A History of Violence: Field Evidence on How Trauma Effects Time Discounting and Present Bias

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Abstract

We demonstrate the impact of exposure to violence on individuals' time preferences, specifically their degree of present bias. Prior work has shown a weak or insignificant effect of violence on a general measure of time preferences. We designed an experiment that allows us to separately identify the effects of violence on present bias versus time discounting between future periods. The field experiment was implemented in the Democratic Republic of Congo at a grocery store in an area where a portion of the population was exposed to random, indiscriminant violence. Regular customers of the grocery store were given coupons that could be redeemed soon for smaller amounts of food or later for larger amounts, with equalized transaction costs. To test for present-bias some were given coupons where the earliest date of redemption was the same day and others were given coupons where the earliest date of redemption was the following day. We find that direct exposure to violence is linked to the choice of smaller, earlier rewards, but only when redemption is available on the same day – a statistically and economically significant effect on present bias. We cite aid organization reports and use an instrumental variables robustness check to argue that violence is a causal driver of this result. Additionally, we structurally estimate a quasi-hyperbolic model of discounting, and confirm that the effects of violence on impulsivity, as measured by the present-bias parameter, are substantial Our research suggests that one of the economic costs of violence is a long-run increase in myopic decision-making, which has direct implications for post-conflict development strategies and the targeting of economic policy interventions.

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 as a result of the immediate destruction (Stewart, 1993). However, for those who survive, exposure to violence and other trauma has been shown affect behavior and lead to costly, suboptimal decision-making long after the negative event has passed. Research in psychology has demonstrated that exposure to violence has complex, deleterious long-run effects on both mental and physical health (Boscarino, 2006; Yehuda, 2002). Recent work in economics has shown that such experiences also affect economic decision-making. Traumatic experiences lead to significant changes in risk taking across a variety of contexts (Voors et al 2011, Callen et all 2014), and affect financial decision making decades into the future (Malmendier and Nagel, 2011).

In this paper, we explore the effect of violence on time discounting, specifically impulsivity (present bias). The extent to which individuals discount the future is a critical determinant of their life time outcomes (Frederick, Loewenstein & O'Donoghue, 2002). Increased impatience has been shown to have harmful effects on saving and investment (Laibson, 1997), purchasing decisions (Zauberman, 2003), and overall health outcomes (Dellavigna and Malmendier, 2006). Prior evidence has shown a weak or insignificant relationship between violence and a measure of time preferences where all choices lay in the future (Voors et al., 2011; Bchir and Willinger, 2013). We designed an experiment that allowed us to separately identify the effect of violence on present bias versus discounting between future periods. Our results show that direct exposure to violence (as opposed just to being near others affected by violence) causes a statistically and economically significant increase in present bias —the tendency to forgo large future benefits in favor of immediate, smaller rewards. Replicating prior work, violence does not have a significant effect on discounting when all rewards lay in the future. Changes in present bias affect the extent to which individuals' preferences exhibit dynamic inconsistency; barring the availability of commitment devices, increases in present bias lead to more 'mistakes' and have negative consequences for many intuitive definitions of welfare (O'Donoghue and Rabin, 1999). As such, by identifying the effects of violence on present bias separately from discounting between future dates, our findings imply that policies designed to help individuals and communities recover from violence need to account for impulsivity and that policies designed to help individuals overcome impulsivity could be gainfully targeted at individuals trying to cope with histories of violence (Camerer, Issacharoff, Loewenstein, O'Donogue and Rabin, 2003; Bernheim and Rangel, 2007).

Although much of the prior literature on time preferences has typically examined tradeoffs between monetary rewards, recent work has argued and showed that absent confounds, identifying present bias requires tradeoffs between more direct proxies for consumption (Augenblick, Niederle, Sprenger, 2014). As such, we designed an experiment to measure time preferences over consumption goods in a region with a heterogeneous population varying in exposure to violence.

We worked together with a local store in Bukavu in the Democratic Republic Congo (DRC), where exposure to violence has been identified by governmental and international organizations as random and indiscriminant of the target (European Commission Report, 2014; Elbert et al, 2013). Upon arriving at the grocery store, customers were randomly placed into one of two treatments. In both, individuals received a coupon that could be exchanged for varying amounts of flour depending on when it was redeemed. In the Immediate treatment, the coupon could be redeemed right away for a small amount of flour, the next day for a larger amount, and so on, up until 5 days later. The Delayed treatment shifted the redemption schedule by one day: the coupon could be redeemed on the next day for a small amount of flour, and so on, up until 5 days later (6 days from the day of receipt). The coupon in the Delayed condition could not be redeemed on the same day. The date when the individual chose to redeem her coupon was our main measure of time preference: earlier redemption for smaller amounts signified greater impatience than later redemption for larger amounts.

This setting was chosen to minimize potential confounds such as uncertainty about the delivery of a future reward and transaction costs (Andreoni and Sprenger, 2012). Due to a lack of access to refrigeration, customers went to the store every morning to buy food for the day, controlling for transaction costs associated with redeeming the coupon on the same day versus at a later date. Further, participants' frequent interactions with the store and its staff both before and after the experiment increased familiarity and minimized uncertainty that future payouts would be delivered.[1]

Our design allows us to test whether violence affects time preferences by changing the weight placed on the present relative to all other subsequent periods (present bias), changing the discount rate between all periods, or both. If discounting between all periods is affected, then those exposed to violence should redeem their coupons earlier both when the sooner reward is available right away (Immediate treatment) as well as the day after (Delayed treatment). If only the importance of

[1] As shown in Section 3, measures of trust did not differ by treatment or exposure to violence.

immediate rewards is affected, but not discounting in general, then exposure to violence should influence coupon redemption only when the reward is available right away (Immediate treatment). In the context of a quasi-hyperbolic β-δ model of time preferences, the former hypothesis implies a significant relationship between δ and exposure to violence, while the latter implies a relationship with β but not δ. Given prior evidence on the negative impact of trauma on emotional regulation (Osofsky, 1995), we predicted violence would affect the ability to exercise self-control and thereby increase the extent of present bias.

Our results provide support for the second hypothesis. While neither violence nor treatment status had a significant main effect on the amount of flour redeemed, our analysis revealed a significant interaction. Those in the Immediate treatment chose to receive a smaller reward significantly earlier than in the Delay treatment only if they were directly exposed to violence; violence did not affect choice of redemption date in the Delay treatment. As we demonstrate in Section 4, following the welfare criterion proposed by Laibson (1997), the shifts in present-bias caused by exposure to violence had significant consequences for individual welfare.

