Credible or Confounded?

What we do (not) know about who supports peace with the FARC

Chad Hazlett, Francesca Parente

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Abstract

In October 2016, Colombian voters narrowly defeated a peace accord intended to end over 50 years of conflict with the FARC. The vote was highly polarized, with support ranging from 19% to 96% among towns with at least 1000 votes. One widely proposed explanation for how people voted is variation in exposure to violence, with the most supportive areas tending to be those that faced FARC violence. A second possible explanation is "political affiliation", with supporters of President Santos overwhelmingly favoring the deal. Showing that either factor causally influenced support for the deal is extremely difficult, since both political affiliation and exposure to violence are far from random. In this paper, we examine what can be learned despite these challenges through sensitivity analyses that reveal what types of confounders would substantively alter our conclusions regarding the effects of these two factors. We find that the observed evidence is consistent with both arguments, in terms of point estimates and their substantive and statistical significance. However, the estimated effect of violence can be easily over-turned by even very weak confounders. By contrast, the estimated effect of political affiliation, measured by municipal voteshare for Santos in 2014, would require extremely strong confounding to overturn. While "perfect identification" is unlikely to be convincingly achieved in this circumstance, these results provide a very useful indication of how confident we can be in the results of regression estimates under potential confounding, and discipline the debate about the role of confounders in these estimates.

1 Introduction

On October 2, 2016, Colombian voters were asked to ratify a peace accord with the FARC intended to end over 50 years of conflict. It was narrowly defeated, with 50.2% of voters saying "no". The vote was highly polarized, with even department-level support as low as 29% and as high as 80%. Among towns with at least 1000 votes, support ranged from 19% to 96%. What explains such enormous variation in acceptance of the accord?

There are undoubtedly many reasons why citizens voted the way they did. One explanation, widely hypothesized in press coverage, is exposure to FARC violence. On the one hand, suffering violence at the hands of FARC may engender a desire for justice or vengeance, neither of which are well served by the peace accord. Alternatively, exposure to violence may create a stronger willingness to end the conflict, even if it means making concessions on justice and granting financial and political benefits to FARC, which the proposed deal did. Indeed, both of these claims have been made in the press. The day after the vote, a BBC report[1] noted that "In Choco, one of the provinces hardest hit by the conflict, 80% of voters backed the deal... In the town of Bojaya, where at least 119 people were killed when a church was hit by FARC mortar bombs, 96% of residents voted 'yes'." By contrast, some areas known to have suffered under FARC rejected the accord. For example, "In ... Casanare... 71.1% voted against the deal. It is an area where farmers and landowners have for years been extorted by the FARC and other illegal groups."

A less common, but also plausible, explanation is that support for the deal does not reflect citizens' preferences or experiences, but is the consequence of following one's political leadership. In this case, the political significance of the deal and its connection to specific political elites and parties was widely known and discussed. The referendum pitted current president Juan Manuel Santos, who had brokered the deal, against former president Álvaro Uribe. President Santos had been Minister of National Defense in the Uribe administration and was perceived as Uribe's natural successor in the 2010 presidential election, winning the election partly on Uribe's coattails.[2] However, after Santos took office, he broke from Uribe's hard-line policy regarding

[1] "Colombia referendum: Voters reject FARC peace deal," 3 October 2016, *BCC News.* Available online at `http://www.bbc.com/news/world-latin-america-37537252`

[2] Adriaan Alsema, "Peace or no peace, Colombia disapproves of its president, a lot," 14 February 2017, *Colombia Reports.* Available online at `https://colombiareports.com/peace-no-peace-colombia-disapproves-president-lot/`

the FARC and sought to instead negotiate for peace. Uribe denounced the talks — and Santos — as negotiating with terrorists.[3] The referendum for peace was thus highly politicized, with well-known elite supporters (Santos) and denouncers (Uribe).

Beyond these qualitative claims, can we empirically determine whether "violence" and/or "political affiliation" indeed influenced votes for the peace deal? Observational studies provide evidence consistent with both accounts – as reviewed below, Tellez (2018) finds that those living in areas exposed to more conflict are more supportive of the deal, and Matanock and Garcia-Sánchez (2017) find a relationship between which elites an individual supports and their level of support for the peace deal. Yet, convincingly determining the causal effect of these factors on how people voted poses serious methodological challenges, as neither exposure to violence nor political affiliation are randomly assigned. Individuals who differ on these characteristics are likely to differ on many other characteristics, confounding comparisons. Further, these differences may occur in unobserved characteristics, and thus cannot be adjusted for in our models. For example, suppose some areas are more sympathetic to the FARC in some way that is unmeasured. Suppose further that in these areas: (1) the FARC commits little violence, and (2) people agree with the deal, which is seen as favorable to the FARC. In this case, "sympathy for the FARC" is an unobserved confounder that can falsely generate an observed relationship between violence and support for the deal. Such simple confounding stories can easily be constructed in either direction, and for a large set of possible unmeasured confounders.

Given the seemingly intractable problem posed by such confounders, how can we proceed? Rather than argue that the estimates from a given model are definitively free of confounding bias, we show the degree of confounding that would substantively alter estimates from imperfect models, and discuss whether such confounding is likely, or the assumptions required to rule out confounders of these magnitudes. We then explore assumptions that can be used to bound the degree of confounding and produce informative estimates under those assumptions. In doing so, we describe what we must be prepared to believe to either sustain a causal claim, or to argue that an estimate is due to confounding. This replaces debate about whether an effect estimate is "well identified" with a precise and quantitative account of what would alter our conclusions.

