Constructing Independent Evidence from Regression and Instrumental Variables with an Application to the Eect of Violent Conict on Altru-ism and Risk Preference

Bikram Karmakar[1] and Dylan S. Small

Abstract. To provide an unbiased estimate, a regression analysis depends, among other things, on there being no unmeasured confounding. Often, unmeasured confounding is thought to be possible, but not severe; leading to a secondary instrumental variables (IV) analysis. However, these two analyses are correlated. It is unclear how much independent evidence is provided by the IV analysis. We resolve this redundancy using a new estimator which extracts the part of the regression estimator uncorrelated to the IV based 2SLS estimator. We apply our approach to analyze the eect of exposure to violent conict on preferences for altruistic behavior, time and risk.

Keywords: Independent pieces of evidence, instrumental variables, preferences, sensitivity analysis, unmeasured confounding.

1 Introduction

1.1 Instrumental Variables Analysis as a Secondary Analysis: A Redundancy

The linear regression model is often used to infer about the causal eect of a treatment controlling for various confounders. Consider the linear regression model

$$H_8 = 3_8 V, \ U^> x_8, \ D_8 - \tag{1}$$

for $8 = 1 - 2 - \bullet\bullet\bullet - =$, popular in practice to assess the signicance of the eect of a treatment 3 on another variable H, controlling for confounders x. The Gauss-Markov theorem says, the best possible linear unbiased estimator for V under the conditions of zero mean, uncorrelated errors and homoskedasticity of the errors is the ordinary least squares (OLS) estimator, $\hat{V}_{\$!(}$. Most empirical studies of the eect of one variable on the other start with the traditional t-statistics and the corresponding p-value calculation based on $\hat{V}_{\$!(}$. After that, to address the most common criticism that there might be unmeasured confounding variables that make the OLS estimate biased and thus

a test of signicance of V based on $V_{\hat{\$}!(}$ invalid for assessing the causal eect of the treatment 3, often an instrumental variables (IV) based analysis is presented (see e.g., Angrist, Chen and Song, 2011; Gonzйez-Alegre, 2015; Parker et al., 2013). Brookhart et al. (2010) recommend IV methods as a secondary analysis to regression analysis for the following setting: \IV methods are inecient and should not be used as a primary analysis unless unmeasured confounding is thought be strong ... If unmeasured confounding is possible, but not expected to be severe, IV may be more appropriately used as a secondary analysis." IV analysis has been used as a secondary analysis in various studies (Allen, 2014; Earle et al., 2001; Wang et al., 2005).

[Figure 1 about here.]

[Table 1 about here.]

When IV is used as a secondary analysis to a primary regression analysis using the same data set, the two analyses are correlated. Let's consider the simplest instrumental variable model with a single instrument, I, for our variable of interest 3

$$3_8 = I_8 W, \ Z^> x_8 , \ E_8 \bullet \tag{2}$$

The standard estimate of the coecient V using instrumental variables is the two stage least squares (2SLS) estimator, which is obtained by rst regressing 3 on I and then regressing H on the predicted values of 3 from the rst regression. The 2SLS estimate of V here is denoted by $V_{2(!(}^{\wedge}$.

Figure 1 shows a scatter plot of the two estimators $\hat{V}_{\$!(}$ and $\hat{V}_{2(!(}$ for 1000 simulations with V = 0•1, W = 1, and no covariates. The high (positive) correlation of the two estimators is immediate from this plot. A quantitative view of the correlation structure is given in Table 1. Even with a very small correlation of 3_8 and I_8 through equation (2), we can see the high amount of correlation between the two estimators. For example, with W= 0•5 these two estimators in this simple model have correlation around 0.5. When W= 2, the simulated values of the correlation are as high as 0•92. Therefore, in an empirical study, a chance error that led to concluding statistical signicance of the eect based on the OLS estimator when there is truly no eect could very well be the reason for a signicant eect in the secondary IV analysis as well. This is worrisome, since we would not like to be a fool who bought \several copies of the morning paper to assure himself

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that what it said was true" (Wittgenstein, 1958, #265, quoted in Rosenbaum, 2010) by reporting two highly correlated pieces of evidence as if they are separate pieces. In supplement section ??, we provide additional simulation results that also include a covariate.

Suppose that after the primary regression analysis nds a signicant treatment eect, the secondary IV analysis also nds a signicant treatment eect. Does this provide meaningful conrmatory evidence for there being a true causal eect of treatment? As these analyses are correlated, it is dicult to interpret such ndings.

1.2 Summary of the Proposed Methodology

In this paper, we develop an approach to constructing two independent pieces of evidence from a regression analysis and an IV analysis for the setting when the IV analysis is used as a secondary analysis. We rst present the methodology in the context of the linear structural equations model. Then, in Section ?? of the supplement, we extend the method to a general treatment eect model that allows for heterogeneity and nonadditive eects.

For the linear structural equations model, our procedure rst carries out the primary OLS analysis. If this analysis rejects the null hypothesis, we propose to present two secondary analyses, one based on the instrumental variable based 2SLS estimator and a second based on a new estimator, denoted in Figure 2 by $\hat{V}_-$, which is asymptotically independent from the 2SLS estimator. These two secondary analyses provide separate evidence about the causal eect.

Using the testing of hypotheses in order argument of Rosenbaum (2008) we show that for this two stage procedure, which conducts three tests of hypothesis each at a prespecied level U, the overall type I error is still controlled at level U.

[Figure 2 about here.]

Rosenbaum (2010) uses the term evidence factors when two independent analyses are conducted for one null hypothesis where the individual analyses depend on dierent sets of assumptions. In the case when only one of two sets of assumptions fails, while the other remains true, the decision based on combining the two analyses remains reliable. In our proposal, the two evidence factors in the secondary analysis are based on the estimators $\hat{V}_{2(!(}$ and $\hat{V}_-$, both of which test the same null hypothesis of no causal eect of the treatment while they are derived from dierent assumptions.

The 2SLS estimator assumes that the instruments are valid, namely, A.1) the instruments are associated with the treatment, A.2) instruments cannot directly aect the outcome, and A.3) there is no unmeasured confounding between the instruments and the outcome. The EX estimator assumes that A.4) there is no unmeasured confounding between the treatment and the outcome, and A.1) the instruments are associated with the treatment. Note that, the assumption A.1 is testable while A.2, A.3 and A.4 are not. We also discuss later a way to assess the sensitivity of these analyses to potential violations of assumptions A.2, A.3 and A.4.

The rest of the paper is organized as follows. Our motivating example is presented in Section 2. A new estimator, the EX estimator, is dened via (8) in Section 3.1. Section 3.2 veries the consistency of the proposed estimator. The technical result showing that the 2SLS analysis and the proposed analysis are asymptotically independent is proved in Section 3.3. Results for our motivating example are presented in Section 4. Section 5 discusses methods of sensitivity analysis and results of sensitivity analysis for the example are presented in Section 6. The supplement further provides an extension of the estimation procedure to general instrumental variables model that allows for heterogeneity and nonadditive eects.

2 Motivating Example: Does Exposure to Violence Alter Preferences? Data from Civil War in Burundi

Wars have been referred to as \development in reverse" because of their destruction of capital (World Bank, 2003). In several countries over the past century, we have observed remarkable economic and social rebounds after a period of war. These postwar recoveries can be partly attributed to generous humanitarian aid (Regan, 2000). An additional hypothesis for rapid economic growth after war is that, violent conict can spur societal reforms that promote economic growth (e.g., Cramer, 2006). One mechanism by which violent conict could spur (or hinder) economic growth is that the conict may alter preferences (Blattman, 2009; Bellows and Miguel, 2009; Voors et al., 2012; Rohner et al., 2013).

