Quiet Riot:
Estimating a Causal Effect of Protest Violence*

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Version: August 2015

Preliminary Draft - Comments Welcome

Abstract

Recent protests taking a violent form, in the United States and elsewhere, have renewed interest in the study of such low-level political violence and its efficacy. However, estimating the effect of violent forms of political protest on protest success is complicated by endogeneity and omitted variable bias. In this work, I utilize instrumental variables methods to estimate a causal effect of violent protest on the likelihood that protesters win policy concessions. Using daily French protest data and a set of school holiday and weather instruments, I find a significant and negative relationship between property destruction associated with protests and the chance of near-term success in changing policy. The IV estimates are larger than OLS estimates and are robust to a variety of alternative specifications. Such findings are predicted by several posited endogeneity channels, and, they suggest that political violence does not, in fact, pay off.

†Department of Economics, Middlebury College (email: ehuetvaughn@middlebury.edu). I am especially grateful to Emmanuel Saez and Edward Miguel for sustained guidance and frequent inspiration. I also thank Leonardo Arriola, Liang Bai, David Card, Ernesto Dal Bo, Frederico Finan, Patrick Kline, Gabriel Lenz, Jonah Levy, Peter Lorentzen, Kevin O'Brien, Robert Powell, Ana Rocca and seminar participants at the UC Berkeley development lunch series and the Conflict Consortium Virtual Workshop for valuable feedback. John Ridener of the UC Berkeley Earth Sciences and Map Library provided generous training in ArcGIS. All mistakes are my own.

I Introduction

The decision to engage in violent forms of political protest comes at significant personal and economic cost. Risk of arrest, injury, and even death are high and the associated negative economic consequences are often lasting.[1] Yet, in the face of these steep costs, we regularly observe political actors turning to violence, even in economically developed and relatively democratic societies where other modes of political influence are readily available.

In light of this, one important question is whether political violence actually works. The answer to this question is of intrinsic interest to empirical researchers and critical to our understanding of why individuals engage in costly political violence at all. While theoretical and empirical work has made headway explaining the causes of political violence (Walter, 1997; Collier and Hoeffler 1998, 2004; Fearon and Laitan 2003; Powell, 2003, 2006; Dal Bo and Dal Bo 2004; Sambanis 2004; Garfinkel and Skarpedes 2007; Blattman and Miguel, 2010), existing scholarship regarding its effectiveness, spanning the disciplines of political science, sociology, and economics, offers conflicting empirical results. Early work on labor unrest by Shorter and Tilly (1971) finding a positive relationship between violent protest and protest success has since been complemented with positive findings in the context of anti-war protests (McAdam and Su, 2002) and urban rioting and the expansion of the welfare state (Colby, 1975; Jennings, 1979; Hicks and Swank, 1983; Iris, 1983; Fording, 1997, 2001).[2] Chenoweth and Stephan's (2009; 2011) work studying global resistance movements is among the most recent to find the opposite relationship, and is preceded in this negative finding by Franklin (2009) (studying Latin American protest movements), Snyder and Kelly (1976)

[1] For instance, Collins and Margo (2007) show a persistent and statistically significant depression in long-term black home owner property values in areas hardest hit by "race riots" in the 1960s. See also Collins and Margo (2004) and Abadie and Gardeazabal (2003).

[2] Many of these works on American urban riots of the 1960s derive their conclusions not from a comparison of violent and non-violent protests but by comparing the degree of violence among a set of violent protests. Works explicitly assessing the effects of protest violence relative to non-violent protest will be discussed further in Section II.

(studying labor unrest), and Welch (1975) (also studying urban riots but challenging the Piven and Clawford (1971, 1977) social control thesis)). Still, other work has found no net effect of protest violence on protest success (Kelly and Snyder, 1980; Frey Dietz and Kalof, 1992, who reanalyze Gamson, 1975) or conflicting results that lend support to both a negative and positive interpretation of the efficacy of protest violence (Button, 1978; Isaac and Kelly, 1981). Related literature focusing on the efficacy of terrorism (Pape, 2003; Abrahms, 2006, 2012; Gould and Klor, 2010) has also found mixed results.

Clearly, when it comes to the question of whether political violence is an effective means of achieving concessions, the existing empirical literature has not produced a consistent answer. This may be due to the lack of clear causal identification in existing work. Specifically, preceding papers on the efficacy of protest violence have not adequately addressed confounding issues of omitted variable bias or the endogeneity of violent forms of protest to prospects of protest success, possibly explaining the wildly different reported estimates and casting doubt on any inferred causal relationship.

This paper is the first to look at the efficacy of protest violence using an identification strategy that allows for credible causal inference. Specifically, we use a set of weather and school holiday measures as instrumental variables for protest violence, a design made possible by disaggregated, micro-level protest event data constituting the universe of reported French protests from 1980-1995 and including an indicator for property destruction. Our results suggest that protest violence lowers the incidence of obtaining a concession. Moreover, this statistically significant negative relationship is more pronounced than revealed by naive regression analysis that does not take into account omitted variable bias or endogeneity. The significant negative estimate is robust to a variety of specifications and remains even when relying only on the more traditional weather instruments. As a result, this paper provides the first empirical evidence that protester violence has a negative *causal* effect (as distinguished from simple correlation) on the incidence of concession that protesters obtain.