We find that the observed evidence is consistent with both claims: exposure to violence and political affiliation (with Santos) increase support for peace. The results for exposure to

[3] "Santos v Uribe," 7 April 2012, *The Economist.*

violence, in at least one model, suggest a powerful effect in terms of the point estimate, with one additional death in a municipality between 2011 and 2015 associated with 0.6 percentage points more support for the FARC deal in that municipality. However, this estimate is extremely fragile in the face of unobserved confounding: even in the more favorable model we test, confounders explaining just 6% of the residual variance of violence and support for peace would be enough to reduce the effect to zero. A confounder explaining a mere 0.4% of residual variance in violence and in support for peace would reduce the effect below conventional statistical significance at the 5% level. Such confounding may or may not occur, but is certainly difficult to rule out.

By contrast, the estimate for political affiliation would require far more powerful confounding to undermine. For example, even a confounder explaining 100% of the residual variation of support for peace would need to explain 59% of the residual variation in political affiliation.

Retuning to the bounding approach, we regard GDP per capita as a potentially powerful factor in determining both political affiliation and support for the peace deal. Let us assume that GDP per capita is "worse than" confounding, i.e. GDP per capita explains a larger share of the residual variance of political affiliation and of support for peace than does confounding. Under such an assumption, the resulting level or permissible confounding would barely change the estimate. The same holds even if confounding is three times worse than GDP per capita. This invites readers to shift from general critiques about the possibility of confounding to instead attempt to name one or more confounders that could meet such criteria.

In Section 2, we provide background theory and literature regarding the potential role for violence and political affiliation in support for the FARC peace deal. We also describe the challenges of identification in greater detail, and introduce the sensitivity analysis approach that we follow. In *Results* (Section 3), we provide the estimates and sensitivity analyses forming the core of our analyses. Section 4 concludes.

2 Background

2.1 Violence and political affiliation as potential causes

As widely claimed by popular media after the referendum, statistically speaking it is the case that municipalities exposed to more violence were also more likely to vote in favor of the deal.

Tellez (2018) finds that citizens in municipalities the government labels "conflict zones" were more likely to report that they supported the peace process and concessions to FARC in AmericasBarometer surveys. Such an effect of violence on attitudes toward peace specifically has been documented in quasi-experimental work on indiscriminate violence in Darfur (Hazlett, 2018) and Syria (Fabbe, Hazlett and Sinmazdemir, 2018). Further, also relying largely on quasi-experimental evidence, a growing literature considers the effects of violence on a range of attitudes that are potentially related to peace, such as pro-sociality and cooperation (see Bauer et al., 2016, for a recent review). On the other hand—and perhaps more intuitively—exposure to violence may make citizens *less* likely to support peace, especially when it comes at the expense of justice. Petersen and Daly (2010*a*,*b*) stress the role of anger and emotions in determining attitudes toward peace, with exposure to violence making victims less likely to support reconciliation. Alternatively, more complicated relationships may exist: Weintraub, Vargas and Flores (2015) find a non-monotonic relationship between exposure to violence in the Colombian conflict and support for President Santos in the 2014 election, which was centered around the negotiations with FARC. They find that municipalities with low violence *and* high violence were less likely to support Santos, while those with moderate levels of violence were more likely to support him.

The second potential explanation we consider is that votes for peace simply follow political affiliation. While a less ubiquitous explanation than violence, this account has also been made in journalistic reports.[4] Such an explanation flows directly from a vast political science literature on the power of elites in directing voting behavior, especially in low-information environments (Zaller, 1992), as supported by numerous survey experiments showing that elite endorsements influence opinion on policies (e.g. Lupia, 1994; Levendusky, 2010; Nicholson, 2012; Druckman, Peterson and Slothuus, 2013; Guisinger and Saunders, 2017). In the Colombian context, we have just such a low-information environment combined with elite dissensus: President Santos very publicly campaigned for the deal, while his predecessor President Uribe publicly opposed it. As one Colombian commentator put it, some citizens were hesitant to vote for the deal because

[4] See, e.g. *Washington Post*, "Colombians vote against historical peace agreement with FARC rebels" (`https://www.washingtonpost.com/world/colombians-vote-on-historic-peace-agreement-with-farc-rebels/2016/10/02/8ef1a2a2-84b4-11e6-b57d-dd49277af02f_story.html?utm_term=.a538816c55e9`) and *Reuters*, "Colombia's peace deal in limbo after shock referendum" (`https://www.reuters.com/article/us-colombia-peace-idUSKCN1230BH?`).

they could not vote "without feeling they were being invited to support Santos."[5] The political loyalties attached to voting for or against the referendum appear to have been well understood and openly discussed. Recent academic work has also shown the plausibility of this explanation. Matanock and Garcia-Sánchez (2017) find observational evidence that support for particular political elites is correlated with support for the peace deal. In addition, experimental evidence for this mechanism exists in the Colombian context: Matanock, Garbiras-Díaz and García-Sánchez (2018) provide evidence from a survey experiment that manipulates elite endorsements. They conclude that "elite cues matter in shaping citizens attitudes towards the two provisions [of the peace deal] that we asked about", and that "that the direction of this effect is contingent on individuals affinity with controversial elites" (pg. 2). We are thus interested in pursuing the "political affiliation" explanation in addition to the "exposure to violence" explanation, and in both cases, determining how sensitive the observational results would be the face of possible unobserved confounding.

2.2 Identification and Sensitivity

Both exposure to violence and political affiliation are far from random in how they are "assigned". Confounding thus poses a major challenge to credibly estimating the causal effects of either factor on support for the FARC peace deal. At times, opportunities arise to overcome confounding concerns, such as when convincing natural experiments can be found. We do not find such an opportunity here. Moreover, finding natural experiments on "political affiliation" is far more difficult since it is harder to imagine cases where these affiliations are "indiscriminate" or haphazard, as may be possible for violence. Thus, arguments purporting to rule out confounders entirely are unlikely to be convincing, at least with the research designs we employ herein.