Voors et al. (2012) conducted a study to test whether exposure to a violent conict changes people's preferences toward altruistic behavior, time preference (in particular, a measure of impatience for gaining some money now vs. more money later) and risk preference. The study was based

on data on the history of violence in dierent communities of Burundi combined with data from eld experiments used to determine preferences.

Burundi went through a civil war between 1993{2006, resulting from long standing ethnic divisions between the Hutu and Tutsi ethnic groups; in the war, the Tutsi-dominated army clashed with Hutu militias. The war left over 300,000 people dead and many displaced (BBC News, 2004).

Following Voors et al. our treatment variable of interest is a measure of conict victimization, the number (relative to the community) of war-related deaths between 1993{2003. We have household level observations on 35 communities on various household level variables and various community level variables. These communities are shown in the map in Figure 3. Out of the 35 communities, 11 communities did not experience any form of violence while among the communities which were exposed to violence the percentage of death during the period 1993{2003 varied from 0.078% to 15.62%. Table 2 provides the descriptive statistics of the variables in the data set.

Four outcome variables are used to quantify preferences toward altruism, risk and time preference. Experimental games were used to measure these preferences. Degree of altruism was measured on a scale from 0 (purely selsh preference) to 100 (unselsh preferences) with a value of 50 denoting the social optimum which maximizes joint payos. The average of this measure was 27, which indicates an overall selsh preference. Risk preferences for gains/losses were measured on a scale of 0 to 3. The average values of these risk preferences for gains and losses were 1.87 and 2.31 respectively, indicating an overall risk taking behavior. Finally, time preference of a subject was measured as the discount rate 3 such that the subject is willing to choose a xed amount of money at d% interest rate two weeks later over receiving the xed amount the following day. For detailed information on these experiments see the web Appendix to Voors et al. (2012).

To estimate the causal eect of exposure to violence on preferences Voors et al.'s primary analysis regressed the preference measures on the exposure to violence. For this analysis to provide a consistent estimate of the causal eect, there should be no unmeasured confounding. Support for the assumption of no unmeasured confounding is provided by the fact that the violence in the civil war was thought to be largely indiscriminate because of the army's inability to identify rebels, a desire for extermination, \revenge by proxy", plundering, and a perceived need to demonstrate power as part of the tactics of fear to control a population (Uvin, 1999; Krueger and Krueger, 2007; Human Rights Watch, 1998). However, there are possible unmeasured confounders. Communities

with greater ethnic or political cleavages may be easier targets because they are less able to defend themselves or, conversely, communities with fewer cleavages may be more likely to be targeted because of their potential support for the \other side" (Voors et al., 2012). To address concerns about unmeasured confounding, Voors et al. conducted a secondary instrumental variable (IV) analysis using altitude and distance to Bujumbura, Burundi's capital, as instruments.

We consider two instrumental variables following Voors et al. { distance to the capital Bujumbura and the altitude of the community both on a log scale. These two variables are fairly negatively correlated with a correlation value of 0•39. A principal component analysis of these two instruments shows that 95% of the total variability is explained by the rst component which has a loading of 0•995 on the distance to the capital.

[Figure 3 about here.]

Figure 3 shows the geography of Burundi and remoteness of various cities of the country from its capital Bujumbura which is situated on the western land border by Lake Tanganyika. Data was collected from 35 randomly selected communities from 13 provinces. Within each community a number of random household heads were surveyed to measure their preferences. Dierent communities in the data are shown in the map with a color gradient depicting the violence level experienced by the community during the war. The primary regression analysis tests the null hypothesis that violence does not aect preferences by assuming that violence experienced by a community, re- ected by the color gradient in Figure 3, is independent of what the community's preferences would have been in the absence of violence. The IV analysis using distance from the capital as the IV tests the null hypothesis of no eect of violence on preferences by assuming that distance to capital is independent of what the community's preferences would have been in the absence of violence. Essentially, the IV analysis is assuming that what the average community preferences would be in the absence of violence in a contour of average distance from the capital (dashed contours) is the same across contours while allowing for unmeasured confounding within a contour, whereas the regression analysis assumes no unmeasured confounding both within a contour and across contours.

For example, the rst row of Table 3 (discussed in detail later in Section 4) shows the regression analysis (OLS) and IV analysis (2SLS) for the eect of exposure to violence on altruistic preferences. The regression analysis shows a signicant positive eect, providing evidence that exposure to

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violence causes altruistic preferences under the assumption that altruistic preferences in the absence of violence would be the same on average between the communities that were actually exposed to high levels of violence compared to communities actually exposed to low levels of violence. The IV analysis also shows a signicant positive eect, providing evidence that exposure alters altruistic preferences under the assumption that altruistic preferences in the absence of violence would be the same on average among communities that are near vs. far from the capital. But these two analyses are not separate; they use the same data set and they are correlated.

[Table 2 about here.]

3 Methodology

3.1 A New Estimator

We rst describe our causal model involving instrumental variables. Let the units of the data be denoted by $8 = 1-2- \bullet \bullet \bullet -=$. We denote by H the outcome, 3 the treatment, and I the instrument(s). Each unit 8 has a potential outcome $H_8^{"3-I"}$, the outcome the unit would have if the unit received the treatment 3 and the instrument value I (Neyman, 1923; Rubin, 1974). We rst start by assuming the exclusion restriction, which says that,

A.2 The instrument only aects the outcome through its eect on treatment received, i.e., the instrument has no direct eect on the outcome.

This allows us to write the potential outcome $H_8^{"3-I"}$ as $H_8^{"3"}$ dropping the eect of the instrument. In the Burundi data the treatment is the level of violence measured as the percentage of death in violent conict in the community. The outcome is a measure of preference. Each unit thus has a vector of potential outcomes and depending on the observed value of the treatment for unit 8, a particular corresponding potential outcome value is observed. Let $8 := 3_\varepsilon^{>1B}$ denote the observed level of the treatment for unit 8, $/_8 := I^{>1B}$ denote the observed instrument level, and $H_8 := H_8^{3_8^{>1B}}$ denote the observed outcome. An additive, linear constant-eect causal model for the potential outcomes is (see Holland, 1988)

$$ H_8^{"3"} = H_8^{"0"} , 3 \bullet \tag{3} $$

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Our interest is in the parameter β. β is the causal effect of the treatment D on the outcome variable Y for a one unit increase in the treatment level.

A variable (or vector of variables) Z_i is a valid instrument for estimating β if along with A.2, the following are satisfied (see Angrist, Imbens and Rubin, 1996; Small, 2007)

A.1 Z_i is associated with the observed treatment $D_i = D^{Z_i}_i$.

A.3 There is no confounding between the instrument and the observed outcome. Under model (3) this is the same as saying $Y^{,0"}_i$ and Z_i are independent.

Often we need to condition on observed covariates so that proposed instrument(s) Z_i satisfies these conditions. Let X_i be the vector of observed covariates and conditioning on X_i the conditions A.1, A.2, and A.3 are satisfied by Z_i. We consider a linear model for $„Y^{,0"}_i \mid X_i, Z_i"$ and rewrite (3) as

$$Y^{,D"}_i = \beta \, , \, D_i, X_i - E\,„D_i \mid X_i, Z_i" = 0 \bullet \tag{4}$$

We consider the following model on the observed data under (4) with a linear model on the $D_i = D^{Z_i}_i$,

$$Y_i = \beta \, , \, D_i, X_i - E\,„D_i \mid X_i, Z_i" = 0 - D_i = \gamma_1 X_i, \gamma_2 Z_i, {}^{>}+_i \bullet \tag{5}$$

The first equation above is implied by the potential outcomes model (4). The second equation is an additional assumption. Model (5) is the linear structural (simultaneous) equations model that is widely used in economics (Hausman, 1983).