The paper is organized as follows. Section 2 reviews the literature. In Section 3 we discuss the procedures and the data, outlining our hypotheses and identification strategy. Section 4 presents results, robustness checks and welfare analysis. Section 5 concludes.

2. Trauma and Behavioral Change

Standard economic theory typically takes preferences as exogenous and stable over time. Models of habit formation (Constantinides, 1990) and rational addiction (Becker and Murphy, 1988) acknowledge that people's tastes may evolve with time but the changes to preferences are fully anticipated and the time path of preferences is optimally chosen by the individual. For example, a teenager deciding whether to begin to use cigarettes is modeled as being fully aware that his desire for cigarettes will increase the more he smokes. If he chooses to begin smoking, it is only after weighing the costs (e.g. financial, health) and benefits (e.g. pleasure) of the addition path. Recent work in both psychology and economics has documented that preferences are in fact malleable, subject to change due to fleeting emotional states (Loewenstein, 1996), visceral factors such as hunger (Danziger, Levav and Avnaim-Pesso, 2011; Kuhn, Kuhn and Villeval, 2014) and intoxication (Schilbach, 2015), as well as exogenous events like natural disasters (Eckel, El-Gamal and Wilson, 2009).

In the domain of choice under uncertainty, prior events and life experiences drastically change individuals' willingness to take risks. Malmendier and Nagel (2011) demonstrate that experiencing macroeconomic shocks such as the Great Depression significantly affected preferences for risk decades later. The authors find that those who experienced poor returns on stocks are less likely to invest in the stock market and take on financial risk, while those who were previously burned by bonds are less likely to participate in the bond market. Natural disasters have also been shown to significantly affect risk preferences, though evidence on the direction is mixed. Eckel, El-Gamal and Wilson (2009) and Bchir and Willinger (2013) show that people negatively impacted by Hurricane Katrina in New Orleans and mudslides in Arequipa, Peru, 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. (2011) find that people exposed to violence become more willing to take risk, Callen et al. (2013) find the opposite – that people become more risk averse.[2]

Transient emotional states such as happiness (Ifcher and Zarghamee, 2011) and feelings of loss of control (Gneezy and Imas, 2014) have been shown to have a significant effect on how people make choices over time (for overview, see Lerner and Loewenstein, 2003). However, evidence on the medium to long run consequences of prior events and experiences has been mixed. Prior work has found that living in an area where a negative event or violence occurred has a weak (Voors et al. 2011) or insignificant (Bchir and Willinger, 2013) effect on time preferences. These studies measured exposure to violence on the community level, and in turn, individuals who may have seen violence indirectly or not at all were classified as exposed. Additionally, they examined time preferences over outcomes that all lay in the future, and in turn could not identify an effect on present bias versus discounting between future outcomes.

Several lines of work suggest that rather than influencing discounting between future periods in general, violence affects present bias. Callen et al. (2013) demonstrate that rather than increasing risk aversion in general, exposure to a violent act exacerbates the certainty effect – the discontinuity between preferences over certain versus uncertain outcomes (Kahneman and Tversky, 1979). Andereoni and Sprenger (2012a and 2012b)

[2] However, several potentially important features distinguish the two studies such as the fact that Voors et al. (2011) measured exposure to violence on a community level while Callen et al. (2013) measured exposure to violence on the individual level.

argue that given the inherent certainty in the present and uncertainty in the future, this discontinuity is a factor in impulsivity and present bias. 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). Given this evidence, we hypothesize that direct exposure to violence has a significant effect on present bias.

3. Experiment Procedures

3.1 Background

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). For more than 20 years, the DRC has been facing an ongoing, complex and multifactor militarized conflict. By 2008, the first and second Congo wars and their aftermaths had killed 5.4 million people mostly in the East Congo,[3] random violence was widespread[4], and over 1.4 million more people remained displaced within the DRC, from a peak of 3.4 million at the end of 2003.[5] As a result of the Rwandan genocide in 1994, at least one million people fled to the DRC (at that time known as Eastern Zaire). Following the militarization of the Rwandan refugee camps in the Kivu provinces close to the Rwandan borders, in November 1996 Rwandan and Ugandan armies entered the DRC, launching the First Congo War. Although the war formally ended in 1998, the Second Congo War, also known as Africa's Great War, started immediately and lasted until December 2002. This war was formally (though not effectively) terminated by the Lusaka Peace Accord in

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[3] International Rescue Commission report (2008).
[4] Learning on Gender & Conflict in Africa report (2013).
[5] Amnesty International reports (2004, 2008).

1999 and a UN mission, Mission de l'Organisation des Nations Unies en République démocratique du Congo (MONUC), was deployed to the DRC in 2000. Despite the UN efforts, including the Goma peace agreements of 2008 and 2009, fighting among various armed groups continues until this day.

Since the store is located near an active combat zone, our population is a mix of people with different exposures to violence. We measure exposure to violence at the individual level using a detailed survey completed in a controlled setting. Participants went through a list of scenarios relating to exposure to violence. In our sample, 34% were directly exposed to violence ("personally injured during the war"), 39% were indirectly exposed to violence ("members of family injured during war") and 27% were not exposed to violence. The store is popular among locals and 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, the people in our sample visited the store every day. The store ran as usual during the study and was staffed by the family that has owned and operated it for the past decade in order to avoid disrupting customers' familiarity with the store and to reduce uncertainty related to the experiment taking place. One of the authors supervised all aspects of the procedures for the entire length of the experiment.

3.2 Design and 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 hypothesis and treatment assignment. The survey was in both Swahili and French and the participant chose which was more convenient for them. On average the survey took 30 minutes to complete.

Participants were then randomly assigned to one of two treatments in which each received a coupon that could be exchanged for varying amounts of flour depending on when it was redeemed.[6] In the Immediate treatment, the coupon could be redeemed on the same day (t_0) for 1 bag of flour, the next day (t_1) for 2 bags of flour, and so on, up until 5 days later (t_5). The Delayed treatment shifted the redemption schedule by one day: the coupon could be redeemed on the next day (t_0) for 1 bag of flour, and so on, up until 6 days later (t_5). We note that the subscript on t denotes days of value accrual rather than days from coupon receipt. The date when the coupon was redeemed serves as our measure of time preference. Due to the material incentives and the fact that participants came to the store every day, the redemption rate was 100%.