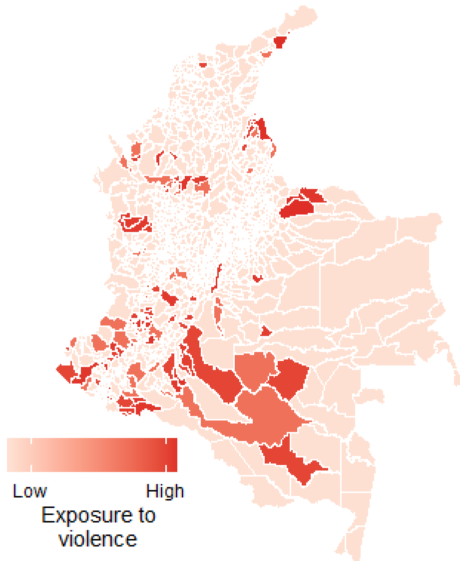
Instead of simply abandoning this research question or ignoring the problem of confounding, our strategy is to characterize what we know, and the consequences that confounding could have on our estimates. That is, rather than claiming zero bias, we provide estimates of the effect, then determine the degree of confounding that would be required to alter our conclusions. If even confounders far too small to be convincingly ruled out can drastically alter our estimate, we

[5] Isabel Hilton, "Why Colombians voted against peace with the FARC," 3 October 2016, *The Guardian*. Available online at `https://www.theguardian.com/commentisfree/2016/oct/03/why-colombians-voted-against-peace-farc-president-santos-better-deal`

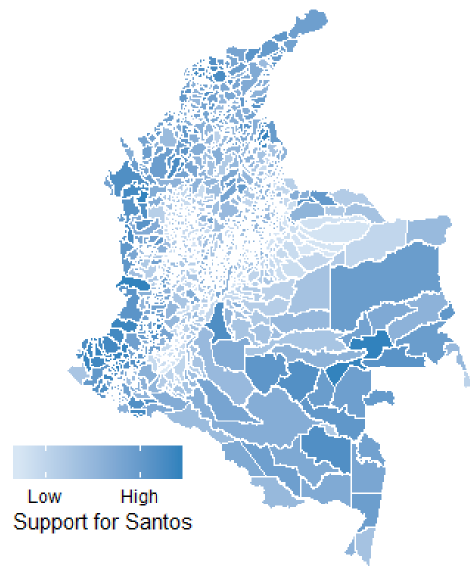
have little confidence in that particular estimate or model. Alternatively, we may find that the type of confounding required to alter our conclusions is large enough to be potentially ruled out. Our expert knowledge can be employed to aid in judging the plausibility of such confounders, as illustrated here.[6]

To briefly describe the sensitivity analyses, we follow the recommendation of Cinelli and Hazlett (2018) in reporting augmented regression tables and contour plots to characterize and visualize "how bad" confounding would need to be to overturn our estimates. There are three measures that help quantify the sensitivity of the estimates to unobserved confounding. The first is the partial R^2 of the treatment with the outcome, having accounted for control variables, written as $R^2_{Y\sim D|\mathbf{X}}$. This quantity can most concretely be interpreted as the result of an "extreme scenario" analysis: if we assume that confounders explain 100% of the residual variance of the outcome, the $R^2_{Y\sim D|\mathbf{X}}$ tells us how much of the residual variance in the treatment such confounders would need to explain to bring the estimated effect down to zero. The second quantity we add to regression tables is the *robustness value* (RV) for the estimate. This quantity summarizes sensitivity to confounders by describing the fraction of the residual variance of the treatment *and* the outcome that unobserved confounders would need to explain in order to explain away the estimated treatment effect. Closely related is the $RV_{\alpha=0.05}$, which tells us what percentage of the residual variance of the treatment and the outcome unobserved confounders would need to explain in order for the estimated treatment effect to no longer be statistically significant at the $\alpha = 0.05$ level. In addition to these simple numerical summaries of sensitivity, we provide a number of plots that more fully characterize how our estimates would vary under different hypothesized levels of confounding. Finally, to improve upon our ability to argue that a given degree of confounding is plausible or not, we describe how assumptions about whether a confounder is "stronger" than unobserved confounding can be translated into bounds on the permissible level of confounding. This provides a clear statement of assumptions that, if believed, would rule out problematic levels of confounding, as we do here.

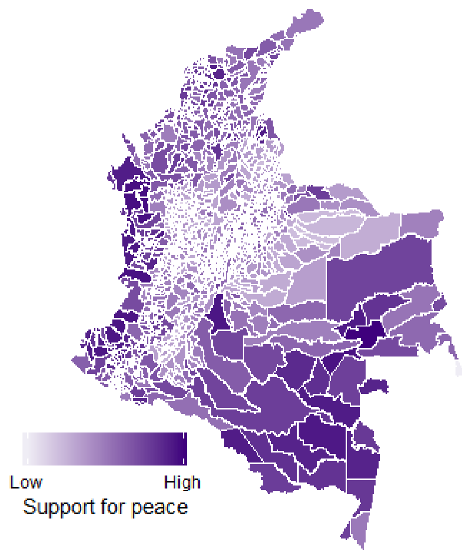
Figure 1: Spatial Distribution of Key Variables



(A) Exposure to violence



(B) Santos vote



(C) FARC referendum

Note: Maps visualizing the municipal-level distribution of: (A) FARC-caused deaths, (B) vote share for Santos in the 2014 election, and (C) vote share in support of the FARC peace deal in the 2016 referendum.