In our presentation we frame the problem in a general setting allowing the treatment to be a multilevel variable. In matrix notation model (5) is written as

$$Y = \beta \, , \, X \, , \, D - \epsilon \tag{6}$$

$$ = \gamma_1, Z\gamma_2, +\bullet \tag{7}$$

In the first equation, $Y = „Y_1 - \bullet\bullet\bullet - Y_="^{>}$ is a $n \times 1$ vector of outcome variable, $D = „D_1 : \bullet\bullet\bullet : D_="^{>}$ is a $n \times k$ matrix of observations on a k dimensional variable of interest (i.e., treatment variables), $X = „X_1 : \bullet\bullet\bullet : X_="^{>}$ is a $n \times p_1$ matrix of control covariates, and $\epsilon = „\epsilon_1 - \bullet\bullet\bullet - \epsilon_="^{>}$ of dimension $n \times 1$, is the error term. In the IV model (7) $Z = „Z_1 : \bullet\bullet\bullet : Z_="^{>}$ is a $n \times p_2$ matrix of p_2 instruments

and $+ = „+_1 : \bullet \bullet \bullet : +_="^>$ is a $= <$ matrix. For a matrix let % be the projection matrix on the column space of and " be the projection matrix on the orthogonal space of the column space of . With a slight abuse of notation we write $\%_{-/}$ and $"_{-/}$ for $\%_{»-:/…}$ and $"_{»-:/…}$, respectively.

Our interest is in the parameter . The OLS and the 2SLS estimators of are $\wedge_{\$!(} = »^> "_{-…}{}^1 {}^> "_-$ H and $_{2(!(}\wedge = »^> \%"_{-/}…{}^1 {}^> \%"_{-/}$ H• The proposed E X estimator is

$$\wedge_- = »^> "_{-/}…{}^1 {}^> "_{-/} H• \tag{8}$$

This estimator can be calculated as the coecient of when H is regressed on and $-−/$.

The purpose of this new estimator is for it to be used along with the 2SLS estimator to strengthen the supplementary analysis when the OLS analysis is conducted as the primary analysis. Below, we provide discussion of how existing literature motivates this estimator.

The 2SLS estimator motivates our estimator $\wedge_-$. The 2SLS estimator is easily calculated using a two stage least squares method by which in the rst stage, a predicted value of () from (7) is calculated and in the second stage, is estimated through regression model (6) by replacing . Our proposed estimator, $\wedge_-$ is a dierent type of two stage estimator where at the end of the rst stage of regressing on - and /, we store the residual + and in the second stage, is estimated through regression model (6) by $+^\wedge$ replacing .

The 2SLS estimator uses the part of the variation in that is explained by the instrument / to estimate the treatment eect. Our proposed estimator is the converse, it uses the part of the variation in that is not explained by the instrument / to estimate the treatment eect.

This two-stage procedure might remind a reader of the augmented regression version of the Durbin-Wu-Hausman test (Durbin, 1954, Wu, 1973, Hausman 1978; see Davidson and MacKinnon, 2003, Section 8.7 for augmented regression version) for testing whether there is unmeasured confounding assuming valid instruments. Yet, they are dierent. The Durbin-Wu-Hausman test tests whether the expected value of the dierence of the OLS and 2SLS estimator is zero, as it is expected to be when the outcome model is free of any unmeasured confounders. The augmented regression version of the Durbin-Wu-Hausman test, which is asymptotically equivalent to the original test, uses the residuals of the rst stage regression t, +, as a new independent variable in the second stage regression alongside and - and tests if its coecient is zero. Thus, unlike the two stage

procedure that gives us $\hat{\beta}$ with the second stage regressing H on $<$, $:_1$ regressors, the second stage regression in the augmented regression version of the Durbin-Wu-Hausman test regresses H on $2<$, $:_1$ regressors.

3.2 Consistency of the Proposed Estimator

Under standard conditions on the variables, the proposed estimator $\hat{\beta}$ in (8) is a consistent estimator of the parameter under the assumption of no unmeasured confounding between the treatment and the outcome.

Theorem 1. Suppose the observations are iid with nite second moments such that $f_{„} -> /_{1}^{>„>}$, $_{1'} -> /_{1}^{>„} g_1$ is non-singular, $_{„} D_1 j -- /" = 0$, and $+_1$ is not a linear function of $_{„-_1}{}^{>} : /_1{}^{„>}$ with probability 1. Then $_{„}+^{>} D j -- /" = 0$ implies $\hat{}$ is consistent for .

Proof. We write $\hat{\beta}$ as, $\hat{} = $, $»+^{>}"_-/+ \ldots {}^{1}+^{>}"_-/D$. Then the proof follows from similar arguments as in Chapter 5.2 of Wooldridge (2010).

The assumption $_{„}+^{>} D j -- /" = 0$ or equivalently $_{„}{}^{>} D j -- /" = 0$ is the assumption that conditional on the observed covariates and the instruments, there is no unmeasured confounding between the treatment and the outcome. This condition implies no unmeasured confounding in (6), i.e., is exogenous with respect to H conditional on the covariates $-$, if assumption A.3 is satised.

If the condition of Theorem 1 is not satised, the bias of the estimator (8) can be quantied. The bias of the estimator $\hat{\beta}$ is of the order of $_{„}+_1 D_1{}^{>} j -- /"$. Details of the bias calculation are given in the supplement.

3.3 $\hat{}_{2(!(}$ and $\hat{\beta}$ are Asymptotically Independent

We show that $\hat{}_{2(!(}$ and $\hat{\beta}$ are asymptotically independent. Therefore, evidence based on the two IV based estimators $\hat{}_{2(!(}$ and $\hat{\beta}$ carry separate information about the coecient .

Theorem 2. Assume the conditions of Theorem 1. Also assume the following,

(a) D_1 and $+_1$ have nite fth moments conditional on $--/$,

(b) $_{„}+_1 j -- /" = 0$, and

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(c) „D²+₁₁ j -− /" = 0 and „D²+₁+> j₁ -− /" = f² −/ „+₁+₁ j >− /".

Then, ^₂(!(and ^₋ are asymptotically independent.

Our results all hold true under setups with heteroskedasticity and clustering in which robust standard errors are valid (Huber, 1967; White, 1980).

3.4 OLS, 2SLS, and EX Estimators

An OLS analysis provides a consistent estimator of the causal parameter assuming no unmeasured confounding between the treatment and the outcome. The instrumental variables based 2SLS analysis is consistent when the instruments satisfy assumptions A.1, A.2, and A.3. Even if there is concern about unmeasured confounding and the instruments are thought to be valid (satisfy A.1, A.2 and A.3), OLS may still be the preferred method for primary analysis if the unmeasured confounding is thought not likely to be serious, because 2SLS is inecient compared to OLS (Sawa, 1969; Basu and Chen, 2014). We propose a method of analysis where both the OLS analysis and the 2SLS analysis can be considered together, while keeping the OLS analysis as primary. If we are going to consider the two analyses together, then we need to control for multiple testing. Bonferroni treats the OLS and IV analyses equally. The testing in order method we suggest gives more power to the OLS analysis while allowing us to look at the 2SLS and EX analyses. In Section 3.3, we showed that the two instrumental variables based estimators, the 2SLS estimator and the EX estimator, are asymptotically independent. Further, we note that the EX estimator is valid as long as assumption A.1 is satised and unmeasured confounding is not severe (i.e., „>D j -− /" = >„1"). Thus, these two estimators form evidence factors in testing for a treatment eect (Rosenbaum, 2010). We propose to change the standard practice of considering 2SLS analysis as a secondary analysis and provide both 2SLS and EX analysis as secondary analyses.