3.3 Identifying Assumptions

Our identifying assumptions are that assignment to treatment was random and that exposure to violence was independent of preferences. Of the 258 participants, 136 were assigned to the Immediate treatment and 122 to the Delayed treatment. Table 1 presents summary statistics from the questionnaire to verify that key demographic and preference variables are uncorrelated with treatment assignment. The frequency of significant differences is consistent with randomness in the most relevant variables below as well as

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

in the broader survey.[7] Importantly, neither trust, direct exposure to violence, stated preference for risk nor sense of control are correlated with treatment.

For the second assumption, according to the reports from local and international NGOs and the US State Department[8] (UNCHR Report, July 2011, Doctors Without Borders April 2005, Amnesty International May 2004[9]), the violence perpetrated by armed groups in the region was indiscriminate. According to UN Security Council report from 30 September 2013 armed groups are indiscriminately shelling populated areas including camps for internally displaced persons and the airport.[10] According to the Human Rights Watch report (March 2000), "armed groups indiscriminately attacked civilians and burned houses". The violence is so widespread and perpetrated by such a large number of different forces that victims and witnesses of attacks had difficulty identifying the perpetrators.[11]

Table 2 shows that our measure of violence exposure had no significant correlations with a wide range of demographic and preference variables. As in Table 1, neither stated trust, preference for risk nor sense of control are correlated with exposure to violence. Importantly, length of stay at current location does not differ between those who were directly exposed to violence versus those who were not, suggesting similar migration tendencies between the two groups. This indicates that the individuals within the camp in the exposed and unexposed groups have not differentially selected, relative to their population distributions, into the general location of the study. One variable that

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[7] See Appendix for full survey.
[8] http://travel.state.gov/content/passports/english/alertswarnings/democratic-republic-of-the-congo-travel-warning.html
[9] http://www.refworld.org/docid/40b5a1f14.html
[10] http://www.refworld.org/pdfid/52d3b0f94.pdf
[11] http://www.hrw.org/reports/2000/drc/Drc005-03.htm

does significantly correlate with exposure to violence is geographical distance of residence from the city center. Indeed, Voors et al. (2011) use an analogous geographical distance variable as an instrument to obtain a causal effect of violence. As a robustness check, we similarly use the distance from the city center as an instrument to mitigate concerns of endogeneity in the link between violence and the choice of when to redeem coupons for rewards.

Direct exposure to violence is correlated with other, less direct types of exposure to violence such as seeing violent acts committed against others, narrowly avoiding injury from bombings or shootings, having family members injured, killed or go missing and having close friends injured, killed or go missing. Additionally, it is related to damage, destruction and confiscation of one's home and forced migration.

4. Results

We break our results into two subsections. First we present reduced-form results that characterize the data and effects of the experimental manipulations. Second, we make an effort to estimate structural discounting parameters to contribute to the growing literature estimating the magnitude of deviation from standard models of time preference and to cast the effects of violence and treatment status in an interpretable and externally relevant metric.

4.1 Reduced Form Estimates

We estimate the difference between the Immediate and Delayed groups, measuring the outcome in terms of the day of coupon redemption (from 0 to 5). First we examine whether there are differences in the frequency with which individuals redeem the coupons as soon as possible. There are 34 individuals (25%) in the Immediate treatment who redeem the coupon as soon as possible for 1 bag of flour whereas only 11 (9%) do so in the Delayed treatment. The 16% difference is statistically significant ($p < 0.01$).

This establishes that individuals in our sample are subject to impulsive behavior: when forced to think about their choice overnight, individuals are more likely to allow the coupon to acquire additional value before using it. This means that instead of simply pushing back the onset of temptation and impulsivity by a day, the delay appears to have changed the thought process behind the decision. Choices made on the first day that redemption is possible in the Delay treatment are different from choices made on that first day in the Immediate treatment.

We move now to examining the interaction between the effect of experimental treatment and stated direct exposure to violence. Using at the binary decision of whether to redeem the coupon as soon as it becomes possible to redeem it, Figure 1 shows the effect of treatment broken down into the exposed and unexposed groups. There is a clear interactive effect of the treatment with exposure to violence. The levels of impulsive choice across groups, within the Delay treatment are almost identical (8% unexposed versus 11% exposed, $p = 0.69$), but vastly different in Immediate (19% unexposed versus 35% exposed, $p = 0.01$). With the addition of a standard set of demographic, preference and study controls, we estimate a between violence group difference of 22% in the Immediate treatment and 3% in the Delay treatment, with the 19% difference in differences significant at a 10% confidence level ($p = 0.06$).

Moving beyond the binary measure of behavior, we model the relationship between exposure to violence and redemption rates separately for each treatment. Here the main dependent variable is the number of bags of flour chosen for redemption, which directly corresponds to the amount of time the person waited to collect the prize once redemption became possible (same day in Immediate treatment and next day in the Delayed treatment). Results are presented in Table 3.

We find that exposure to violence in the Immediate treatment corresponds to redeeming the coupons 0.65 days sooner ($p = 0.02$) unconditionally and 0.92 days sooner ($p < 0.01$) with full controls. There is no such link in the Delay treatment: 0.08 days later ($p = 0.70$) unconditionally and 0.18 days later ($p = 0.20$) with full controls.

As a robustness check to mitigate concerns of endogeneity in the significant relationship between violence and redemption date in the Immediate treatment, we implement the IV specification analogous to Voors et al. (2011). For the second stage, The first stage regression reveals a significant relationship between the instrument, distance of residence from city center, and exposure to violence ($p < 0.01$). For the second stage, we find that the coefficient on violence becomes considerably larger in absolute value. Exposure is associated with coupon redemption 2.07 days earlier ($p = 0.09$) unconditionally, and 2.55 days earlier ($p = 0.05$) with full controls. Results are presented in Column 5 of Table 3.[12] Results for the interaction are presented in Table 4. With the addition of controls, the difference in difference estimate to 0.73 days is significant at the 5% confidence level ($p = .046$).