3 Results

We begin by examining the data visually, which provides insights that are borne out in the sensitivity analyses below. Figure 1 illustrates the distribution of exposure to violence (Panel A), votes for President Santos in the 2014 election (Panel B), and votes in favor of the referendum

[6] Note that such "sensitivity to unobserved confounding" is an entirely separate consideration than the size of the coefficient, p-values, t-statistics, or other statistics that investigators may informally look to for evidence of robustness.

for peace (Panel C) by municipality.[7] Several municipalities in the southwestern part of the country (in the departments of Nariño and Cauca) were high on all three measures. On the other hand, departments in the Andes Mountains are low on all three measures. Taken as a whole, exposure to violence (Panel A) seems to be somewhat similar in distribution to support for the FARC referendum (Panel C). The relationship between support for Santos (Panel B) and for the FARC referendum (Panel C) appears to be stronger still.[8]

3.1 Evidence for the effect of violence

The goal of our analyses is to produce basic observational estimates from models that are easy to understand, so that we can then examine the sensitivity of these results. In addressing exposure to violence as an explanation for support, we consider two models. The first is a naive, direct comparison based on the simple model:

$$\text{Model 1:} \quad y_i = \beta_0 + \alpha(deaths_{i,2011-2015}) + \epsilon_i \tag{1}$$

where y_i is the proportion voting "Yes" in municipality i, and $deaths_{i,2011-2015}$ is the number of deaths in municipality i committed by the FARC between 2011 and 2015.[9] The coefficient α then describes how the expected support for peace differs when we observe one additional death.

The second model seeks to control for potentially worrying observed confounders by including them in the model, a more conventional in political science and other social science disciplines. We estimate the model

$$\begin{aligned}\text{Model 2:} \quad y_i = \beta_0 + \alpha(deaths_{i,2011-2015}) + \beta_1(deaths_{i,2006-2010}) + \beta_2(deaths_{i,2001-2005}) + \\ \beta_3(population_i) + \beta_4(GDPpc_i) + \beta_5(Santos2010_i) + \epsilon_i\end{aligned} \tag{2}$$

where $deaths_{i,2006-2010}$ and $deaths_{i,2001-2005}$ are the number of deaths in municipality i in the

[7] Maps were generated using the 'colmaps' package. See `https://github.com/nebulae-co/colmaps`

[8] In the models below, this relationship appears numerically, as a very high partial R^2 of Santos support in explaining support for peace (nearly 60% in the models below). A high partial R^2 tells us much about the robustness of the result in the face of unobserved confounders (Cinelli and Hazlett, 2018).

[9] Estimates of deaths from FARC violence come from the Global Terrorism Database.

corresponding time periods, $population_i$ is the total number of eligible voters, $GDPpc_i$ is GDP per capita, and $Santos2010_i$ is the voteshare for President Santos in the 2010 election.[10]

The results for Model 1 suggest a marginally significant relationship between exposure to violence and support for peace: the coefficient of 0.20 (p=0.06) on violence in 2011-2015 suggests that each additional observed death increases the expected support for the FARC peace deal by 0.20 percentage points. For Model 2, we plot the coefficients on violence in the three time periods in Figure 2. This shows that violence in the most recent period is apparently associated with higher support for peace: each additional death observed predicts a 0.6 percentage point higher level of support for the peace deal ($p = 0.035$).

3.1.1 Sensitivity analysis for effect of violence

Table 1: Augmented regression results for violence

Outcome: *Vote for peace deal*

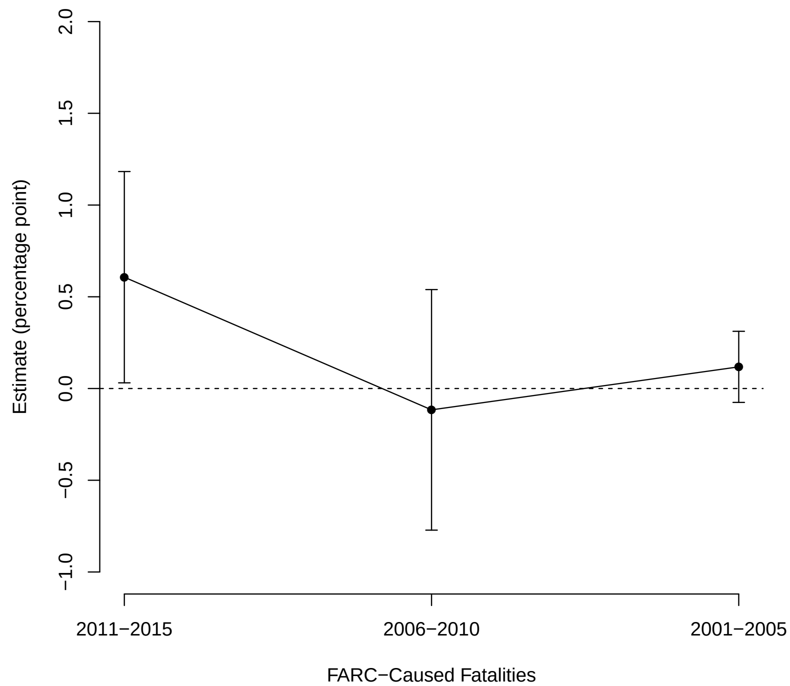
Treatment:	Est.	SE	t-stat	$R^2_{Y\sim D\mid\mathbf{X}}$	RV	$RV_{\alpha=0.05}$
Model 1: Deaths 2011-2015	0.20	0.11	1.89	0.32%	5.5%	0.0%
Model 2: Deaths 2011-2015	0.61	0.29	2.11	0.40%	6.1%	0.4%

Note: Augmented regression table for models relating exposure to violence in 2011-2015 to support for the peace deal. Traditional statistics such as the coeficients estimate (*Est.*) and standard error (*SE*) are shown, with the addition of key sensitivity statistics, $R^2_{Y\sim D\mid\mathbf{X}}$, RV, and $RV_{\alpha=0.05}$. Descriptions of each quantity are given in Section 2.2.