Our method is the following:

1. Conduct the OLS analysis and test for a treatment eect at level 0.05. If the OLS analysis does not reject, stop.

2. If the OLS analysis rejects, then test for a treatment eect using each of the 2SLS and EX analyses, both at level 0.05.

Even though our method involves potentially carrying out three dierent signicance tests at level 0.05, we show in the next section that our method controls the overall type I error rate because of the sequential way in which tests are performed and the relationship between the tests.

3.5 Primary and Secondary Analyses: Testing of Hypotheses in Order

Let $E^{j-}_{„”}$ denote the conditional expectation given -. In the population, $\hat{}_{\$!(}$ estimates the population quantity $mE^{j-}_{„}H_1 j _1”m_1$ since it estimates the slope of when H is projected on - and . Similarly, $\hat{}_{2(!(}$, which is computed as the coecient of the projection of on / when the outcome H is regressed on this projection, estimates the population quantity $mE^{j-}fH_1 j E^{j-}_{„1} j /_1”gmE^{j-}_{„1} j /_1”$. Finally, $\hat{}_-$ estimates the population quantity $mE^{j-}_{„}H_1 j _1-/_1”m_1$. These population parameters do not have a causal interpretation unless further assumptions are made. For example, $mE^{j-}_{„}H_1 j _1”m_1$ only indicates the causal eect of on the outcome under the assumption of no unmeasured confounding, $mE^{j-}fH_1 j E^{j-}_{„1} j /_1”gmE^{j-}_{„1} j /_1”$ has a causal interpretation only when $/_1$ is a valid instrument, and nally $mE^{j-}_{„}H_1 j _1-/_1”m_1$ has causal interpretation when we have no unmeasured confounding and the instrument is associated with .

Whether or not any of the assumptions are valid, signicance tests based on these three esti-mators $\hat{}_{\$!(}$, $\hat{}_{2(!(}$ and $\hat{}_-$ correspond to the following three null hypotheses of the population quantities they estimate, respectively: $_{\$!(} : mE^{j-}_{„}H_1 j _1”m_1 = 0$, $_{2(!(} : mE^{j-}fH_1 j E^{j-}_{„1} j /_1”gmE^{j-}_{„1} j /_1” = 0-$ and $_- : mE^{j-}_{„}H_1 j _1-/_1”m_1 = 0$.

We observe that the three hypotheses above are interrelated as follows. Suppose $mE^{j-}_{„}H_1 j _1-/_1”m_1$ is a continuous function of $_1-/_1$, then, $mE^{j-}_{„}H_1 j _1”m_1 = E^{j-} mE^{j-}_{„}H_1 j _1-/_1”m_1$. Further, suppose $mE^{j-}_{„1} j /_1”m/_1 < 0$, which correspond to assumption A.1 that the instrument is associated with the treatment, and $mE^{j-}_{„1} j /_1”m/_1$ is continuous, then

$$
\frac{mE^{j-}fH_1 j E^{j-}_{„1} j /_1”g}{mE^{j-}_{„1} j /_1”} = E^{j-}_1 \frac{m}{mE^{j-}_{„1} j /_1”} E^{j-}_{„}H_1 j _1/ ”_1
$$

$$
= E^{j-}_1 \frac{mf_1 E^{j}_{„} j /_1”g}{mE^{j-}/ _1 j _1 m_1} E^{j-}_{„}H_1 j _1-/_1” \bullet
$$

As a consequence, if $_{2(!(}$ is true, then $_-$ cannot be false, and if $_{\$!(}$ is false, $_{2(!(}$ cannot be true.

We partition these three hypotheses as $T = fT_{primary}-T_{secondary}g$ where $T_{primary} = f_{\$!(}g$ and

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$T_{secondary} = f_{2(!(-\_}g.$ Then with the ordering $?A8<0AH$ $B42>=30AH$ on the index set of

T, in the language of Rosenbaum (2008), the partition of disjoint intervals of hypotheses T is sequentially exclusive and Rosenbaum's Proposition 2 applies. Hence, we have the proof of the following proposition.

[Table 3 about here.]

Proposition 1. The probability that the two step method proposed earlier, in Section 3.4, rejects at least one true hypothesis is at most $U = 0\bullet05$.

When OLS analysis shows signicance and the proposed method allows us to perform two secondary analyses, based on the results of the secondary analyses we can say that we have 'no evidence' or 'one piece of evidence' or 'two pieces of evidence' at the secondary level, depending on whether neither or only one or both of the secondary estimators gives statistical signicance.

4 Results: Three Estimates in the Burundi Data

Table 3 reports the estimates of the eect of violence and the corresponding robust standard errors clustered at the community level, for the four outcome variables. The OLS analyses show statistically signicant positive eects of conict on preferences toward altruistic behavior, risk for gains and time preference. The OLS estimate does not show a signicant non-zero eect on risk preferences losses. For the three outcomes where OLS analysis shows statistical signicance, we perform the 2SLS and EX analysis simultaneously. When considering the eect of conict on the degree of altruism both secondary analyses show signicance. Only the EX analysis shows a positive eect of conict on risk preferences gains at the 5% signicance level. The 2SLS estimate is not signicant for risk preferences gains. The 2SLS estimate for time preference is statistically signicant while the EX estimate is not. Therefore, a naive summary of Table 3 would say that, a person exposed to violent conict tends to exhibit more altruistic behavior, be more of a risk taker in the context of gaining and be less patient.

Table 3 tells us that, when considering the eect of violent conict on the degree of altruism we have two pieces of evidence against the null hypotheses of no eect while for the eect on risk preferences for gains and time preference we only have one piece of evidence at signicance level $0\bullet05$.

[Figure 4 about here.]

We can further test for any specied eect of the treatment. For example, suppose we are interested in the null hypothesis of $_0$: = 3 for the eect of conict on degree of altruism. The OLS estimator rejects this null hypothesis at level 0•05, then, among the two secondary analyses only the E X analysis rejects the null hypothesis. This is because the corresponding 95% condence interval for the OLS estimator does not contain the hypothesized eect,the same is true for the E X condence interval, while the 95% condence interval for the 2SLS estimator contains the hypothesized eect. Such statistical decisions based on our proposed method for any such null hypothesis can be shown in a plot such as in Figure 4. This gure presents for each of the four outcome variables, the eect levels which we fail to reject, or reject only based on the primary OLS analysis, or reject with one piece of secondary evidence, or reject with two pieces of secondary evidence. As an illustration, let's consider risk preference in losses. We fail to reject any null hypothesis $_0$: = $_0$ with value of $_0$ between 0•019 and 0•058. A specied eect value between 0•033 and 0•019 is rejected only in primary OLS analysis. We have one piece of secondary evidence for the null hypothesis $_0$: = 0•12 and any $_0$ more than 0•152 or less than 0•067 is rejected with two pieces of secondary evidence.

5 Sensitivity Analyses

A sensitivity analysis asks the following question: if an analysis is based on a certain assumption, then what magnitude of violation in that assumption needs to be present to alter the conclusion based on such an analysis. If we know that the conclusion of the naive analysis that presumes a certain assumption is still valid when some amount of bias is allowed, then this knowledge will strengthen the conclusion. For discussion of various methods of sensitivity analysis for observational studies, see Corneld et al. (1959); Rosenbaum and Rubin (1983); Rosenbaum (1987); Manski and Nagin (1990); Yu and Gastwirth (2005); Shepherd et al. (2006); McCandless, Gustafson and Levy (2007); Hosman, Hansen and Holland (2010); Hsu and Small (2013); Liu, Kuramoto and Stuart (2013); and Ding and VanderWeele (2016). One crucial step of a sensitivity analysis is to determine a suitable parameter that can be used to quantify the deviation from the assumption. A sensitivity parameter should be such that, a larger value of the parameter indicates a bigger deviation from

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the assumption in some intuitive sense. Once such a sensitivity parameter, say X, is determined, a sensitivity analysis will report for a given value , if the conclusion of an analysis based on the assumption would remain unchanged if we allow for a deviation in magnitude of at most , i.e., if X .