[12] We face a weak instrument problem in the Delay treatment. This is surprising given that both the instrument, distance of residence from the city center, and the potentially endogenous regressors, exposure to violence are balanced across treatment status. Ignoring the weak instruments problem and runing the specification for the Delay treatment, we find that exposure delays redemption by 0.83 days ($p = 0.91$) unconditionally and hastens it by 0.99 days ($p = 0.90$) with full controls.

In sum, we identify impulsive behavior in our data using experimental treatments, and show that exposure to violence exacerbates impulsivity by strongly correlating with propensity to redeem the coupon immediately when there is no delay. While the instrumental variables approach we borrow from the literature corroborates our findings in the Immediate treatment, it does not allow for the reliable estimates of the difference in differences.

4.2 Structural Estimates

A common approach to characterizing the severity and welfare effects of impulsive behavior is to estimate the parameters of an intertemporal utility function that allows for deviations from time-consistent planning. As discussed earlier, we focus on the β-δ formulation from Laibson (1997) and O'Donoghue and Rabin (1999). The utility function associated with consumption at time $t = T$, from the point of view of time $t = 0$ is

$$U(c_T) = \beta^{1(T=0)} \cdot \delta^{T} \cdot u(c_T) \quad , \qquad (1)$$

where $u(c_t)$ is the instantaneous consumption utility function. For this section, we assume that $u(c_t) = c_t$, and explore alternatives in the appendix. The key deviation from classic exponential discounting is that the β parameter matters only when comparing consumption in the present period to consumption in a later period.

In the context of our study, when $\beta < 1$ (present-bias), an individual in the Immediate treatment is more likely to choose to consume at the first opportunity ($t = 0$) than they would be in the Delay treatment ($t = 1$). This is true regardless of the exponential discount factor δ. A very patient individual with $\delta = 1$ would wait the maximum possible time in the Delay treatment to redeem. If they were present-biased, the only possible effect of moving them to the Immediate treatment would be to move them to immediate redemption option. This is why we put so much importance on the fraction of individuals choosing soonest-possible redemption in both treatments: under specific assumptions, comparing these statistics leads directly to an estimate of β. Before exploring more general techniques, we define this structure.

Because reducing choices to soonest-possible or not binarizes the data, the modeling approach we take here is to use a random-utility model. The unobserved value an individual *i,* gets from choice option *j,* is

$$V_{i,j}(X_j) = U(X_j) + \varepsilon_{i,j} \quad , \qquad (2)$$

where X_j is the consumption value associated with option *j* and U(·) is the observed utility function, for which we use the intertemporal formulation in (1).

First, assume that individuals simply compare redeeming as soon as possible to redeeming as late as possible. In the Immediate treatment this means comparing $1 + \varepsilon_{i,0}$ to $5 \cdot \beta \cdot \delta^4 + \varepsilon_{i,4}$. In the Delay treatment the structure of the comparison depends on when the comparison is being made (in the context of the β-δ model). Motivated in part by our reduced-form results, we model it as if it is made without the presence of the present-bias parameter (and with common factors removed). Thus, individuals in Delay will choose the soonest possible redemption if $1 + \varepsilon_{i,1} > 5 \cdot \delta^4 + \varepsilon_{i,5}$. The probability of this is

$$Pr(1 + \varepsilon_{i,1} > 5 \cdot \delta^4 + \varepsilon_{i,5}) = Pr(\varepsilon_{i,5} - \varepsilon_{i,1} < 1 - 5 \cdot \delta^4) = F(1 - 5 \cdot \delta^4) \quad , \qquad (3)$$

where $F(\cdot)$ is the CDF of the difference in epsilon terms.

Second, assume that the difference distribution is uniform on the interval [-1,1]. Thus, $F(x) = (x + 1)/2$ and $Pr(X^* = 1) = (1 - 5 \cdot \delta^4 + 1)/2$. Call z_D the observed frequency of soonest-possible choice in the Delay treatment. Matching this to the structural probability gives

$$z_D = (1 - 5\cdot\delta^4 + 1)/2 \quad \text{, implying } \delta = (2(1 - z_D)/5)^{1/4} \ . \qquad (4)$$

Similar formulations arise for the other choices for options to compare to the soonest possible option.

The next step is to derive the above probability for the Immediate treatment instead. The probability of redeeming immediately is

$$Pr(1 + \varepsilon_{i,0} > 5\cdot\beta\cdot\delta^4 + \varepsilon_{i,4}) = Pr(\varepsilon_{i,4} - \varepsilon_{i,0} < 1 - 5\cdot\beta\cdot\delta^4) = F(1 - 5\cdot\beta\cdot\delta^4) \ , \qquad (5)$$

which can also be matched to the observed frequency of soonest-possible choice, yielding

$$z_I = (1 - 5\cdot\beta\cdot\delta^4 + 1)/2 \text{ , implying } \beta = (2/5)\cdot(1 - z_I)/\delta^4 = (1 - z_I)/(1 - z_D) \ . \qquad (6)$$

A key feature of this formulation is that one arrives at this formula for the estimation of β regardless of which alternative to the soonest-possible redemption choice is used. Table 5 presents the parameter estimation results using the above method. The estimated present-bias parameters are well within the range established in previous literature, and show a substantial, economically significant gulf between those with and without direct exposure to violence during the war: 0.73 for those exposed and 0.88 for those unexposed. Estimates of δ are similar for both groups and demonstrate very high rates of discounting. Extrapolating from short-horizon estimates such as these to characterize long-horizon interest rate preferences is unlikely to be informative, as noted in similar studies.

The distributional assumptions required for such a simple estimation of the discounting parameters are highly specific. For that reason, we now take an approach with identification founded in the first-order condition of a non-binary utility maximization problem. Consider that an individual choosing when to redeem their coupon is trading off between the amount they receive and when they receive it. Calling the value of the coupon x, this means that an individual in the Delay treatment is solving

$$\max(t,x) \qquad U(t,x) = \delta^t \cdot x \quad \text{such that} \qquad x = t,\ 1 \leq t \leq 5 \qquad . \qquad (7)$$

Substituting in the constraint and taking a log expansion yields

$$\max(t) \qquad \ln(U(t)) = t \cdot \ln(\delta) + \ln(t) \qquad \text{such that} \qquad 1 \leq t \leq 5 \qquad , \qquad (8)$$

with first-order conditions

$$\ln(\delta) + 1/t = 0 \qquad , \qquad \text{implying} \qquad t^* = -1/\ln(\delta) \quad . \qquad (9)$$

An estimate of δ can be obtained as a non-linear combination of the average choice of t in the Delay treatment, when adjusted properly for the censoring at the bounds.