Turning to the sensitivity of this estimate, we employ several tools to characterize the types of confounding that would problematically alter our results. First, consider the augmented regression table in Table 1. This table shows that in both models, the estimated coefficients would change radically in the presence of even small confounders. Beginning with Model 1, which had no covariates, the observed relationship between deaths and support for peace was already only marginally significant at $p = 0.06$. The robustness value (RV) tells us that a confounder explaining just 5.5% of the residual variance in violence *and* in support for peace

[10]For simplicity, we focus on violence in the 2011-2015 period as more recent violence more plausibly impacts attitudes. GDP per capita is unfortunately measured in 2013; we recognize this is post-treatment relative to violence occurring in 2011 and 2012; however, our assumption is that the effect of additional deaths at this level on GDP per capita is too small to be problematic. Finally, we note that an additional pre-treatment characteristic we could include in this model is $elevation_i$, as being in mountainous areas may be an important confounder. Including it entirely removes the estimated effect of violence here, which we avoid to allow us to mimic the results of other work (such as Tellez, 2018). The fact that including $elevation_i$ drastically alters these estimates corroborates our main argument and result of the sensitivity analysis: findings regarding the role of violence are extremely delicate and could have been easily reversed had other variables been included.

Figure 2: Effect of deaths and attacks on support for peace



Note: Coefficients from Model 2, showing estimated relationship between violence (FARC-caused deaths) at each of three lags with support for the FARC peace deal. Recent violence (in 2011 to 2015) is associated with higher support, conditionally on violence in earlier periods and controlling for population, GDP per capita, and support for Santos in the 2010 election.

would be enough to eliminate this effect entirely (i.e. if such a confounder existed, the observed result would be due entirely to bias).[11] The $RV_{\alpha=0.05}$ tells us what strength of confounding would be required to reduce the estimated effect not to zero, but to the boundary of statistical significance at the $\alpha = 0.05$ level. Here, no confounder is required to do this since the p-value is already above 0.05. Finally, the value of $R^2_{Y\sim D|\mathbf{X}}$ is useful in part because it provides an "extreme-scenario" analysis: in the unfortunate case that unobserved confounding explains 100% of the remaining outcome variation, such a confounder would have to explain only 0.32% of the residual variation in violence in order to reduce the estimated effect to zero.

Turning to Model 2, Table 1 provides only slightly more promising news. The effect estimate is now slightly larger (0.61) and more statistically significant in conventional terms with a t-statistic of 2.11. However, a confounder explaining only 6.1% of the residual variation in both

[11] Confounders explaining more than 5.5% of either exposure to violence or of support for peace, but less on the other, can be considered using contour plots such as those below.

violence and support for peace would eliminate the effect; a confounder explaining only 0.4% of both would reduce the effect to the boundary of insignificance at the $\alpha = 0.05$ level.

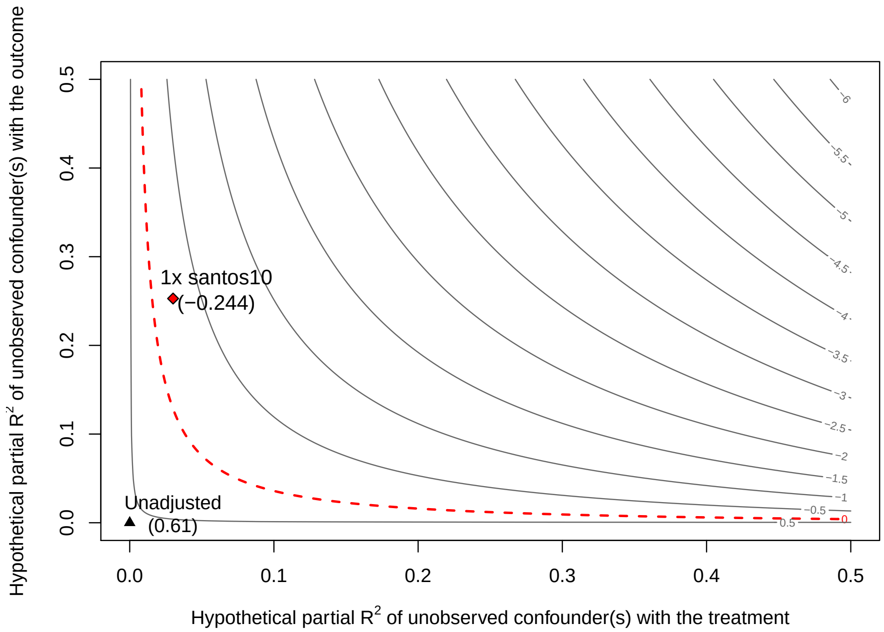
Consequently, we conclude that very small confounders indeed would alter our conclusions regarding the role of violence in support for peace. It is not hard to imagine that such confounders could exist, but we can gain further insight by examining what types of confounding would be permissible under certain assumptions, following the benchmarking approach in Cinelli and Hazlett (2018). First, we generate a contour plot that illustrates how our estimate would change as hypothetical confounding explains more of the residual variation in violence and in support for peace (Figure 3).[12] We then consider assumptions that will allow us to impose bounds upon the degree of confounding. We do this by looking at covariates we can argue to be "worse than" unobserved confounding and subsequently examining the consequences of such assumptions for the amount of confounding that is permissible.

The ideal "benchmark" covariate would arguably explain more of the treatment and outcome than unobserved confounding can, conditionally upon the other covariates in the model (in the mean squared error sense). More generally, we can argue that a confounder explains no more than k times more of (the residual variance in) the treatment and outcome than this confounder in each of these dimensions. One promising candidate for benchmarking in this way is a measure of political affiliation (prior to our measure of violence). As already argued, political affiliation may be associated with violence, but is almost certainly associated with support for the peace accord, given what we know about the politics of the deal. Here we measure political affiliation using voteshare for Santos in the 2010 presidential election (*Santos 2010*). Even if we assume that total confounding is *no worse* than this, Figure 3 shows that such a confounder would dramatically alter our conclusions, changing the estimate in Model 2 all the way from 0.61 to $-.24$. As the bottom panel shows, the same exercise can be conducted to examine how the t-statistic would be altered. Thus, while the initial results are consistent with violence affecting votes for peace, our conclusions are quite easily overturned by confounders that are potentially plausible, or at least difficult to dismiss.