Successful identification of the causal effect of protest violence is hard to achieve in the non-experimental world in which scholars of political violence find ourselves. Correlations found with ordinary least squares regressions on observational data may be biased downward by unobserved negative local economic shocks, for instance, which might both lower the flexibility of firms or governments to make concessions to protesters and lead to an increase in protest violence (if the opportunity cost of engaging in violence has lowered with the worsened economic conditions or if police forces have been cut in austerity policies responding to the downturn). Alternatively, OLS results may be biased upward by unobserved weakness of the protest target, as protesters may be more likely to use violence when there are fewer negative consequences to it from a weaker authority and a weaker authority may be more likely to concede from any protest, violent or not. Similarly, some policies may be perceived as going too far by the public *and* by elements of the governing coalition, thus, enflaming the public's passions and independently leading to unreported internal channels of successful elite checks on the controversial policy. In short, any number of additional pathways exist that might compromise the reliability of causal inference using observational data and the traditional OLS specifications used to date.

If able to conduct a field experiment to avoid the pitfalls inherent in the use of observational data, one might randomly purchase bus tickets for black-bloc anarchists to attend some protests and not others, and then assess whether there are differential success rates at those protests with and without the black-bloc participants. Instead of this notional experimental ideal, however, we must rely on non-experimental data in the real world. As such, researchers interested in causal identification are left with the difficult task of finding a source of naturally occurring, random-like variation in the violence associated with political protest. Instrumental variable methods offer one such solution. Specifically, we seek instrumental variables which are uncorrelated with the potential outcome (exogeneity assumption) but which are correlated with protest violence (relevance) and which have no effect on the

protest success except through their effect on violence (exclusion restriction)(Angrist et al., 1996; Woolridge, 2002; Angrist and Pischke, 2009).

We argue that the instrumental variables used here - precipitation, maximum daily temperature, and secondary school holidays - observe these properties and provide us with random-like variation in protest violence that allows us to test the efficacy of political violence. Rainfall and temperature on the day of a protest are thought to affect the incidence of violence at the protest through the relationship between these variables and physiological or psychological states that incline one to violence. In the case of temperature, the general folk notion of hot temperatures begetting hot tempers has basis in social psychology and criminology literatures. For instance, Rotton and Cohn (2000) find disorderly conduct calls increase (though not monotonically) with temperature. Other work shows aggressive behaviors increasing with temperature (for a linear effect of temperature see Kenrick and MacFarlane, 1984; Reifman, Larrick, and Fein, 1991; and the survey by Anderson et al., 2002; for evidence of an inverted U see Baron, 1972; Bell and Baron, 1976; Rotton and Cohn, 2000a, 2000b). Hsiang, Burke, and Miguel's (2013) meta-study finds rising temperatures associated with increases in a variety of violent behaviors, such as domestic violence, other violent crime, armed conflict, and rioting. In the case of precipitation, getting wet lowers the local body temperature, causing protesters to conserve energy and lowering the likelihood they will engage in physically taxing, caloric-intensive violent behaviors. This paper follows a series of recent papers that have used rainfall as an instrument.[3] In the most closely related, Collins and Margo (2007) use rainfall as an instrument for riot severity and

[3]Rain has been used as an instrument in several papers since Miguel et al. (2004) pioneered its use in the study of political violence. In their case, the authors sought to explain the onset, or existence of, civil war, treating political violence as the outcome of interest and using rain to instrument for economic growth in a study of the effect of income shocks on the outbreak and incidence of civil war. Bruckner and Ciccone (2011) similarly use rainfall as an instrument for economic shocks to asses the effect of income shocks on the degree to which a country possesses democratic institutions. Unpublished work by Madestam et al. (2012) makes use of rain as an instrument for protest size, not violence, a potential concern for our identification strategy. However, we do not find a significant effect on size in this work, discounting the possibility that there is an exclusion restriction violation from rain affecting protest success via weather variables, as detailed below.

cite extensive media reports in the U.S. in the 1960s making the link between rainfall and riot severity.

The rational for the school holiday instrument comes from the incapacitation effect of school. Jacob and Lefgren (2003) document that the level of property crime (the measure of violence used here) committed by juveniles in the United States decreases by 14 percent on days when school is in session. In our context, we theorize the composition of protests held on school holidays will change to include an increased share of teenage participants (who are no longer incapacitated by school) and the greater destructive tendency of these younger protest participants will lead to greater incidence of property destruction associated with these protests. Ethnographic studies of soccer hooliganism find evidence of rioters rioting for the thrill of the riot (Buford, 1991; Wilkinson, 2009) and something similar may be going on with French youth, though we are agnostic as to whether this is a more appropriate explanation than criminal burglary motives, or general intemperateness among hormonal teens. While this work constitutes the first time any of our three instruments (or any others) have been used to estimate protest violence's effect on protest success, rainfall and temperature instruments have been used in other studies on violence. There has been no previous use (to our knowledge) of the school holiday instrument, however (though, as stated, the results are robust to use of only the more traditional instruments as well). We use all three of these instrumental variables to identify plausibly exogenous variation in protest violence, and hope this effort brings a newfound interest to clean identification of causal effects in the study of political violence and civil conflict.

In the section that follows we present a survey of related literature regarding political violence. In Section III we review the data sets and data construction methods. Section IV presents the estimation framework. Section V presents the main results and considers their robustness. In Section VI we briefly review the consistency of candidate modeling assumptions with the results presented here, and Section VII concludes.