In our discussion we are interested in the eect of treatment on the outcome H. Thus for a given value of the eect $_0$ we would conclude either we reject the null hypothesis $_0 :$ $=$ $_0$ or we fail to reject it. For simplicity we consider the case where we have only one variable of interest ($< = 1$). For ease of explanation we use 3 to denote an $= 1$ univariate treatment variable, rather than the matrix which allowed multiple treatments.

Let's consider the OLS analysis rst. The validity of the OLS analysis depends on the assumption that there are no unmeasured confounders. Denote the potential unmeasured confounder by F. Our sensitivity analysis method builds on a method proposed by Hosman, Hansen and Holland (2010). We use the parameter $X_1 = d_{F3-}$, which is the partial correlation between the unmeasured confounder and the treatment conditional on the covariates - . The parameter X_1 measures the magnitude of association of the unmeasured confounder F and the treatment 3 while we let the magnitude of association between the unmeasured confounder and the outcome be unrestricted. A larger value of jX_1j indicates a larger deviation from the assumption of no unmeasured confounding. When jX_1j is allowed to take value in a certain range, for a given signicance level U, we calculate an interval in which the „1 U"100% condence interval of based on its OLS estimator must be contained. Such an interval is called a sensitivity interval. If, $_0 :$ $=$ $_0$, allowing for unmeasured confounder with sensitivity parameter X_1, we would still be able to reject the null hypothesis if the sensitivity interval does not include $_0$.

A sensitivity interval when jX_1j can be calculated as

$$\wedge_{\$!(} \quad \stackrel{p}{2_{„}} \overline{= \quad A_- \quad 1-" \quad B4_{„}} \quad {}_\$!\hat{(}" -$$ (9)

where $A_- = A0=:_{„}-"$ and $2_{„};-1" = ;»;$, $\varsigma_{2U-;} \; _{„};$, $_{„}1 \; ^2 \quad 1"2"•_{„}; \quad 1"..._{„}1 \; ^2 \quad 1"_2$ with $C_{U-;}$ denoting the „1 U"-th quantile of a t-distribution with ; degrees of freedom. Using (9) we can determine the sensitivity of OLS analysis for dierent bounds on the partial correlation $X_1 = d_{F3-}$. The derivation of (9) and other sensitivity intervals presented below in this section are given in the

supplement.

Let's now consider the instrumental variable based 2SLS estimator. This estimator depends on the validity of the instruments, which is to say that assumptions A.1, A.2 and A.3 are true. Assumption A.1 which says that there is association between the instruments and the outcome, can be empirically validated. If the amount of association is very small, i.e., the instruments are weak, then conventional asymptotic results of the 2SLS estimator are misleading for even large nite samples (Bekker, 1994; Choi and Phillips, 1992; Staiger and Stock, 1997). Whether the strength of the instrumental variable is adequate for the asymptotic results to be reliable can be tested using the data and thus a sensitivity analysis is not needed for this assumption (Hahn and Hausman, 2002; Stock, Write and Yogo, 2002).

To see how the observed outcome behaves when assumptions A.2/A.3 are not enforced, we go back to our potential outcomes framework. Recall $H_\varepsilon^{"3-I"}$ is the potential outcome for unit 8, on receiving a treatment 3 and at value of the instrument I. The additive, linear constant-eect causal model for the potential outcomes is parallel to (3): $H_\varepsilon^{"3-I"} = H_8^{"0-I"}$, 3 • Let $^1_\varepsilon"I" = H_8^{"0-I"}$ $H_\varepsilon^{"0-0"}$, so that $^{8}_{\varepsilon}"$ measures the direct eect of the IV on the outcome; a nonzero value of $:^8_{"}$ violates A.2. As before let $3_8 := 3^{>1B}$ denote the observed level of the treatment for unit 8, $/_8 := I^{>1B}$ denote the observed instrument level, and $H_8 := H_\varepsilon^{"3_8-/_8"}$ denote the observed outcome. Now write

$$H_\varepsilon^{"0-0"} = {}_{"}H_8^{"0-0"} j /_8 = 0", \ f_{"}H_\varepsilon^{"0-0"} j /_8" \ {}_{"}H^{"0-0"}_\varepsilon j /_8 = 0"g, \ fH_\varepsilon^{"0-0"} \ {}_{"}H^{"0-0"} {}_{i} /_8"g$$

$$= {}_{"}H_\varepsilon^{"0-0"} j /_8 = 0", \ ^2_8{}_{"}/_8", \ D_8 •$$

In the above equation, $^{8}_{"}"I" := {}_{"}H_8^{"0-0"} j /_8 = I" \ {}_{"}H_8^{"0-0"} j /_8 = 0"$ is the eect of unmeasured confounders between the IV and outcome; a nonzero value of $:^8_{"}"I"$ violates A.3. We assume the error term $D_8 = H_\varepsilon^{"0-0"} \ {}_{"}H_\varepsilon^{"0-0"} j /_8"$ is independent of $/_8$. Finally, combining these, the observed outcome model for unit 8 is given by

$$H_8 = H_\varepsilon^{"3_8-/_8"} = {}_{"}H_\varepsilon^{"3_8-/_8"} \ H_\varepsilon^{"0-/_8""}, \ {}_{"}H_\varepsilon^{"0-/_8"} \ H_\varepsilon^{"0-0""}, {}_{\varepsilon}H^{"0-0"}$$

$$= {}_{"}H_\varepsilon^{"0-0"} j /_8 = 0", \ 3_8 \ , \ ^1_8{}_{"}/_8", \ ^2_8{}_{"}/_8", \ D_8 • \qquad (10)$$

Therefore, $^1_8{}_{"}/_8", \ ^2_8{}_{"}/_8"$ is the term that measures the violation of either of the two assumptions

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A.2 and A.3. We assume that $\wedge^{81}{}_{„}/{}_8"$, $\wedge^{2}{}_{„}/{}_8" = \wedge 6_{„}/{}_8"$, i.e., it is a linear function of $6_{„}/{}_8"$. The function $6_{„}/"$ which maps the instruments to a real number indicates the mechanism by which the IV assumptions A.2 and A.3 could be violated. In our study we make $6_{„}/"$ the function that calculates the distance of the community from the capital city Bujumbura. We provide further explanation on this choice of the function $6_{„}/"$ in Section 6. Also, see Section 7 for choice of $6_{„}/"$ for other studies and how they are calculated from the data. The larger the magnitude of $\wedge$, the more the assumptions are violated with $\wedge = 0$ corresponding to the assumptions holding (see Holland, 1988; Small, 2007).

Now we include our observed covariates in model (11), and in the matrix notation our model for the observed outcomes is

$$H = 3 \quad , \quad - \quad , \quad \wedge 6_{„}/" , \quad D \bullet \tag{11}$$

The 2SLS estimator, $\wedge^C_{2(!(}$, and its standard error under models (11) and (7) can be written as

$$\wedge^C_{2(!(} = \wedge_{2(!(} , \; B4_{„}\wedge_{2(!(}" \frac{\dfrac{p}{=}{-}A_{-} \; {}^{-}1 \, d_{6_{„}/"\wedge_{-}}^{3}}{q} \; d_{H6_{„}/" 3_{-}}$$

$$B4_{„}\wedge^C_{2(!(}" = B4_{„}\wedge_{2(!(}" \frac{p}{p} \frac{{=} \; A_{-} \; 1}{{=} \; A \; 2} \frac{q \frac{1 \; d_{6_{„}/" 3_{-}}^{2}}{\;}}{q \; 1 \; d_{H6_{„}/" 3_{-}}^{2}} \bullet$$

We calculate $d_{6_{„}/"3_{-}}$ from the data.