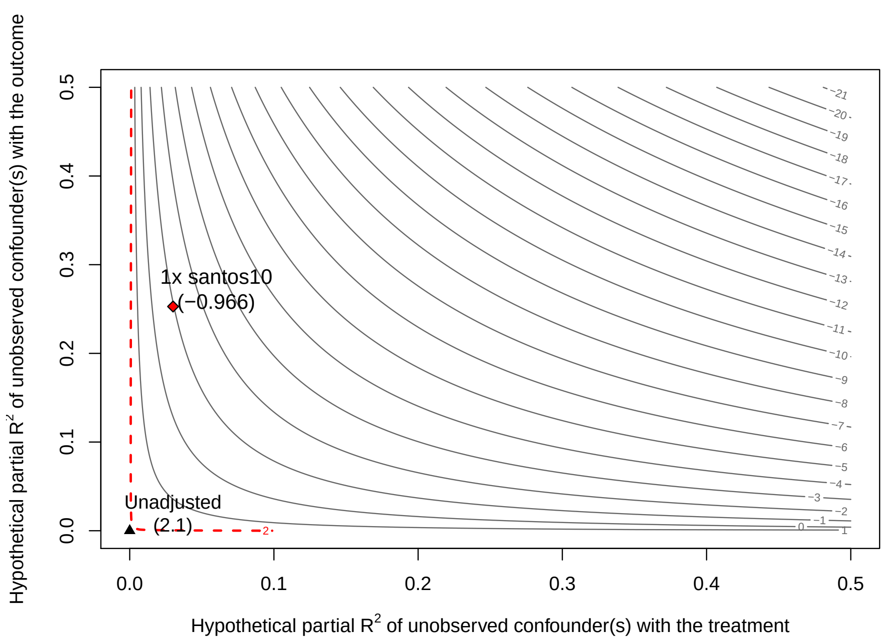
Finally, for comparison, we consider the robustness of the results reported in Tellez (2018), which estimates the effect of being in a (government classified) "conflict zone" on reported

[12] Note that these and all other results are conservative in the case of multiple confounders, possibly acting non-linearly. See Cinelli and Hazlett (2018).

Figure 3: Contour plots showing sensitivity to hypothesized confounding.



Note: Contours showing adjusted regression coefficient on recent violence, at levels of hypothesized confounders parameterized by the strength of relationship to the treatment (FARC-caused deaths in the municipality) and the outcome (municipal vote for the FARC peace deal). The bound ("1 x santos10") shows the worst confounding that can exist, were we to assume that confounding is "no worse" than the vote share for Santos in 2010 in terms of the residual variance of the treatment and outcome they explain. A confounder this bad would easily change the sign of the estimate.



Note: Similar plot as above, but showing how the t-statistics of a regression would be adjusted by a hypothesized confounder. The dashed line indicates where the t-statistic would equal 2, the approximate standard critical value for an $\alpha = 0.05$ two-sided test. If we assume confounding is no worse than the observed variable, *Santos 2010*, we see a t-statistic of nearly -1.

attitudes toward components of the peace deal in AmericasBarometer surveys. The sensitivity analysis quantities we report above for our models can also be derived from any regression table using a t-statistic and degrees of freedom. We use the models reported in Online Appendix Table A5, which are the main regression models.[13] We estimate the degrees of freedom to be approximately 4200, as there "roughly 4,200 observations" (Tellez, 2018, pg. 13). Across the three models shown there, the most favorable from a robustness point of view has an effect estimate of 0.22, and a standard error of 0.07, for a t-statistic of 3.14.[14] Using calculations detailed in Cinelli and Hazlett (2018), this translates to an RV of 4.7%, indicating that a confounder explaining 4.7% of the residual variation in who is assigned to a "conflict zone" and in attitudes toward peace would be sufficient to eliminate the estimated effect. The effect would lose statistical significance at conventional levels with a confounder explaining just $RV_{\alpha=0.05}$=1.8% of these two residual variances. Finally, a confounder explaining all the remaining variation in the outcome need only explain 0.2% of who lives in a conflict zone in order to explain away the effect. Even a confounder explaining only 25% of the residual variation in the outcome would eliminate the estimated effect if it explains just 1% of residual variation in who is in a conflict zone.

3.2 Evidence for an effect of political affiliation

We consider municipal vote share for President Santos in 2014 to be a reasonable proxy for political affiliation (with Santos). Political affiliation in this sense is an awkward "treatment", as many factors may influence the voteshare for a party or candidate at the municipal level, including socioeconomic background, average age, or educational level. The "ideal experiment" that describes what we mean by this treatment is a hypothetical example in which we could somehow change the political affiliation for individuals in a given municipality, without changing these background characteristics. Of course, such an idea may seem strange if we believe political affiliation is mainly the result of these background characteristics. On the other hand, in the US context, it is well established that "party ID" is generally the strongest predictor of how one votes, regardless (or even in defiance) of how we might expect people to vote given their apparent

[13]Online appendix available at `https://www.dropbox.com/s/uw843lubacqt7y2/worlds-apart-online-supplement.pdf?dl=0`

[14]These numbers correspond to Model (2) in Table A5, with the outcome measure of willingness to grant concessions to FARC.

self-interest. In this sense, the treatment we have in mind is closer to the idea of political "loyalty" – the degree to which one's vote in the referendum is strictly a function of the position taken by the political leader they follow. Other reasons why one's vote on the referendum may be correlated with one's political affiliation – such as a socioeconomic background that explains both support for peace and political affiliation – would thus be considered confounders.

We model the effect of political affiliation according to

$$y_i = \beta_0 + \beta_1(Santos\ 2014_i) + \beta_2(deaths_{i,10-13}) + \beta_3(elevation_i) + \\ \beta_4(gdppc_i) + \beta_5(population_i) + \epsilon \quad (3)$$

where y_i is the proportion voting "Yes" in municipality i and $deaths_{i,10-13}$ is the total number of deaths due to FARC violence between 2010 and 2013 in municipality i.[15] We again control for total number of eligible voters ($population_i$), as well as the mean elevation above sea level ($elevation_i$) and GDP per capita of the municipality ($GDPpc_i$).