II Existing Literature

In recent years economists and political scientists have paid renewed attention to the study of violent civil conflict and political violence. Much of this work has tried to explain the outbreak of civil war (Collier and Hoeffler 1998, 2004; Fearon and Laitan 2003; Sambanis 2004). Contest models have modeled the decision to go to war as a choice between production and predation, with the probability of war increasing as the opportunity cost of fighting decreases (Dal Bo and Dal Bo 2004; Garfinkel and Skarpedes 2007). Models of asymmetry of information (Powell 2002) have predicted outbreaks of civil war when opposing sides fail to understand the true probabilities of success, and commitment problems have been proposed as further explanations of violent breakdowns in bargaining (Walter 1997; Powell 2006). The consequences of violence have also been studied in the context of this recent renaissance in the study of civil war, with attention paid to the effects of war on poverty, growth, human capital, and other macro-level indicators (see Blattman and Miguel 2010 for a detailed summary).

The focus of this paper departs from this emerging civil conflict literature in two main ways. First, while many recent works study political violence from the vantage of large-scale civil wars, here, we are interested in lower-level civil unrest in the form of popular protest, including demonstrations, occupations, strikes, and other sub-military political protest acts in which protest may be used to exact short term, issue-specific policy concessions rather than to overthrow an entire regime (non-displacement goals) and where opposition to a certain policy may or may not take a violent form. Secondly, in contrast to much of this work, which focuses on providing explanations for the causes of civil conflict, I look at civil unrest's *effects* - specifically the effect of protest violence on the achievement of policy concessions that are consistent with protester demands.

Such a research focus has precedent in both the sociological and political science liter-

atures. Classic work in sociology by Shorter and Tilly (1971) finds a positive correlation between violence and historical strike success in France (using primarily bivariate cross-tabular analysis), while Snyder and Kelly (1976) find the opposite with respect to industrial violence in Italy after controlling for other protest characteristics in a linear probability model. In a multivariate re-anlysis of Gamson's (1975) influential work studying the efficacy of a broad group of American social movements, Frey, Dietz, and Kalof (1992) find a weakly significant (only 10% significance level at best) negative effect of violence on the achievement of concessions. Giugni (1998) offers an overview of the mixed empirical evidence regarding the ability of urban rioting to effect policy and lead to protester gains. To take one recent example in this strand of work, McAdam and Su (2002) find that property damage associated with anti-Vietnam war protests increase the "pro-peace" vote share in Congress while, also, slowing the frequency of Congressional votes about the war.

In political science, Franklin (2009), conducting a multinomial logit analysis, concludes that disruptive but non-violent contentious challenges to Latin American governments were more effective at gaining concessions and that violent challenges were likely to lead to repression. Chenoweth and Stephan (2009, 2011), using a combination of case studies and regression analysis with a detailed international and historical data set, find that violent tactics diminish the chance of a movement's eventual success. Other work, yielding varied results, has focused on the outcomes of riots without making comparisons directly to non-violent protest (Welch, 1975; Hicks and Swank 1983; Fording 1997, 2001).

Inherent in all of this work is the lack of exogenous variation in violence (no small, and no easily corrected matter), leaving open questions as to whether endogeneity or omitted variable bias are driving any of the above (conflicting) results. To the author's knowledge, no existing studies on the efficacy of protest violence have used an instrumental variables strategy or other quasi-experimental methods to effectively or convincingly control for the endogeneity or omitted variable bias affecting violence's occurrence. Chenoweth and Stephan

in the notable book version of their work are the first to make an attempt to implement an IV strategy in a related context, but their ability to do so effectively is hindered by the difficult setting they work in, one which lacks obvious candidates for valid instruments applicable to the geographically and temporally varied protests under study[4] (they focus on maximalist protest campaigns - anti-occupation, secessionist, and regime change movements - that fall outside the scope of the current study). Though not pursuing an IV strategy, two additional papers deserve mention for research designs that share the spirit of this work's pursuit of random-like variation in the incidence of political violence and use of micro data. One, by economists Eric D. Gould and Esteban F. Klor (2010), looks at the effectiveness of political violence - however, in this instance in the context of terrorism - by using the geographic variation in terrorist attacks as an identification strategy to determine that more violent terrorist acts, up to a point, effectively shift the opinions of the targeted population to a more conciliatory position (they do not make a comparison with non-violent tactics). The other paper, by political scientist Jason Lyall (2009), focuses on the effects of state violence rather than protester violence, positing allegedly indiscriminate artillery shelling as a random-like distribution of violence, finding that the areas affected by the shelling were less likely to host future insurgent attacks. While the identification strategies of these papers hinge on the presumption that Palestinian militants and the Russian military, respectively, choose the location of their targets randomly, and may thus be open to challenge, we admire their attention to causal identification and think they present some of the more interesting initial approximations to date of Susan D. Hyde's (2010) entreaty to extend field experimental methods to the study of political science (in her case international relations, in ours political violence). In the study of political violence, approximations may be the best we can do, as

[4]In Table 3.3 of their book, Chenoweth and Stephan report the results of three IV specifications. All specifications rely on an instrument for whether the protest campaign is secessionist. While there may be first stage relevance for this variable, it poses a clear threat to the exclusion restriction (secessionist campaigns are likely to have a direct effect on the incidence of concession and the choice to make a political campaign explicitly secessionist may be endogenous to the chance of success).

the notional political violence field experiment raises obvious legal, ethical, and operational hurdles. It is the author's opinion that the instrumental variables design offered here presents the next-best thing to the randomized trial gold standard, and that this work takes us a large step forward in the pursuit of successful identification of the causal effect of political violence.