We use the solution above to develop an estimation strategy for β. First, we note that the maximization problem is slightly different in the Immediate group, such that

$$\max(t,x) \qquad U(t,x) = \beta^{1(t>0)} \cdot \delta^t \cdot x \quad \text{such that} \qquad x = t + 1,\ 0 \leq t \leq 4 \quad . \qquad (10)$$

Conditional on δ and $t = 0$, there exists a most preferred redemption date (which differs from (9) by only a subtraction of 1 from the t^* formula). Introducing present-bias is like setting up a binary choice problem between that most preferred date and immediate redemption. Specifically, we can plug the solution back into the log expansion of (10) to get

$$\ln(U(t^*)) = \ln(\beta) - \ln(\delta) - \ln(-\ln(\delta)) - 1 \qquad , \qquad (11)$$

which represents the utility obtained if the individual is constrained away from immediate redemption. If immediate redemption is chosen, then $U(0) = 0$. Therefore, an individual chooses to redeem immediately if

$$\ln(\beta) - \ln(\delta) - \ln(-\ln(\delta)) - 1 < 0 \quad . \qquad (12)$$

We rearrange (12) for the purposes of estimation to get that individuals redeem immediately if

$$\delta \cdot \ln(\delta) < -\beta/e \quad . \qquad (13)$$

Notably, we have not yet inserted an unobservable error term into (13) in order to generate choice probabilities. We take two approaches from this point. First, in the more traditional approach, we assume that the population mean of δ is measured with some error. For simplicity, we represent that as mean-zero uncertainty around our estimate of $\delta \cdot \ln(\delta)$ such that

$$Pr(t^* = 0) = Pr(\delta \cdot \ln(\delta) + \varepsilon < -\beta/e) \quad . \qquad (14)$$

In the case of ε being normally distributed, its standard deviation is calibrated such that 99% of its realizations imply the left side lies within the interval given by the theoretical restriction that $\delta \in (0,1)$. We also use a uniformly distributed ε, with strict bounds placed such that all realizations imply the left side obeys the theoretical range.

A less traditional approach we take uses both the estimate of the mean and standard deviation of δ in the population from the Delay treatment. While we still need to make a normality or uniformity assumption (on the distribution of δ around its mean), the standard deviation of that normal distribution is supplied from the estimation. Using the mean and standard deviation, we simulate the distribution of $\delta \cdot \ln(\delta)$, which is now the driving random variable. To translate the simulated distribution back to the maximum likelihood estimation, we fit it using the highly flexible, two parameter Beta distribution, and use it in the log-likelihood function

$$l(\beta) = \Sigma_i \, 1(t_i^* = 0) \cdot \ln(B((-\beta) + (1 - 1(t_i^* = 0)) \cdot \ln((1 - B(-\beta))) \quad , \qquad (15)$$

where $B(\cdot)$ is the CDF of the beta distribution used to approximate the $\delta \cdot \ln(\delta)$ distribution, and the argument of the CDF is simplified to $-\beta$ by the transformation of the inequality in (13) that puts the data in the support of the beta distribution. Results from the various approaches are presented in Table 6.

The models consistently estimate a gap between the exposed and unexposed group in the direction of exposed individuals exhibiting more present-bias. The magnitude of this effect varies across the standard deviation specifications, but is always significant at the 5% confidence level. More importantly, the size of the gap is substantial. While it appears that unexposed individuals do exhibit present bias of some degree, the level shifts considerably; β ranges from 0.96 to 0.74. For individuals exposed to violence during the war, β is much further from 1 and moves around less; β ranges from 0.76 to 0.67. Those familiar with the literature can understand the enormous impact on decision making of such a value of β, but we characterize it directly in the Welfare Analysis section.

The last approach we take is a fully simultaneous estimation of both parameters by combining the samples. The advantage of this approach is that we use the data in the Immediate treatment beyond an indicator variable, and we expect that this approach would be the first-instinct approach of many researchers. However, there are two downsides. First, because utility in the Immediate treatment features a discontinuity around $t = 0$, the convex model cannot be used without invoking an unordered probabilistic choice assumption: a poor use of the data from an efficiency standpoint. Second, the full distribution of choices in the Immediate treatment is theoretically inconsistent with those in the Delay treatment. As mentioned earlier, the only theoretical effect of moving from Delay to Immediate is to shift some choices from all of the later options to the soonest possible date. The frequency of choice at any particular non-immediate option should not go up in response. However, the frequency of choosing the latest-possible option is higher in Immediate than Delay. In other words, and combining this with the main result from earlier, the effect of moving from Delay to Immediate is to push choices to the extremes. Because these extreme choices are censoring points in the convex choice problem in (10), there is little precise information in the Immediate treatment to help us estimate δ. Despite these concerns, we use a multinomial logit choice model to estimate both δ and β together. The results suggest poor performance of the model, with results in Table 6.

4.3 Welfare Analysis

A central question in the behavioral economics literature on non-standard time preferences is whether the welfare effects of policies that limit choice biases are a) substantial and b) measureable. The second question is an issue of debate and is outside the scope of the current paper. Rather, we perform a straightforward calculation of the value that an individual, temporarily free of their bias, would associate with moving from a choice made with present bias to their optimal choice without, remembering that this decreases the overall value of all rewards by shifting them into the future. In other words, we calculate a sort of compensating variation associated with a policy move from Delay to Immediate, using estimates of δ and β from the previous section. Figure 2 shows a graphical interpretation of this measurement. Parameters are chosen for visual clarity and are not based on those from the previous section. This compensating variation is then measured in terms of bags of flour on day 0 of the study.

We cannot calculate the welfare estimate directly from the estimated utility parameters because our estimates of β come from a model of probabilistic choice. The result is that the indifference curve for individuals in the Immediate treatment associated with a utility level of 1 (immediate redemption) intersects the Immediate treatment budget (non-tangentially). This is to say that the *average individual* is not present-biased enough to redeem immediately. Therefore, we down-weight the welfare loss by the probability that an individual with the average β chooses to redeem immediately. This comes directly from the model of probabilistic choice in (14). Table 8 presents the welfare calculation exercise for a variety of utility parameter specifications, to recognize that the parameter magnitudes (especially δ) may vary considerably across contexts.