We can again augment the usual regression table to show sensitivity statistics (Table 2). The estimated effect of Santos 2014 vote share on support for peace is positive and statistically significant. This is a very strong relationship in terms of variance explained: voteshare for Santos in 2014 explains a stunning 59% of the residual variation in support for peace. This has important ramifications for sensitivity. Most directly, it means that even a confounder explaining 100% of the residual variation in the outcome would need to also explain 59% of the residual variation in voteshare for Santos in order to eliminate the estimated effect ($R^2_{Y\sim D|\mathbf{X}}$). A confounder explaining equal portions of voteshare for Santos and support for peace, would have to explain 68% of both (RV) to eliminate the effect, or 66% of both ($RV_{\alpha=0.05}$) to reduce the effect below statistical significance at the $\alpha = 0.05$ level.

Table 2: Augmented regression results for political affiliation

Outcome: *Vote for peace deal*

Treatment:	Est.	SE	t-stat	$R^2_{Y\sim D\|\mathbf{X}}$	RV	$RV_{\alpha=0.05}$
Santos 2014 vote share	0.67	0.02	37.5	59%	68%	66%
df = 983						

[15]Note that we use deaths between 2010–2013 as the pre-treatment covariate here, as the presidential election occurred in 2014.

While this describes the degree of confounding required to alter our conclusions, are such confounders likely to exist? We can never answer this definitively; however, we consider two lines of argumentation. First, finding such powerful relationships is rare. Correlations considered strong in many social science settings are typically in the 0.3 to 0.5 range, which imply R^2 values in the 0.1 to .25 range. The confounding required to alter our conclusion is much stronger. Second and more principled, we return to the bounding approach employed above. One potential confounder is GDP per capita, which we expect might affect both treatment (choice of political party) and the outcome (support for the deal). Figure 4 shows the contour plot, to which we add a bound on the basis of an assumption that "confounding is no more than three times 'worse' than GDP per capita", in terms of the residual variation the confounder would need to explain in where violence occurs and in support for peace.[16] The dashed line indicates the bound at which the result would be eliminated. A confounder three times as strong as that of GDP per capita would hardly reduce the estimate – from 0.67 to 0.64. Similarly, *elevation* can be used to formulate a bounding assumption, as previous scholarship has found a relationship between mountainous terrain and various forms of violence (see e.g. Fearon and Laitin, 2003; Do and Iyer, 2010; Nemeth, Mauslein and Stapley, 2014). Let us therefore assume that confounding is no more than three times "as bad as" elevation. Again, the worst confounding that is possible under such an assumption would still hardly change the result, from 0.67 to 0.64.

Figure 5 shows "extreme-scenarios". We already know from the $R^2_{Y\sim D|\mathbf{X}}$ value in Table 2 that a confounder explaining 100% of the residual variance of the outcome would need to explain 59% of the residual variance in political affiliation in order to overturn the result. We can further see in Figure 5 that a less conservative confounder that explains 50% or 30% of the residual outcome variation would have to explain over 70% and 80% of the residual variation in *Santos 2014*, respectively.

In short, extremely powerful confounding would be required to alter our conclusion about the role of political affiliation in explaining support for the peace deal. While we cannot definitively rule out such confounding, our sensitivity analyses do provide a measure of confidence in the observational evidence provided here and in Matanock and Garcia-Sánchez (2017).

[16] Note that we may wish to consider a k value for GDP per capita even higher than 3 to determine how far this robustness. As it turns out the maximum *possible* k value on this variable is 3.88. Such a proposed confounder would explain all of the residual variance of either the treatment or the outcome, and so a proposed confounder higher than this cannot exist. At $k = 3.88$, the point estimate still barely changes, to 0.63.