III Data and Measurement

A Protest Data

All explanatory variables and outcome measures are derived from the European Protest and Coercion Data (EPCD) database, created by political scientist Ronald Francisco.[5] This micro-foundation data set contains a rich set of variables characterizing protests, including date, day, location, protester identifier, protest target, issue under protest, protest size, measures of protest violence, and protest form/action type (e.g. rally, strike, occupation, hunger strike etc.). The data covers the years 1980-1995 and provides a daily record of "all reported protests" in France with identifiable date and location that appeared in the newspaper record of some 60-plus French and international newspapers and wire services. Analysis here is limited to the French mainland and excludes observations whose issue of protest/grievance is coded as various forms of "separatism" (the frequent recourse to violence and lack of success among Corsican, Breton, and other separatists of the era suggests that their inclusion would only make OLS estimates more negative, but we leave an IV analysis of the impact of violence in separatist movements, which are likely governed by a different dynamic than the non-maximalist campaigns studied here, for future work).

The dependent variable is derived from a binary measure of whether the abovementioned news sources report that protester demands were met with a concession by the protest's

[5]Data is available on Ronald Francisco's website http://web.ku.edu/~ronfran/data/index.html. For discussion of the benefits of protest event analysis data sets of this type see Nam (2006) and Koopmans and Rucht (2002).

target (coded as 1) or not (coded as 0). To the author's knowledge, other existing, multi-issue protest data sets do not allow for the present analysis of the effect of protest violence on protest success since they do not provide the information needed to code outcome measures while at the same time using daily protests with specific locations as the unit of observation, a necessary requirement for the IV strategy pursued here. While the EPCD does do so, this clear advantage comes with certain caveats. For one, the outcome measure almost certainly undercounts protest successes, particularly when protests indirectly affect policy changes and when the policy changes occur in the long run, as the reported concessions that constitute a success in our metric require journalists to some way link the protest and the protest target's subsequent changed behavior. The more indirect, second-order, or temporally distant any such change is from related protests the less likely news reports are to link policy changes to protesters' actions.[6]. Additionally, while this effect of temporal distance would be true in any event, as explained in the next subsection it is necessarily the case here given the data contruction strategy outlined below. Thus, concessions in this paper should be understood to be attributed concessions that end a protest or take place during its span or in a short window following the protest's end (specifically within seven days, as detailed below), and it is entirely possible that longer-run policy changes may be impacted differently by protest violence than the short-run changes assessed here. Taking account of these longer-run effects would presumably increase the rate of total concession beyond the relatively low rate observed in the data set overall (as seen in Table 1).

Secondly, an outcome coded as a concession should not be interpreted as an absolute

[6]That news reports are more likely to miss the indirect, second-order, or temporally distant effects of protests on policy highlights how concessions here, as with any data relying on media sources, should be understood to be *attributed* concessions (with the act of making such attribution always subject to a degree of subjectivity and possible deviation from real happenings, whether from reporters' judgments (Davenport, 2010) or those of data coders) Even if journalists are not taken as the mediators of information, Lipsky (1968) and Tarrow (1998) point out the inevitable difficulty in attributing the full range of successes that are in part due to popular protest, as the more numerous indirect and non-proximate successes are very difficult to trace. See O'Brien and Li (2005) for a discussion of some of the indirect channels of protest efficacy in a Chinese context.

victory for protesters, but, rather, as arrival at some point along a spectrum ranging from winning everything demanded to winning something more than nothing. In many cases, the data set describes a newspaper account of a concession that clearly marks a complete capitulation to protester demands by the protest target ("management agrees to demands", or, workers "end strike after winning pay increase"), but, in others, the extent of the protesters' win is exceedingly modest ("management agrees to reduce number of layoffs from 270 to 192"). Protests that win concessions should be seen then as an improvement beyond the expected status quo absent the protest.

B Protest Event Groups

As indicated, the EPCD database records daily protests. However, protests over successive days are often linked and are reasonably thought of as a unified event (e.g., an ongoing strike, an encampment in the main city square, a building occupation, an organized week of action). Thus, the unit of observation is taken to be such unified, ongoing protests, termed protest event groups. Specifically, these protest event groups consist of protests that share the same protester and target and protest issue and that are separated by no more than a week from another protest in the group.[7] Those protests without a defined issue were dropped on the grounds that we are interested only in protests with a clearly defined claim, and, those protests which coders could not classify into issue categories may be thought to lack an identifiable focus. Data is then collapsed by these groupings, with the protest event groups taken as the unit of analysis. A seven day window between same protester-target-issue protests is used to account for the fact that reporting on ongoing protests can drop off in the dataset and then reappear days later in another news story, and we want to avoid

[7]For a more detailed summary of the algorithm used to group protests and a description of the manual checks using basic article descriptions of protests that were undertaken to avoid false matches or false splits of protest event groups, contact the author. It is encouraging to note the results reported do not depend on the manual checks and are robust without them.

classifying such protests as separate events rather than one protest event with a long span. However, this decision also imposes an upper limit of seven days from the final day of protest by when a concession must materialize to be counted in the analysis, since a protest event group is assigned a concession as the value for its outcome variable if any of the individual daily protest reports composing the protest event group reported a concession. In this sense, the consequences of political violence assessed here truly are short-term consequences for protest success.

With these protest event groups as the unit of observation, a word on the construction of the explanatory variable of interest and other explanatory variables is in order. In the raw data, protest violence is characterized by an indicator variable that takes a value of one in the event of reported property destruction attending the protest. To measure the degree to which such property destruction defines the protest event group, in the collapsed data the *Violence* variable used in the following specifications is the fraction of newspaper reports on the protest throughout the protest's span that report the occurrence of property damage, giving us a measure of the the degree to which a protest event group is characterized by violence. Additional explanatory variables used in some specifications include *Paris*, the number of days that protests took place in the capital in the protest event group; *Size*, an indicator variable 1[average reported protest size in the protest event group $>= 1000$]; *Election*, an indicator variable equal to 1 if part of the protest span is between thirty days prior to the first round of a presidential election and the first round election (or, the second round election, if applicable); and *Duration*, the number of days from the start to finish of the protest event group.