The row highlighted in green represents the closest match of parameters and moments to the unexposed group. The row highlighted in yellow represents the closest match of parameters and moments to the exposed group. In our case, the welfare loss in the exposed group is just under twice as large as for the exposed group, corresponding to about a tenth of a bag of flour on day zero. In other words, individuals in Delay achieve welfare that corresponds to an Immediate budget that involves 1.09 bags of flour on day 0 rather than 1 bag. A back-of-the-envelope calculation translates this difference to a quarter of a standard loaf of bread.

Robustness

One primary issue of robustness has to do with the maintained assumption in the previous section that individuals' utility in flour is linear. Previous work demonstrates that if this assumption is incorrect, it can bias the estimates of the discounting parameters. While we are primarily concerned with the difference in estimates of β across groups, the magnitude of the deviation of the estimates from one is important for weighing the importance of present bias. While utility curvature is not separately identified from δ in the data, we present results from the re-estimation of the convex model from Column (1) of Table 6 assuming an instantaneous utility function of the form $u(x) = x^{\alpha}$. Results are in Table 9. An α of 0.90 (0.10) has the interpretation that as one moves from having one loaf of bread (roughly 0.36 of one bag of flour in weight) to ten loaves of bread, the marginal utility of flour falls by about 21% (87%). However, this characterization assumes no flour consumption beyond that delivered by the coupon, which is unlikely to be the case.

All specifications feature a significant difference between the individuals who were or were not personal injured during the war. Decreasing α from one has the effect of diminishing the level of present-bias and compressing the across-group difference. This is because lower values directly decrease the utility returns to growth in the value of the coupon over time.

We also performed a version of the experiment with Coca-Cola instead of flour. The results were not highlighted for two reasons: 1) they are highly similar to the flour results and 2) the same individuals who first participated in the flour study went on to participate in the Coca-Cola study. The Appendix shows the corresponding results.

5. Conclusion

In this paper, we observe that direct exposure to violence causes a significant increase in present bias – a preference for immediate, smaller rewards instead of larger future benefits – but find that violence does not have a significant effect on discounting when all rewards lay in the future. Results from structural estimation suggest that exposure to violence has significant negative welfare consequences beyond those typically measured as a result of increases in present bias.

This shift in preferences implies greater dynamic inconsistency which may lead to self control problems across a variety of domains such as health (DellaVigna and Malmendier, 2006), savings (Laibson, 1997) and education (Ariely and Wertenbroch, 2002). As such, policies designed to help individuals and communities recover from violence may be more successful when accounting for the increase in impulsivity, and existing policies designed to help individuals mitigate impulsivity should take note on their histories of violence.

Future research should examine how long after being exposed to violence do individuals exhibit the shift in preferences. Additionally, in order to probe the generality of our results, it is important to learn whether different types of violence (e.g. domestic versus in the context of war) have similar effects on present bias.

Table 1: Observable Balance across Treatments

Variable	Immediate	Delayed	Difference
Female?	0.41	0.42	-0.01
Age	30.90	30.36	0.54
Secondary Education or Beyond?	0.79	0.77	0.02
Importance of Religion (1-4 scale)°	1.47	1.53	-0.06
How Safe Do You Feel? (1-4 scale)°	2.34	2.53	-0.20*
Access to Electricity (1-4 scale)°	3.32	3.33	-0.01
I am not afraid to take risks (1-4 scale)”	1.97	1.88	0.09
I feel I have no control over my life (1-4 scale)”	2.32	2.23	0.09
Expectations of Future Conditions (1-5 scale)°	3.72	3.73	-0.01
Can Most People Can Be Trusted? (1-4 scale)”	2.38	2.55	-0.17
Where do you live (1-3 scale, from city center)	1.57	1.61	-0.04
How long have you lived there (1-7 scale)^	5.17	5.55	-0.38
Home Damaged during War?	0.32	0.42	-0.09
Personally Injured during War?	0.38	0.30	0.08

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
°Scales where 1=Good/Important, 4/5=Not at all.
“Scale where 1 = Strongly Agree, 4 = Strongly Disagree
^Scale where 1 = Less than one week, 7 = More than two years.

Table 2: Observable Balance across Violence

Variable	Violence	No Violence	Dif.
Female?	0.47	0.42	0.05
Age	32.05	29.96	2.09
Secondary Education or Beyond?	0.73	0.81	-0.08
Importance of Religion (1-4 scale)°	1.63	1.44	0.19
How Safe Do You Feel? (1-4 scale)°	2.63	2.54	0.09
Access to Electricity (1-4 scale)°	3.43	3.26	0.17
I am not afraid to take risks (1-4 scale)”	2.05	1.86	0.19
I feel I have No Control Over My Life (1-4 scale)”	2.33	2.25	0.08
Expectations of Future Conditions (1-5 scale)°	2.38	2.22	0.16
Can Most People Can Be Trusted? (1-4 scale)”	2.53	2.55	-0.02
Where do you live (1-3 scale, from city center)	1.72	1.52	0.20**
How long have you lived there (1-7 scale)^	5.06	5.50	-0.44

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

°Scales where 1=Good/Important, 4/5=Not at all.

“Scale where 1 = Strongly Agree, 4 = Strongly Disagree

^Scale where 1 = Less than one week, 7 = More than two years.

Table 3: Exposure to Violence and Redemption Date by Treatment

	Treatment				
	Immediate	Delayed	Immediate	Delayed	Immediate
	(1)	(2)	(3)	(4)	(5)
Direct exposure to violence?	-0.65** (0.28)	-0.08 (0.21)	-0.92*** (0.30)	-0.18 (0.21)	-2.54** (1.27)
Male?			-0.79** (0.30)	-0.12 (0.21)	-0.78*** (0.27)
Children?			0.04 (0.34)	0.26 (0.25)	0.03 (0.04)
Employed currently?			0.14 (0.29)	-0.06 (0.21)	0.13 (0.30)
Not afraid of risk (0-3 scale)			0.10 (0.15)	-0.04 (0.10)	-0.05 (0.16)
Control over life (0-3 scale)			0.31** (0.13)	-0.05 (0.09)	0.32 (0.13)
Property damage during war?			0.49 (0.30)	0.49** (0.20)	0.48 (0.31)
Constant	3.67 (0.17)	3.30 (0.11)	2.87 (0.83)	2.99 (0.46)	1.47 (3.02)
Study Day Fixed Effects?	N	N	Y	Y	Y
Observations	136	122	128	120	128

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
Column (5) contains estimates from the 2SLS regression.