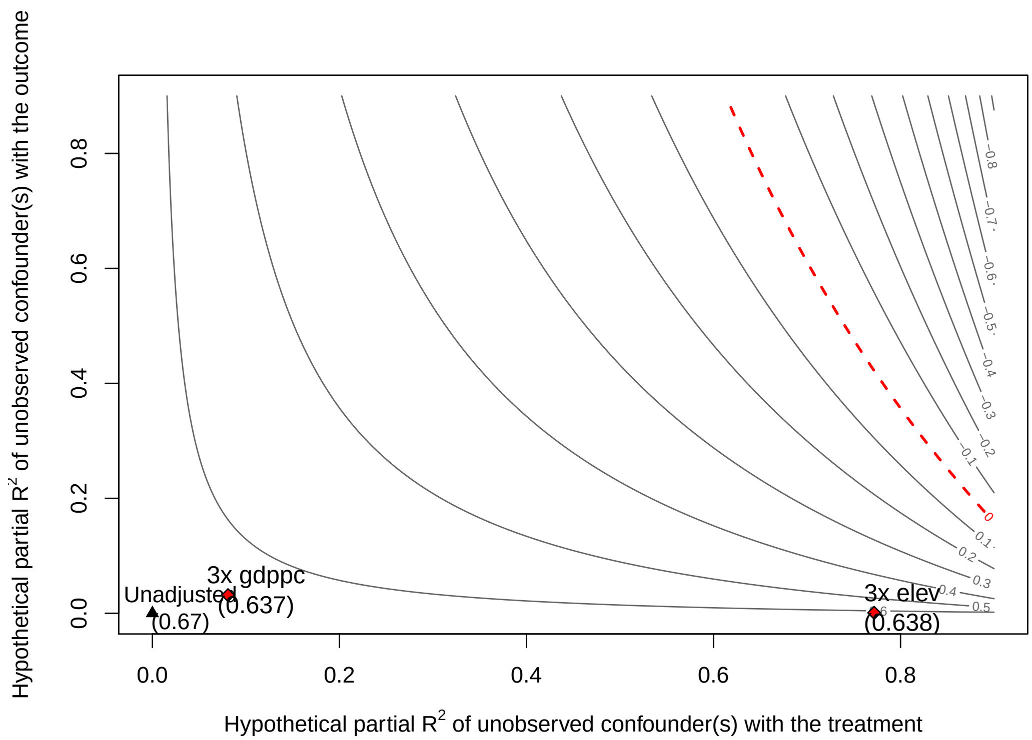


Figure 4: Effect of unobserved confounding on estimate for political affiliation

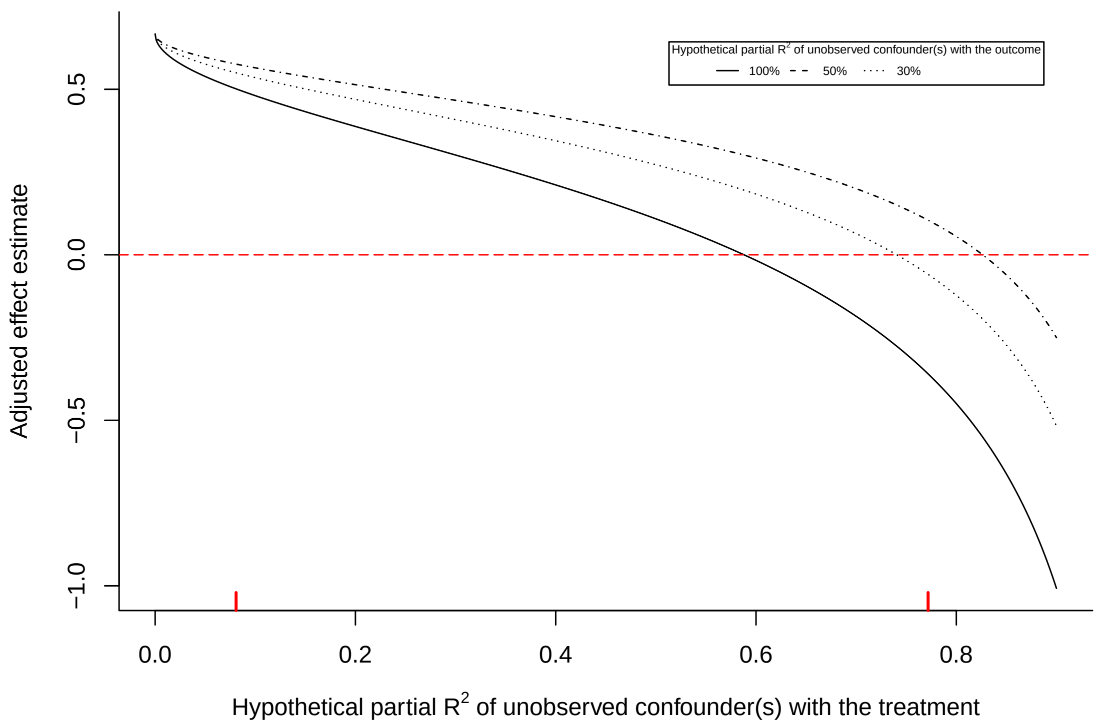


Figure 5: Worst case scenario

4 Conclusion

As suggested by the media, both exposure to violence and political affiliation are *plausible* explanations of votes in favor of the Colombia FARC referendum: they are consistent with the observed evidence, and even produce substantively and statistically significant estimates in some of the models that investigators may employ or already have employed. Conventional analyses may stop here, and scholars may either accept them (if they are willing to set aside confounding concerns), or reject them (if they worry confounding cannot be ruled out under such research designs).

However, we can say much more and with greater precision. First, the estimated effect of exposure to violence on support for the peace deal is extremely fragile to potential confounding: in the stronger of the two models shown here, a confounder explaining just 6.1% of the residual variance of exposure to violence and support for peace would eliminate the result entirely, and a confounder explaining just 0.4% would reduce it below conventional statistical significance. Estimates from prior work finding that exposure to violence is associated with higher support for peace in this referendum (Tellez, 2018) are similarly fragile.

By contrast, evidence for an effect of political affiliation requires far larger confounders to explain away. In the model tested here, even a confounder explaining 100% of the residual variation in support for peace would have to explain 59% of the residual variation in political affiliation to alter our conclusions. Still, we cannot say that such confounding is impossible. In particular, because political affiliation may result from background characteristics such as socioeconomic status, it is possible that powerful confounders exist. That said, if we assume that confounding is limited only in that it can explain no more than three times the residual variance explained by GDP per capita, the remaining confounding can be bounded to a level that barely changes the point estimate. Future improvements can be made if investigators can name (and include) confounders they believe to be sufficient to alter our conclusions, according to the sensitivity results above.

Additionally, future research may propose alternative identification strategies, such as landmines or instrumental variables, that allow us to better rule-out confounders and make the resulting estimates and sensitivity analyses more persuasive. For example, with sufficient information on the penetration of landmines in each municipality, the timing of when these landmines go off may provide the basis for a natural experiment. These models could also be augmented by sensitivity analyses. However, our work demonstrates that even without a perfect identification strategy, researchers may still learn something through sensitivity analysis.

On a broader epistemological note, many substantively important and policy-relevant questions do not lend themselves to credible natural experiments, instrumental variables, or other convincing identification strategies. Rather than choose not to investigate these questions, a sensitivity-based approach concedes that observational estimates are simply descriptive and influenced by confounding, but shifts our attention to the question of how bad the confounding would need to be to change our conclusions. This disciplines the debate regarding what we would be required to believe about confounding to dismiss or to affirm these estimates, and thus — we hope — provides a path forward to other scholars on how to convincingly answer questions that face similar challenges to identification.

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