C Weather and School Holiday Data

Data for the instruments is taken from two sources. For exogenous weather variation, precipitation and temperature data comes from the National Climatic Data Center's (NCDC)

Global Surface Summary of Day database.[8] The NCDC data provides daily readings from more than 350 weather stations across France (224 of which provide usable data during our period of study), reporting maximum daily temperature (in degrees Fahrenheit) and total daily precipitation (in inches). Other common precipitation data sets, such as the Global Precipitation Climatology Project, are inadequate for the present analysis as they present monthly rather than daily readings. For school holidays, the moving annual calendar of French secondary school holidays, standardized nationally by educational zone, is taken from French Ministry of Education records.[9] Traditionally, French secondary schools see several week-long holidays at intervals of a month to a month and a half throughout the year, as well as an extended holiday in the later part of July and August, providing temporal variation in the holiday measure. The educational zones rotate from year to year so that educational zone A and educational zone B may be on the same holiday calendar one year, but not the next, providing additional regional variation in the holiday measure as well.

Using ArcGIS global imaging software, each individual daily protest is geographically plotted. For the assignment of weather variables, each daily protest is matched with the nearest French weather station containing non-missing data for the day. See Figure 1 for an ArcGIS map of protest and weather station locations. Notice the distribution of unique protest sites and weather station sites is spread across the country with observations spread throughout the French administrative regions. For protests taking place throughout entire administrative regions, departments, or geographic areas, the average maximum daily temperature and average daily precipitation level for weather stations in the relevant area is used for the daily record. Upon collapsing data into protest event groups, the principal precipitation measure used is a dummy variable, *Precipitation*, that takes the value of 1 if it rained or snowed at least one day of the protest's duration at one of the protest sites, while

[8]Data can be found at https://gis.ncdc.noaa.gov/geoportal/catalog/search/resource/details.page?id=gov.noaa.ncdc:C00516

[9]http://www.education.gouv.fr/cid197/les-archives-calendrier-scolaire-partir-1960.html

the principal temperature measure used *Temp* is a dummy variable that equals one when the maximum temperature averaged across days and sites of the protest falls in the range of 60 to 75 degrees Fahrenheit. This range is chosen primarily because it gives a strong first stage consistent with theory, but there are also findings in social psychology literature indicating aggressive behavior and disorderly conduct peak in warm temperatures in this range (See Figure 2, from Rotton and Cohn, 2000a) rather than rising monotonically, suggesting the conventional belief that with rising temperatures come rising tempers holds only up to a point (Baron, 1972; Bell and Baron, 1976; Rotton and Cohn, 2000a, 2000b). Alternative temperature measures, including a linear temperature measure and a dummy for extreme hot or cold temperatures, are tested and arrive at qualitatively similar results for violence as many of the specifications using the *Temp* variable presented below. Similarly, results are robust to increasing the threshold for the precipitation dummy to the mean of average precipitation across all protests and using this instrument instead.

Once protests are plotted in their administrative department, the moving annual calendar of French secondary school holidays is used to determine if secondary school students at the protest location were on extended holiday on the day of the protest. For protests reported to span over multiple school zones, that protest is considered to take place on a school holiday if some of the protesters protest in an area on holiday (results do not differ significantly when they are assigned the holidays common to the spanning school zones). The *Holiday* variable then is defined as the proportion of days at protest event group locations during which students are on extended school holidays.

IV Estimation Framework

A Ordinary Least-Squares Regressions

Table 2 reports ordinary least squares (OLS) regressions of protest success on protest violence and additional protest characteristics. The linear regressions are for the linear probability model

$$y_i = b + \alpha Violence_i + X_i^{'} B_1 + \epsilon_i \tag{1}$$

where y_i is a binary measure of whether protesters succeeded at winning a concession from the target of the protest (as reported by the press), $Violence_i$ is the fraction of newspaper reports on the protest throughout the protest's span that report the occurrence of property damage, X_i is a vector of other variables characterizing the protest which are used in some specifications (enumerated in Section III), and ϵ_i is a random error term, where these errors are clustered by protest target to account for correlations in probability of success among those protests with the same target. Such correlation is reasonable when successive demands upon a protest target affect the target's willingness to concede to future demands.

Column (1) reveals a small but significant negative correlation between protest violence and protest success. Going from a protest event group characterized by no violence to one characterized by violence on every day of its duration lowers the chance of achieving a concession by 2%. Controlling for annual time trends in Column (2) leaves this estimate unchanged. Similarly, adding other explanatory variables that have theoretical foundation in the protest and social movement literature has no effect (results for these other variables, including duration, location, size, and election cycle, are reported in more detail in the Appendix).

Overall, the results in Table 2 demonstrate that violent protests are significantly less likely to result in a concession. There are many reasons, however, not to interpret this correlation as a causal relationship, as already indicated in the discussion in Section I. To obtain a causal effect of political violence on protest success we now turn to an instrumental variables strategy.

B Instrument Relevance

Our identification strategy makes use of a combination of three instrumental variables for protest violence, which is treated as an endogenous regressor from here on. To ascertain the strength of these instruments, violence is modeled as

$$Violence_i = a + \delta Precipitation_i + \gamma Temp_i + \mu Holiday_i + X_i^{'} B_2 + e_i \qquad (2)$$

where *Precipitation*, *Temp*, and *Holiday* are defined as at the end of Section III, $Violence_i$ and $X_i^{'}$ are as defined in the previous subsection, and e_i is a random error term allowed to be correlated across observations with the same target of protest.