We consider $X_2 = d_{H6_{„}/" 3_{-}}$ as our sensitivity parameter for the 2SLS estimator. This sensitivity parameter X_2 can be seen as a scaled version of the coecient $\wedge$ in model (11) since we have the equality $\wedge \mathsf{f} \frac{p}{=}{-}A_{-} \; {}^{-}1 \, B4_{„}\wedge" g = X_2 \bullet \frac{q}{1} \; X_2^2$, where the right hand side ratio is an odd function which is increasing in X_2 on the positive axis. Therefore, X_2 is an appropriate measure of the amount of violation in A.2 and A.3. When we restrict the sensitivity parameter X_2 in the interval $\gg -\ldots$ for the 2SLS estimator (after some algebra) the sensitivity interval can be calculated, when $1 \bullet^2 \; 1$, $C_{U-=}{}^2_{A_{-}} \; {}_1 \bullet_{„} = \; A_{-} \; 2" d_{6_{„}/" 3_{-}\wedge}^2$ as

$$\wedge_{2(!(} \; 2 1_{„} d_{6_{„}/"}^{3} 3_{-\wedge} = \; A_{-} \; 1 -" B4_{„} \; 2(!(}"\wedge" - \tag{12}$$

where $2 1_{„} 0-;- 1" = \frac{p}{} ; \; 1 \, j 0 j \frac{p}{1 \; 0^2}, C_{U-}; \; ;_{„} 1 \frac{p}{1 \; 1 2}" {}_{„}; \frac{p}{1", 1 \; 0^2}".$ When $1 \bullet^2 \ddot{Y} 1, C_{U-=}{}^2_{A_{-}} \; {}_1 \bullet_{„} =$

A. $2''d^2_{6,,/''3}$ the sensitivity interval is

$$\hat{}_{2(!(}22_{,,}d_{6,,/''}{}^3_{-} \widehat{-} = A_{-} 1''B4_{,,} \hat{}_{2(!(''-} \tag{13}$$

where $22_{,,}0-;'' = \dfrac{\sqrt{\text{\rlap{\hspace{-2em}q}}\;\;\;\;\;\;\;\;\;\;\;\;\;\;}}{;_{,,}0^2, \hat{e}_{U-;\bullet,,;} 1''''}\dfrac{p}{1 0^2}.$

For given value of , we can determine if we can reject the null hypothesis by checking whether $_0$ falls outside the interval (12) or (13), whichever one is appropriate.

Finally, E X analysis depends on the assumptions that there are no unmeasured confounders and assumption A.1 that the instrument is associated with the treatment. We only consider violation in the assumption of no unmeasured confounding since A.1 can be empirically validated as explained above. To measure the violation we use the parameter $X_1 = d_{F3-}$ as in the OLS analysis. We further assume that there is no unmeasured confounding between the instrument and the outcome. Then, the sensitivity interval for the E X estimator when we have the range jX_1j is

$$\hat{}_{-} 2_{,,}= A_{-/} 1-_{,,}1 \sqrt{d^2_{3/-}{}^{''1\bullet2''}B4_{,,}\;\;\;} {}_{-}{}^{\hspace{-0.3em}\nwarrow}{}^{-} \tag{14}$$

where $A_{-/} = A0=:_{,,}\gg- : /...''$. Recall, $2_{,,};-1''$ is dened just after (9). In calculating the sensitivity interval we calculate $d_{3/-}$ from the data.

The sensitivity parameters for the OLS and 2SLS estimators are X_1 and X_2 respectively. X_1 measures the potential violation in the assumption of no unmeasured confounders of the treatment-outcome relationship. When there is no confounding between the outcome and the instruments we get the sensitivity analysis for E X estimator at no extra cost using X_1. In that case we consider potential violation in instruments only through a violation in assumption A.2 but not A.3. In the sensitivity analysis for the null hypothesis $_0 : = _0$, for given pair $_{,,}X_1-X_2''$ there are four possible decisions: primary OLS analysis shows no statistical signicance, primary analysis rejects the null but neither of the two secondary analyses shows signicance, OLS rejects and only one of the two secondary analyses shows signicance, or OLS and both the secondary analyses show signicance. We will present the sensitivity analysis result based on the primary OLS estimator and secondary 2SLS and E X estimators using a gray-scale plot depicting these four decisions as in Figure 5 that will be discussed in the next section.

6   Results: Sensitivity Analysis in the Burundi Data

We now go back to the empirical study of the Burundi civil war and its eect on preferences of people exposed to violent conict. A potential unmeasured confounder of the treatment-outcome relationship is the number of ethnic or political cleavages in the communities. A larger number of political cleavages may leave the community more vulnerable to violent attacks. Also, the cleavages may be homogeneous in social preferences so that the number of political cleavages will also be associated with social preferences. If this theory is true, not having information on cleavages can lead to OLS analysis signaling a statistically signicant eect when there is no causal eect.

A potential concern about the validity of the instruments is that distance to the capital could have a direct eect on preferences thus violating assumption A.2 if distance to capital is a proxy for distance to markets (see Henrich et al., 2001).

In Figure 5, we present the result of sensitivity analyses for the four outcome variables. We also consider dierent sizes of eects for each outcome. Results of Table 3 which correspond to $X_1 = X_2 = 0$ and eect size = 0, can be seen in the plots as well. For example, we have two pieces of secondary evidence for a non-zero eect of conict on altruistic behavior. We lose evidence from the 2SLS analysis for sensitivity parameters $|X_2| \geq 0 \bullet 07$, and the evidence from EX estimator is sensitive for $|X_1| \geq 0 \bullet 05$. For an eect size of $0 \bullet 1$ there is no evidence from the EX analysis at any level and the 2SLS analysis is sensitive for $|X_2| \geq 0 \bullet 06$.

As noted in Section 2, for the two instrumental variables in the data, distance of the communities from the nation's capital and altitude, the rst variable is the rst principal component explaining more than 95% of the total variation of the two variables. In the sensitivity interval calculations for the 2SLS estimator we use distance from Bujumbura for the mechanism by which the instruments can be invalid, i.e., $6_{,,}/'' =$ distance from Bujumbura. This choice of $6_{,,}/''$ is also reasonable in view of our earlier discussion that validity of the instrument may be violated because distance to capital is a proxy for distance to markets.

Even though exposure to violence showed a non-zero eect on risk preferences in gains from EX analysis, this analysis is sensitive for $|X_1| \geq 0 \bullet 065$. For a hypothesized negative eect size of

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0•01, 2SLS does not show any evidence (this would have been seen as a horizontal strip). For risk preferences in losses with a hypothesized eect size of 0•01 we do get statistical signicance from both secondary analyses when deviation to the assumption is in the range $|X_1|$ 0•11 and $|X_2|$ 0•06 (the rectangular area with darkest shade). Diering results for gains and losses is not surprising since it has been noted that people have dierent perceptions towards gains and losses (Kahneman, Knetsch and Thaler, 1991).

When considering the eect on time preference, although 2SLS analysis does show a signicant eect, it is sensitive even for a small deviation of $X_2 = 0$•005. Thus a conclusion that exposure to conict can result in less patience cannot be made with condence.

In summary, two secondary analyses along with their sensitivity analyses add considerable information about the evidence provided by the study for the theory that exposure to violent conict aects preferences.

7 Discussion

We have found two independent sources of evidence that in Burundi, exposure to violent conict increased a community's level of altruistic behavior and risk taking behavior. We have considered a two step analysis procedure to arrive at this decision where the rst step, the primary analysis, uses a standard regression analysis and a secondary analysis uses two evidence factors constructed from instrumental variables. While this two step procedure also nds two sources of evidence for conict increasing impatience (time preference) assuming that the instrumental variables of distance to the capital and altitude are strictly valid, this evidence can be easily challenged on the ground that the instruments considered are not strictly valid.