Table 4: Exposure to Violence and Redemption Date

	Model	
	(1)	(2)
Direct exposure to violence?	-0.08 (0.27)	-0.18 (0.27)
Delayed?	0.37* (0.21)	0.43** (0.21)
Interaction	-0.57 (0.36)	-0.73** (0.36)
Male?		-0.45** (0.17)
Children?		0.04 (0.03)
Employed currently?		0.02 (0.18)
Not afraid of risk (0-3 scale)		-0.07 (0.09)
Control over life (0-3 scale)		0.14* (0.08)
Property damage during war?		0.44** (0.18)
Constant	3.30 (0.15)	-0.21 (1.79)
Study Day Fixed Effects?	N	Y
Observations	258	250

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

Table 5: Discounting Parameter Estimates from Binary Choice Model

	Estimated Utility Parameter				
	β	δ (t=2)	δ (t=3)	δ (t=4)	δ (t=5)
Full Sample	0.82 (0.05)	0.91 (0.03)	0.78 (0.01)	0.77 (0.01)	0.78 (0.01)
Exposed to Personal Injury during War	0.73 (0.08)	0.89 (0.06)	0.77 (0.01)	0.76 (0.02)	0.77 (0.01)
Unexposed to Personal Injury during War	0.88 (0.06)	0.92 (0.04)	0.78 (0.02)	0.77 (0.01)	0.78 (0.01)
Difference	-0.16 (0.10)	-0.03 (0.07)	-0.01 (0.03)	-0.01 (0.02)	-0.01 (0.02)

Best match for Delay group modal non-immediate choice. Best match for Immediate group model non-immediate choice. The different specifications of t in the calculation of δ refer to the later option that we assume represents the alternative to soonest-possible redemption in the binary choice specification.

Table 6: Estimates of β from Convex/Probabilistic Models

	(1)	(2)	(3)	(4)
Pooled Sample	0.88 (0.05)	0.81 (0.03)	0.71 (0.02)	0.71 (0.02)
Unexposed to Personal Injury during War Only	0.96 (0.06)	0.85 (0.03)	0.74 (0.02)	0.74 (0.02)
Exposed – Unexposed Difference	-0.20** (0.09)	-0.13** (0.06)	-0.07** (0.04)	-0.07** (0.03)
Population Standard Deviation?	N	N	Y	Y
Error in δ distribution	Normal	Uniform	Normal	Uniform

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. The pooled and separate estimates come from different specifications.

Table 7: Discounting Parameter Estimates from Multinomial Logit Model

	Estimated Utility Parameter	
	β	δ
Full Sample	0.46 (0.10)	0.81 (0.02)
Exposed to Personal Injury during War	0.55 (0.31)	0.29 (0.09)
Unexposed to Personal Injury during War	1.28 (0.32)	0.32 (0.06)
Difference	-0.73 (0.45)	-0.03 (0.11)

Table 8: Welfare Loss Associated with the Move to Immediate from Delay

β	δ	t^* (Delay)	$x^*(U^*,t=0)$ (Delay)	$Pr(t^*=0)$ (Immediate)	Loss: $(1 - x^*(U^*,t=0)) \cdot Pr(t^*=0)$ (measured in bags of flour at $t = 0$)
0.95	0.99	99.50	36.60	0.01	-0.31
0.85	0.99	99.50	36.60	0.02	-0.61
0.75	0.99	99.50	36.60	0.03	-1.11
0.65	0.99	99.50	36.60	0.05	-1.93
0.95	0.95	19.50	7.17	0.02	-0.11
0.85	0.95	19.50	7.17	0.03	-0.20
0.75	0.95	19.50	7.17	0.06	-0.34
0.65	0.95	19.50	7.17	0.09	-0.56
0.95	0.85	6.15	2.26	0.07	-0.09
0.85	0.85	6.15	2.26	0.11	-0.14
0.75	0.85	6.15	2.26	0.17	-0.21
0.65	0.85	6.15	2.26	0.24	-0.30
0.95	**0.75**	**3.48**	**1.28**	**0.17**	**-0.05**
0.85	0.75	3.48	1.28	0.25	-0.07
0.75	**0.75**	**3.48**	**1.28**	**0.34**	**-0.09**
0.65	0.75	3.48	1.28	0.44	-0.12

Best match for unexposed group estimates from the convex model and data moment. Best match for exposed group estimates from the convex model and data moment.

Table 9: Estimates of β from Convex/Probabilistic Model, with Varying α

	$\alpha = 0.90$	$\alpha = 0.70$	$\alpha = 0.50$	$\alpha = 0.30$	$\alpha = 0.10$
Pooled Sample	0.88 (0.04)	0.91 (0.03)	0.94 (0.03)	0.99 (0.02)	1.06 (0.02)
Unexposed to Personal Injury during War Only	0.96 (0.06)	0.97 (0.05)	0.99 (0.04)	1.03 (0.03)	1.09 (0.02)
Exposed – Unexposed Difference	-0.18** (0.08)	-0.15** (0.07)	-0.12** (0.06)	-0.10** (0.05)	-0.08** (0.04)

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. All models mimic the specification from Column (1) of Table 4, in which we model the error in the discount rate using normal, mean zero error around the estimate of the population mean. The pooled and separate estimates come from different specifications.