The first-stage relationship between the instruments and protest violence is significant and robust to the inclusion of additional control variables, as can be seen in columns (1) thru (3) in Table 3. The *Precipitation* variable is significant at the 1% levels in all specifications (with the t-stat being particularly large) and the *Temp* and *Holiday* variables are significant at the 5% significance level or better in all three specifications. The robust F statistic ranges from 13 to 23, suggesting that there is not a weak instrument problem (Stock et al. 2002; Stock and Yogo 2002).[10]

[10]The tables for weak instrument tests presented in the Stock et al. works are not directly applicable in this context, and to our knowledge there is not a comparable work for gmm estimation with non-iid errors. The usual rules of thumb should be seen as suggestive as a consequence.

Moreover, the signs on the instruments' coefficients are all in the direction expected by theory: rainfall leads to less violence, warm temperatures lead to more violence, and violence spikes when students are not incapacitated by school. Results consistent with our theory are present in the underlying uncollapsed data as well, and, when the first stage is run with one instrument at a time the coefficients and standard errors are very similar to those in Table 3 (results unreported). Alternative instruments using the underlying weather and holiday data (including a dummy for rainfall above the median and a linear temperature term) also yield similar results.

V Main Empirical Results

A Initial Estimates With Violence as Endogenous Regressor

Baseline instrumental variables results using *Precipitation*, *Temp*, and *Holiday* as instruments for *Violence* can be seen in Table 4. In the presence of non-iid errors (as is the case here) GMM estimation provides improvements in asymptotic efficiency relative to 2SLS in over-identified models (Baum, 2006), and Table 4, thus, presents IV-GMM estimates correcting for endogeneity and omitted variable bias in the OLS estimates from Table 2.

Protest violence is found to be negatively related to the incidence of concession, with the estimates being highly significant. In Column 1 of Table 4, the point estimate on the violence variable is -0.16 (standard error of 0.05), with better than a 99% confidence level. This means that going from a protest characterized by no property destruction at all to one characterized by property destruction on every day of the protest lowers the probability of concession by almost 20% on average. Since we have instrumented for protest violence we interpret this negative relationship between protest violence and the incidence of concession as a causal one.[11] In column 2 we add time trends and in column 3 we present the results from

[11] Obviously, such inference is conditional on the validity of our instruments, something discussed further

adding other explanatory variables (*Size*, *Duration*, *Paris*, and *Election*). It is common in the social movements and protest violence literature to include other protest characteristics such as these in the regression to assess their relative influence on protest outcomes. The degree to which these additional explanatory variables are actually appropriately considered exogenous control variables varies, as some may be more reasonably considered exogenous than others. In general, we think such variables may not be properly considered control variables and they are included here (with expanded results in the Appendix) primarily for comparability with existing work. In any case, as can be seen, adding these additional explanatory variables as controls does not dramatically change the estimate on *Violence*.

The large difference in the size of the IV and OLS estimates suggests that the OLS estimates are afflicted by endogeneity and omitted variable bias, as suspected.[12] Some omitted variable or endogeneity bias stories can be ruled out as dominant, while others are consistent with the results. For instance, unobserved negative economic shocks may be presumed to increase the tendency to resort to violence by protesters (if opportunity costs of violence are lowered or police budgets have been cut under austerity policies, for instance) while also leaving firms or government less likely to reverse layoffs or program cuts. However, this downward bias of OLS results is inconsistent with the more negative coefficient on $Violence$ in the IV results relative to OLS. On the other hand, unobserved weakness or divisions among the governing authority would predict an upward bias on the OLS estimates, as protesters may be more likely to resort to violence and authorities may be more likely to capitulate independent of the violent tactic employed. One can think of other channels of bias that are similarly consistent with the findings.

later in this section.

[12]The possibility that the IV estimates reveal and correct for attenuation bias on the violence measure in the OLS specifications presumes the existence of classical measurement error in the underlying property damage incidence variable used to construct the $Violence$ variable, something that is not the case given that this variable is dichotomous.

B Considering the Exogeneity Assumption and Exclusion Restriction

The credibility of the IV estimate for violence depends on the degree to which we believe our instruments are exogenous. The argument for the exogeneity of the weather instruments strikes us as obvious.[13] However, the exogeneity of the school holiday instrument, while intuitively plausible, may stand on weaker ground. Though the school holiday calendar is chosen years in advance of time-t protests out of consideration of factors entirely independent of the current events motivating protests and their success at time t, it is possible that those engaged in the protest-concession dynamic make their strategic decisions with the school calendar in mind, causing possible violations of the exogeneity assumption. For instance, policy makers, anticipating either differential violence or youth participation in protest, may schedule the most controversial policy changes in accordance with the school calendar. If these controversial policy changes are also those which policy makers are most intent on implementing, yielding the lowest chance of successful protest, then there may indeed be a violation of the assumption that the school holiday calendar is independent of potential outcomes. Alternatively, protest organizers, with advance knowledge of the local school calendar, may shift the day of their protest by a few days to affect the composition of the protest and this decision may be correlated with underlying perceptions of the incidence of concession.

Such concerns are most plausible with respect to education policy changes, an issue of protest most likely to engage students. Column 1 in Panel B of Table 5 includes the first-stage results from an IV regression that excludes protests in which the issue is education.[14] The first-stage results are unchanged from the base sample, and, as can be seen in the corresponding column in Panel A of Table 5, the IV estimate on violence is also virtually unchanged.

[13] All rain dances aside.

[14] We thank Jonah Levy for fruitful discussions about the nature of French youth participation in protest.