For the three estimators that are involved in our analysis { OLS, 2SLS and E X { we chose sensitivity analysis methods with sensitivity parameters intended to intuitively measure the deviation from the assumption of concern. In a sensitivity analysis of the least squares regression estimator to potential violation of the no unmeasured confounding assumption, we used the correlation of the unmeasured confounder to the treatment. The same parameter is used for a sensitivity analysis of the E X estimator as well. The sensitivity analyses for the three estimators can be conveniently presented in a gray-scale plot as in Figure 5. The sensitivity analysis of the 2SLS estimator requires

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additional information on the mechanism by which the instruments may be invalid. This mechanism is encoded through the function 6 of the instrumental variables. The choice of the function 6 is contextual and needs subject matter consideration. For example, in a study of the eect of imprisonment on earnings Ertefaie et al. (2017) uses judges' ID as the instrument where a violation to the validity of the instrument can be due to the fact that characteristics of the pool of convicted felons may vary as a function of judge's harshness. In this case 6 can be taken as a function of judges' ID that measures their harshness. Ertefaie et al. use this function in a sensitivity analysis based on the Anderson-Rubin statistic (Anderson and Rubin, 1949).

The methods and the visualizations of the paper are coded in the R package ivregEX available at CRAN. The Burundi data set analyzed in this paper is available as a Web supplement to the paper by Voors et al. (2012).

Supplement

An online supplement PDF document is available with the details on bias calculations of the proposed estimator, proof of Theorem 2 and the derivations of the sensitivity intervals discussed in Section 5. The supplement the extends the proposed method for more general non-linear models for instrumental variables analysis.

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Table 1: Correlation of $\hat{\beta}_{1(\cdot)}$ and $\hat{\beta}_{2(\cdot)}$ for varied values of V and W in the models (1) and (2), based on a sample of size $= = 100$; $D_8 - E_8$ #„$0 - 0 \bullet 5^2$"; no G variable; and I_8 D=85»$0 - 1$…. Estimated by averaging over 1000 repetitions.

W! V #	-5	-2	-1	-0.5	0	0.5	1	2	5
-5	0.99	0.91	0.75	0.46	0.07	0.52	0.75	0.91	0.98
-2	0.98	0.92	0.77	0.52	0.05	0.47	0.75	0.91	0.98
-1	0.99	0.91	0.74	0.48	0.02	0.47	0.75	0.91	0.98
-0.5	0.98	0.91	0.74	0.42	-0.05	0.48	0.74	0.92	0.99
0	0.98	0.91	0.75	0.47	-0.01	0.44	0.75	0.91	0.99
0.5	0.98	0.92	0.74	0.43	0.02	0.49	0.74	0.91	0.99
1	0.98	0.91	0.75	0.5	0.02	0.43	0.73	0.92	0.99
2	0.99	0.91	0.73	0.5	0.06	0.48	0.75	0.92	0.98
5	0.98	0.92	0.74	0.48	-0.01	0.5	0.75	0.92	0.99

Table 2: Summary statistics of the variables

	Observations household level	Mean	Standard Deviation	Min	Max
Outcome variables (H)					
Degree of altruism	286	27.32	27.22	0.00	100.00
Risk preferences Gains	220	1.87	1.31	0.00	3.00
Risk preferences Losses	233	2.31	1.18	0.00	3.00
Discount rate	273	40.16	41.43	0.00	100.00
Variable of interest ()					
Relative number of dead in attacks 1993 { 2003	35	2.43	4.55	0.00	15.63
Control variables (-)					
Household level variables					
Household head is literate	285	0.51	0.50	0.00	1.00
Household head age	286	45.96	15.11	18.00	90.00
Household head is male	286	0.62	0.49	0.00	1.00
Household head ethnic origin	281	5.54	5.13	0.10	34.25
Total land holdings per capita (ha^2)	287	0.22	0.41	0.00	1.00
Community level variables					
Land Gini coecients	35	0.29	0.20	0.00	0.54
Distance to market	35	2.86	0.72	1.38	4.22
Conict over land (% yes, 2007)	35	0.25	0.15	0.00	0.60
Ethnic homogeneity	35	87.66	14.59	30.00	100.00
Population density (log, 2008)	35	5.78	0.43	4.50	6.49
Per capita total expenditure (log, 2007)	35	9.24	0.48	8.09	10.40
Instrumental variables (/)					
Distance to Bujumbura (km, log)	35	4.47	0.39	3.68	5.12
Altitude (m, log)	35	7.41	0.09	7.14	7.70

Table 3: Conict and Preferences

| | OLS | 2SLS | E X |
	(1)	(2)	(3)
Degree of altruism	1.6858 [0.5029]***	2.8912 [0.9785]***	1.1435 [0.5377]**
Risk preferences Gains	0.0634 [0.0250]**	0.0729 [0.0403]*	0.0601 [0.0252]**
Risk preferences Losses	0.0196 [0.0191]	-	-
Time preference	1.1875 [0.5536]**	2.3366 [1.1153]**	0.6735 [0.8077]

Notes: Variable of interest: percentage dead in attacks (1993{2003). Only the estimate of the coecient of the variable of interest and the corresponding standard errors clustered at the community level are reported. For a list of all the other covariates see Table 2.

*** Signicant at the 1 percent level; ** Signicant at the 5 percent level; * Signicant at the 10 percent level.

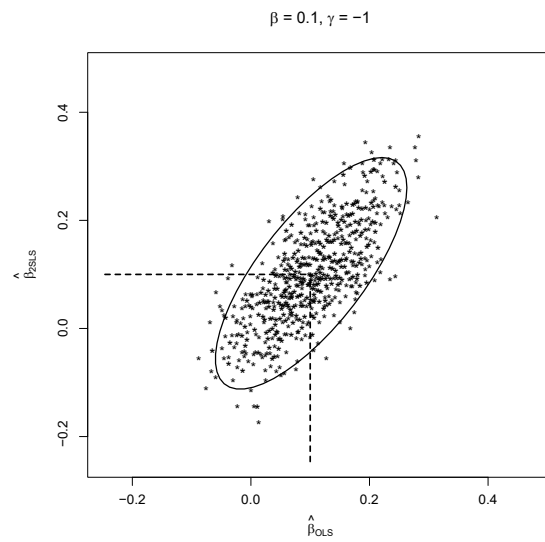Figure 1: Scatter plot of estimates $\hat{\beta}_{S!(}$, $\hat{\beta}_{2(!(}$ and $\hat{\beta}_-$ for the models (1) and (2), with sample size = = 100; $D_8-E_8^{883}\#„0-0•5^2"$; no G variable; and $I_8^{883}=85»0–1..$ based on 1000 repetitions.