Figure 1

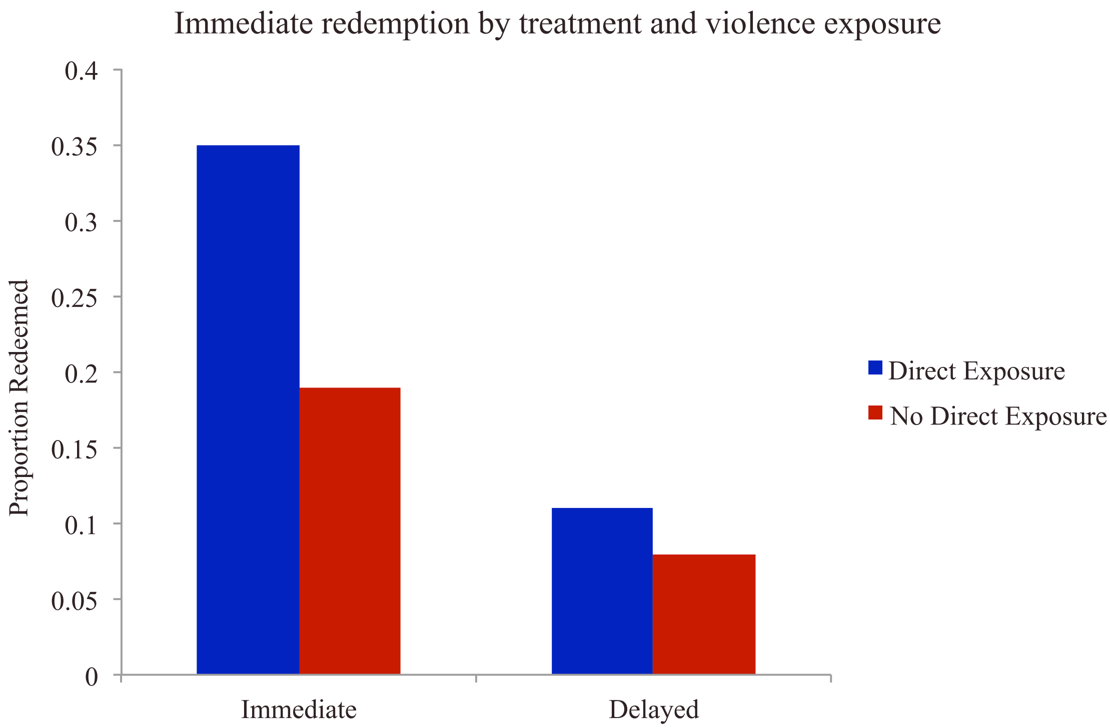

Immediate redemption by treatment and violence exposure
Proportion Redeemed
0.4
0.35
0.3
0.25
0.2
0.15
0.1
0.05
0
Immediate
Delayed
Direct Exposure
No Direct Exposure

Figure 2: Measuring Welfare Loss due to the Immediate Treatment using Indifference Curves

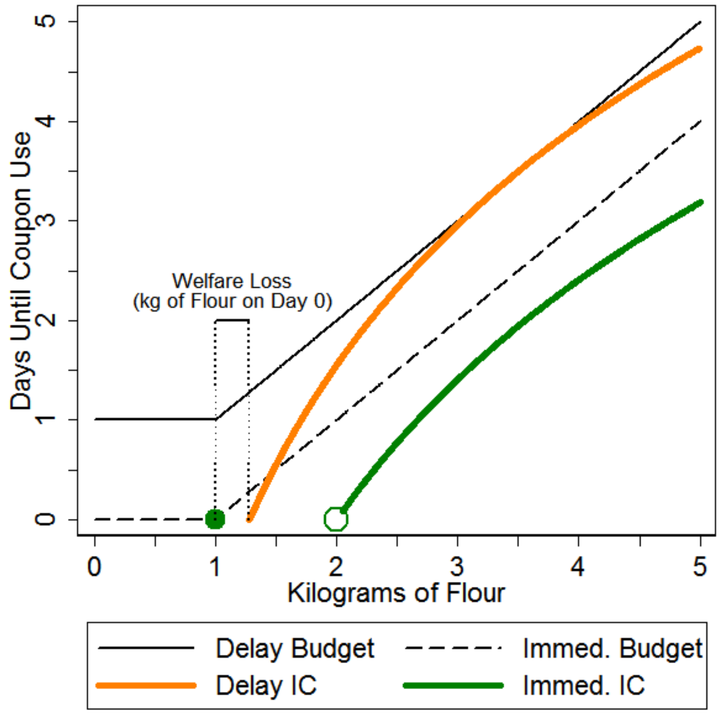
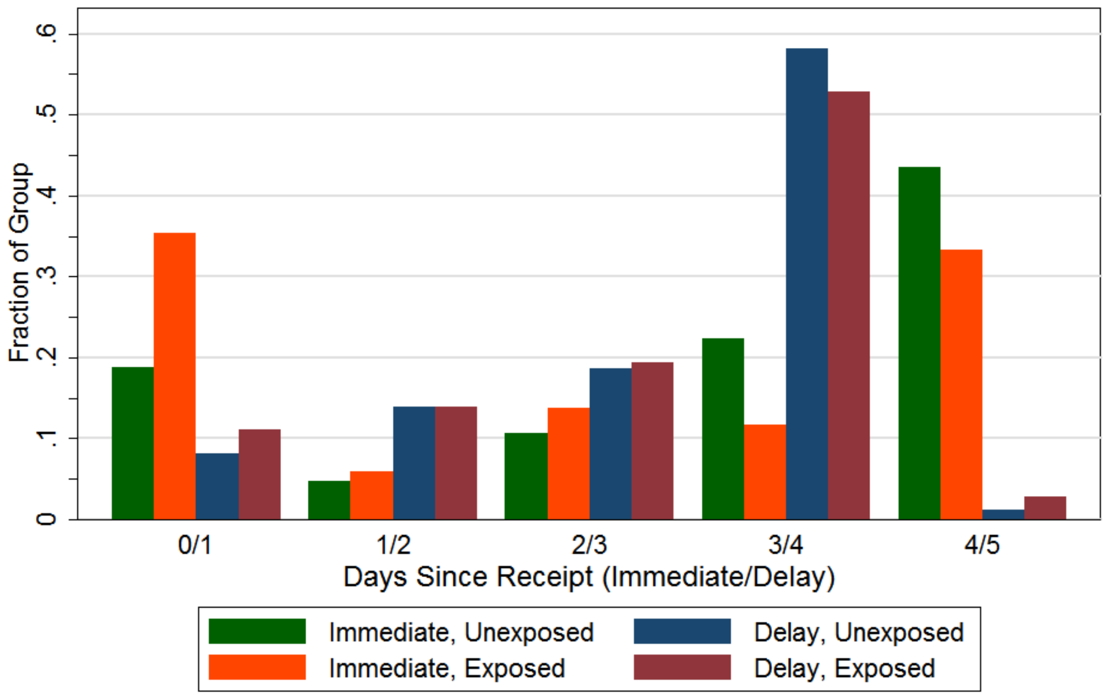


Figure 3: Full Distribution of Choices by Treatment and Violent Exposure



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