Column 2 in Table 5 reports the first-stage and IV estimate without use of the school holiday instrument. Using only rainfall and temperature instruments we continue to find a highly significant (over 99 percent confidence) negative IV estimate of violence on the incidence of a concession (the violence coefficient increases in size to -0.44). These results give us confidence in the credibility of the baseline negative estimates and serve to minimize concern that strategic agenda setting on the part of policy makers or protest planning in accordance with the school holiday calendar is driving the result, though we can not rule out with absolute certainty the possibility that the *Holiday* instrument may not be completely exogenous.

The fact that there is not comparable advance knowledge of, or certainty about, future weather conditions as there is with the school calendar, makes the exogeneity assumption with respect to rain and temperature compelling and the strategic concerns raised above with respect to the *Holiday* instrument unlikely to affect our weather instruments. In a pre-social media age, protest organizers need lead time to publicize their protests, and, once a flyer is put up or word is otherwise spread there is no easy way to alert all who have been informed not to attend. Moreover, weather forecasts until very recently had been notably bad.[15] It is also implausible that we are observing reverse causality or a common cause of both rain and the incidence of concession. However, the exclusion restriction must not be violated in other ways, such as by either of the weather variables having an effect on the protest outcome directly or through a channel other than violence. While such possibilities strike us as unlikely, an overidentification test can be performed to attempt to address the concern. Formally, the overidentification test will reject the validity of the weather instruments if one of the instruments has a direct effect on the incidence of concession, or if they effect

[15]For instance, Silver, 2012, recounts how near the start of our data period weather forecasts three days out were off by six degrees Fahrenheit on average. One study regarding weather predictions in this author's hometown (Kansas City) found that when TV meteorologists predicted a 100% chance of rain it failed to rain at all one third of the time, making it unlikely that protest organizers can confidently make plans based on the forecast even if able to call off protests at the last minute.

incidence of concession through another variable than violence, or, if there is not a constant coefficient α_i in the structural equation (1). The p-value for Hansen's overidentification test in Table 5 Column 2 indicates that we can not reject the validity of the instrument (as do the overidentification test p-values in Table 4, with the *Holiday* instrument included). Columns 3 and 4 in Table 5 present an explicit form of the overidentification test. In these columns one weather instrument is presumed to be valid and the other is included as an exogenous regressor to determine if it has a direct effect on incidence of concession. If so, we would expect to see a significant coefficient on the instrument treated as exogenous, but instead, for both instruments their coefficients are very small and statistically insignificant upon their turn being treated as exogenous.

One obvious related, specific concern is that the weather instruments may be affecting the outcome not through their effect on violence but rather through an effect on protest size. It may be that, just as rain and temperature affect violent behavior at a protest they also affect turnout to the protest and impact concession via that channel instead, confounding our preferred causal interpretation. More generally, it is known that the inclusion of a second endogenous regressor as a control may bias the coefficient on the endogenous regressor being instrumented (see, e.g. Appendix A in Acemoglou et al., 2001). The concern is acute when the instruments used for one endogenous variable are strongly correlated with the second endogenous regressor included as a control. In the case at hand, the protest size variable, included above in some specifications, may reasonably be thought to be endogenous for many of the same reasons as protest violence (e.g. an omitted indicator of what policies are perceived as going too far by the public and by elements of the governing coalition; intransigience or weakness of the decision making authority). However, OLS regressions of *Size* on the instruments reveal that there is no significant relationship between the variable and *Temp* and *Precipitation*. Both instruments have very small coefficients (0.0002 for *Temp* and 0.017 for *Precipitation*) that are not significantly different from zero (the t statistics for

Temp is 0.01 and 0.63 for *Precipitation*. Taken all together, this evidence gives us confidence in the view that our instruments are valid (never something that can be proven absolutely), and that we are in fact identifying a negative causal relationship between protest violence and protest success.

VI Implications for Future Modeling

While the contribution of this paper is primarily empirical, the findings also shed light on the appropriateness of certain assumptions in modeling protest violence. We will now briefly discuss what the negative estimate on violence implies for future modeling of violent protest.

The obvious question raised by the negative finding is why so many protesters still choose to engage in costly violent protest if resorting to violence lowers their chance of success? Is such a decision irrational? Seen from the perspective of a basic two-party bargaining model with the protester as a unitary actor with rational expectations using violence to win concessions, this conclusion makes sense: if fully informed about the expected probability of gaining a concession, protesters would be acting against their interest to resort to violent tactics that in fact lower their chance of achieving their aims.

However, one need not throw away the assumption of rational actors to explain the findings. If instead of hewing to a unitary rational actor model, we allow for two types of protesters and the existence of political agency problems the negative result can be given a rationalist explanaion. For instance, some protest participants may only partly share the objectives of protest organizers. In addition to deriving utility from exacting a concession, these protesters may additionally derive utility from the looting so often associated with protest violence, thus, putting their personal gain from the spoils of rioting in conflict with the collective loss experienced from protest violence lowering the prospect of achieving a concession for the issue under protest.

The motivational heterogeneity among participants in political violence has already been emphasized in previous work. Weinstein (2005, 2007), for instance, makes the distinction between opportunistic and ideological joiners to armed rebel groups, divisions which, to a lesser extent, almost certainly exist in sub-military political protests as well. Powell (2006) explains the decision to fight rather than to agree to a settlement by going beyond unitary actors, allowing one side to be composed of competing factions that can not commit themselves to a future division of their side's gains. In work in progress by this author, we sketch a simple signalling model that employs a biforcation in the protester types, or, alternatively, in the perception of protester types held by the stakeholders to whom the protest target answers (the voting public, shareholders). The upshot is that with complete uncertainty about protester type, the government or firm will react to violence with no concession (and to non-violence with a concession) when stakeholders sufficiently value concessions to nonviolent protesters whom they perceive as principled, and, the net private gain to violence for a criminal type of protester outweighs his net loss in the protest-issue-space from a failed but violent protest (relative to what could have been won via a concession).