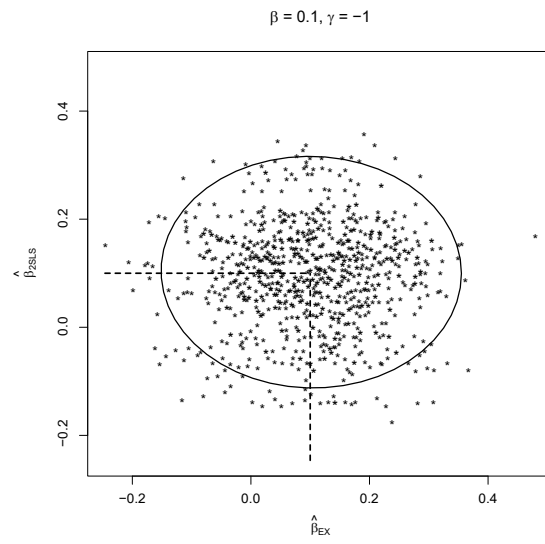Figure 2: Scatter plot of estimates $\hat{\beta}_{2SLS}$ and the proposed estimator $\hat{\beta}$ for the models (1) and (2), with the same data as in Figure 1.
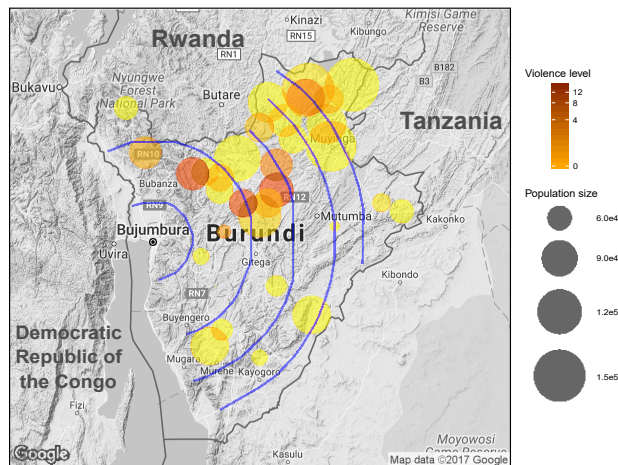
Figure 3: Geography of Burundi. The circles correspond to the communities surveyed. Color gradient is used to show violence level during the civil war period in the communities. Distance contours from the capital of the country, Bujumbura, are shown in dashed lines.
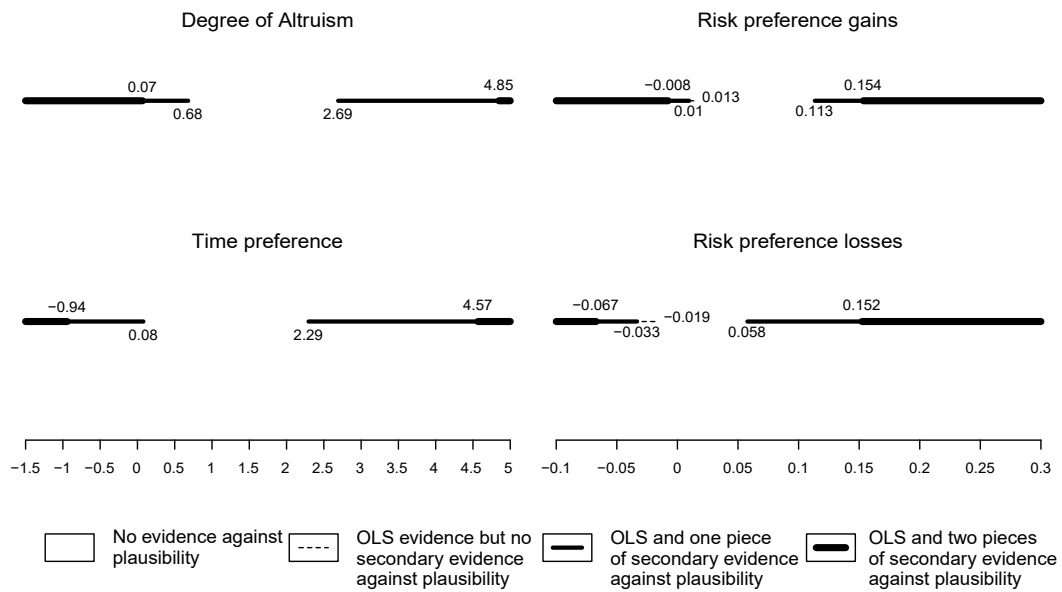
Figure 4: Dierent condence regions for the eect of violent conict on the four outcome variables of the Burundi data at 95% level.
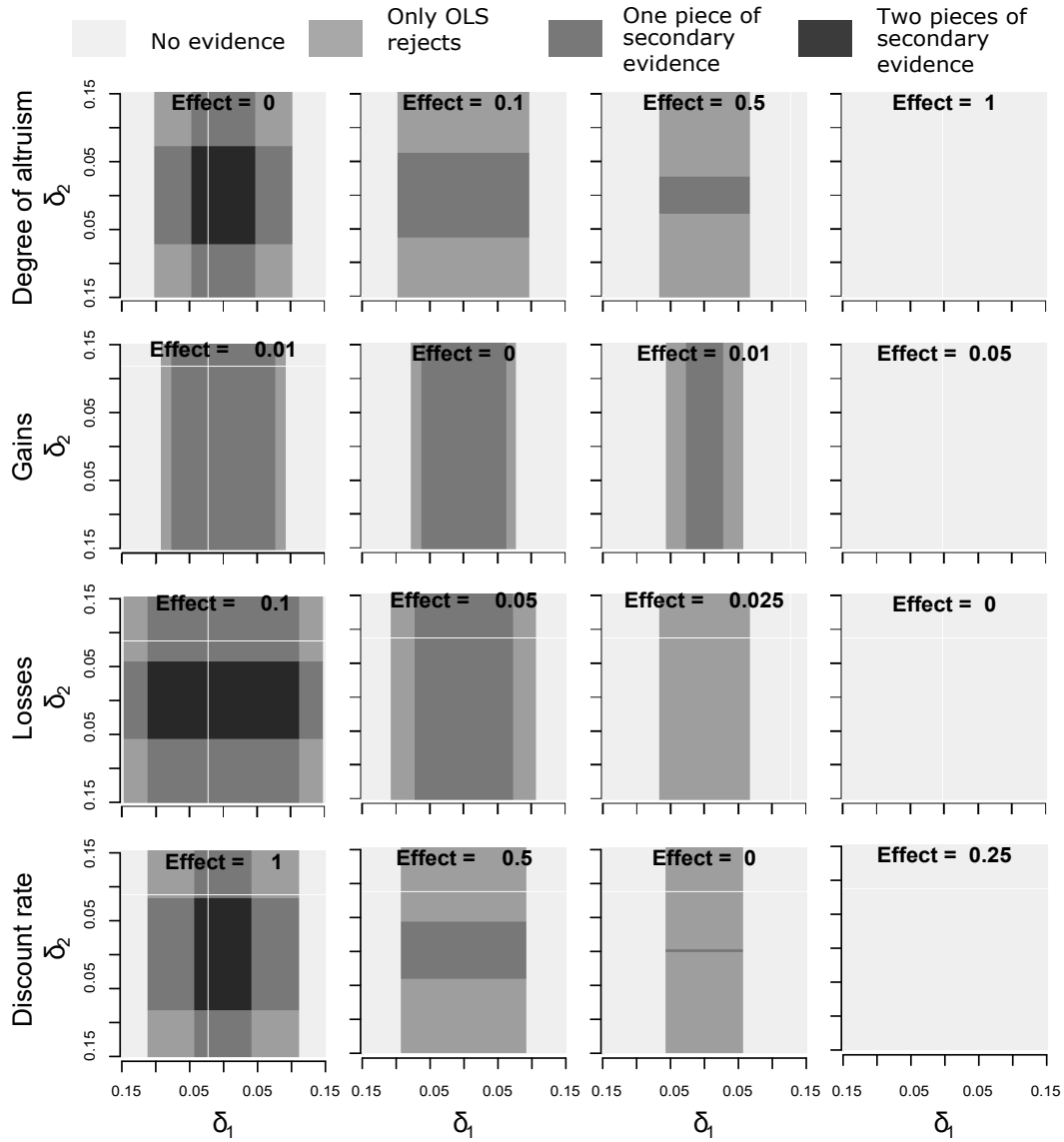
Figure 5: Sensitivity analysis of the four outcome variables for dierent amount of treatment eects using OLS as primary and 2SLS and EX as secondary analysis. The decision is indicated using four dierent shades of gray.