 0.04$).[9] Our second hypothesis is that individuals exposed to violence will be more likely to redeem their coupon for its minimum value in IT than WPT. There is strong support for this hypothesis. 35% of individuals in IT do so, and 11% in WPT do so ($p < 0.01$).[10] Our third hypothesis is that the difference in minimum-value redemption rates between exposed and unexposed individuals will be larger in IT than WPT. The data are consistent with this hypothesis, although we cannot reject the alternative: the 3% gap by exposure group in WPT is smaller than the 16% gap between exposure groups in Immediate ($p = 0.18$). In columns (2), (4), and (8), we shows results with our set of control variables include (see the caution on the interpretation of these estimates in Section 3.2). Results are nearly identical. Under our assumption regarding selection into post-exposure outcomes, this implies that the association between exposure to violence and impulsivity in our study is direct.

[9] Within the Immediate treatment, there is essentially no difference across groups: 11% in the exposed group redeem for the minimum vs. 8% in the unexposed group ($p = 0.63$).

[10] Within the unexposed group, there is also an effect of the waiting period: 19% in IT redeem for the minimum vs. 8% in WPT ($p = 0.04$).

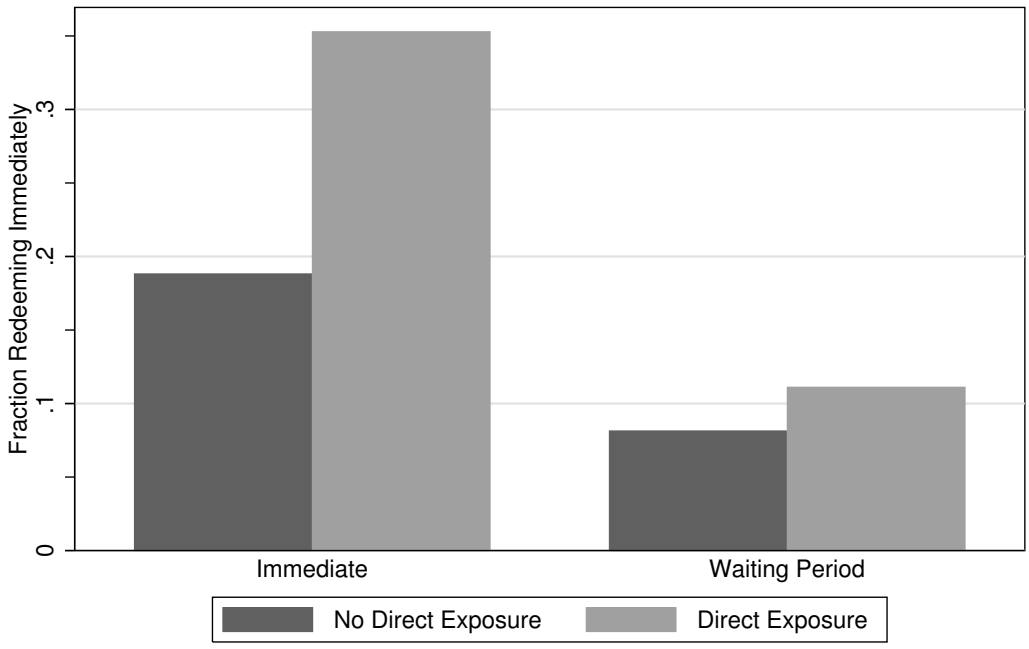

Figure 1: Immediate Redemption by Exposure to Violence and Treatment

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. 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, Non-minimum Value Redeemers

Sample:	Immediate		Waiting Period		All	
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	-0.169	-0.201	-0.006	-0.078	-0.056	-0.122
	(0.213)	(0.234)	(0.165)	(0.162)	(0.143)	(0.151)
Home distance from city center		-0.254		0.025		-0.123
		(0.158)		(0.107)		(0.100)
Food access		0.035		0.018		0.019
		(0.133)		(0.094)		(0.086)
Water access		-0.061		-0.123		-0.069
		(0.092)		(0.086)		(0.074)
Phone network access		-0.043		0.129*		0.079
		(0.087)		(0.065)		(0.060)
Property damage during conflict		0.304		0.298*		0.277**
		(0.211)		(0.153)		(0.139)
Constant	4.290	4.717	3.506	3.259	3.872	3.849
	(0.110)	(0.538)	(0.086)	(0.341)	(0.076)	(0.325)
Observations	102	99	111	108	213	207

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Robust standard errors. Six non-minimum value redeemers did not provide complete information on our questionnaire (three in both IT and WPT), which explains the difference in observation count when controls are included.

Our fourth hypothesis is that among those who do not redeem their coupon for its minimum value, there is no difference in redemption between exposed and unexposed individuals. In other words, among the self-selected group that avoids impulsive choice, exposure to violence does not predict redemption. This is true. In Table 4, we present regressions of kg of flour redeemed on exposure to violence from within each treatment in columns (1)-(4), and with both treatments pooled in columns (5)-(6). While all the coefficients on exposure to violence are negative, they are small and insignificant. The largest effect size is 4.3% of the mean. This finding is consistent with impulsivity being the pathway through which exposure to violence is associated with redemption in our study.[11]

[11]The constant terms in the models limited to IT are substantially larger than the constant terms in the models limited to WPT. This is consistent with selection into non-minimum redemption being a higher bar in IT than WPT.

5 Conclusion

We summarize the results of our study with four empirical findings. First, when individuals can immediately redeem their coupon for its minimum value, exposure to violence is clearly associated with doing so. Second, there is a strong causal impact of waiting periods on individuals who are exposed to violence: the frequency of minimum-value redemption falls 24 percentage points after the one-day waiting period, which is a decrease of more than two-thirds. Third, the causal impact of waiting periods on individuals who were not exposed to violence is smaller –11 percentage points, a decrease of a little more than 50%– although we lack the precision to reject these effects are the same. Fourth, there is no clear association between exposure to violence and the amount the coupon is redeemed for, conditional on non-minimum value redemption.

These findings are consistent with exposure to violence being closely associated with impulsive intertemporal choices, but not with deliberated intertemporal choices. Our control variable approach suggests that this association does not operate indirectly through the fact that those exposed to violence differ on other observables. However, this interpretation is dependent on strong assumptions about the way that selection into post-exposure outcomes operates. Additionally, while there is a lot of information about the Congolese civil wars in the Great Lakes region that suggests civilians were subject to random shelling and other indiscriminate acts of violence, our estimates should not be interpreted as causal. Impulsive individuals may choose not to evacuate regions in advance of random shelling, for example.

Unlike past experimental work on present bias, individuals in our study did not make binding decisions about when to use the coupons at the time they received them. In other words, we allowed for the possibility that in the Waiting Period Treatment, individuals would wake up on the day after receipt and decide to redeem their coupons immediately. This did not happen. Instead, the individuals who had to wait a day before the coupon was redeemable waited until it was much more valuable to do so. Following the taxonomy of Halevy (2015), this suggests a violation of the invariance property of time preferences, an aspect of dynamic inconsistency that has rarely been studied in the past. This is consistent with the work of Gabaix and Laibson (2017) propose a model of discounting in which individuals must estimate their future utility. Rewards that are further in the future are estimated with more noise, and thus subject to more "as-if" discounting. Waiting

periods allow individuals to deliberate and form more precise estimates of future utility, reducing discounting and leading to more patient choices. Our findings suggest that individuals exposed to violence may have a stronger initial instinct that the value future rewards is very noisy.

The propensity to make impulsive, impatient choices can stymie the accumulation of human capital and assets, which is crucial during periods of recovery from conflicts and natural disasters. Our results suggest that simply giving exposed individuals time to seek commitment devices, assistance, advice from family, or to engage in a more deliberative thought process is remarkably successful at making their choices more patient.

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