Of course, one does not need to abandon a unitary actor model to explain the results if willing to allow for agents governed by "behavioral" features such as emotion or overconfidence. Introducing emotion, for instance, into agent's decision-making goes against the basic rationalist model whereby decision making is seen as a cognitive process in which "decision makers [are] assumed to evaluate the potential consequences of their decisions dispassionately and to choose actions that maximize the "utility" of those consequences" (Loewenstein and Lerner, 2003). This explanation for why political violence occurs would diverge from the tradition in political science, influenced by Schelling (1960), which sees recourse to violence as a strategic choice undertaken to coercively increase the chance of victory. While in interstate or insurgent-state conflicts the strategic model may fit, the non-hierachical nature of most protests and their diverse and open participation admits for participants who, unlike

leaders of states or armed forces, have not been selected for their ability to successfully deliver results or maximize societal, or factional, welfare functions. This would seem to allow for a greater role for "behavioral" participants. Emotion can be incorporated with otherwise rational elements (see for example, Passarelli and Tabellini, 2013) and it need not be the only behavioral element admitted. Indeed, the same selection effect referenced might allow those leading states or armies to have a better grasp of their odds of victory, while protests of the masses may well include participants who are overconfident in their assessment of their likelihood of success or in their assessment of how the protest target (and its constituents) will respond to the tactic of violence.

The above explanations parallel some of the those offered by Fearon (1995) in his typology of the explanations for the inefficiency puzzle of war. Just as in that instance, it is also possible when explaining protesters' recourse to ineffective violence to leave aside either of the above explanations, and, to instead envision explanations that involve a strictly rational *and* unitary actor. For instance, it may be that protesters are choosing violence to maximize their returns over their life cycle. Even if violence loses them the chance of concession in the current period it may decrease the chance in the future that additional hostile moves are taken against the protesters by the protest target. This may make sense if facing a target who does not want to be seen as weak from capitulating to protester coercion, and, so, who will not reverse the policy under protest, but, who will be less likely to enact similarly controversial policies in the future out of fear of additional violent reprisal. Thompson's (2003) study of French agricultural protests by Coordination Rurale and other farmer groups opposed to EU trade and agricultural policy in the 1990s - protests that often led to violence in our data set - is consistent with this reading. It is also possible that the lower chance of getting a win with violent protest is compensated for by a more generous split of the pie in the event that one does win. Such an explanation could be ruled out by more fine-grained measures of outcome variables (going beyond simple binary measures of success). Distinguishing between the

competing explanations will need to be done in future work that pays more careful attention to formal modeling and the collection of higher-quality data for empirical tests.

VII Conclusion

In this paper we attempt to address a main methodological problem in work that studies the efficacy of political violence, namely, the possible endogeneity of violence. We aim to go beyond previous correlational studies that yield conflicting conclusions about whether turning to violence pays off for protesters, and, we identify a causal effect of protest violence on the incidence of concession made to protesters in the short run. To do so, we use instrumental variables methods, with rain, temperature, and school holidays as instruments for the degree of violence, so as to identify plausibly random-like variation in violence and avoid omitted variable and endogeneity bias. We find a significant and negative relationship between property destruction associated with protests and the chance of near-term success in changing policy, and we interpret this IV estimate as indicative of a negative causal effect of protest violence on the incidence of concession. Our IV point estimates are significantly larger in magnitude than those from a comparable OLS regression, indicating bias from endogeneity or omitted variables in OLS specifications.

We hope this work demonstrates one avenue by which the study of the effects of political violence can be pursued with careful attention to credible causal identification. We readily admit that "a single estimate is unlikely to provide a definitive and comprehensive basis for informing policy" and what we need is multiple studies based on different populations and different settings to understand the causal mechanisms at work (Imbens, 2009). We are optimistic about the applicability of this particular identification strategy to other countries, as the mechanisms by which our instruments are assumed to affect violence are fairly general. The rain and temperature instruments are presumed to affect violence primarily through

physiological processes that we would expect to be fairly universal (though there may be some variation in the range of sensitivity to bodily temperature changes at different latitudes). The holiday instrument similarly rests on the general tendency of youth to be more violent than average. The extent to which this is true will undoubtedly vary from culture to culture, but as long as youth are at least somewhat more violent than average then school holidays may prove to be a useful instrument in other contexts. More of a challenge to cross-country application of the holiday instrument is the degree to which compulsory schools laws are implemented and enforced: in areas where enforcement against truancy is so lax that many students (possibly the students most likely to engage in property violence) do not regularly attend school, there will be little likelihood of a strong correlation between the nominal days off from school and broken windows at a protest. Thus, the current identification strategy will be easiest to replicate in nations like France, with obligatory participation in secondary education and strict enforcement of truancy laws. Youth participation in protest is also very high in France and the strength of the school holiday instrument also rests on the degree to which the young participate in protests when not incapacitated by school. Existing work suggests that France is not anomalous in this respect (see Resnick and Casale, 2011, for a survey of African nations).

While the identification strategy may be generalizable, it is an open question as to whether the negative estimates for protest violence will be observed in other countries. While many have noted the unusually high frequency of protest in France (something confirmed by cross-country comparisons over the same time period in EPCD data on other European nations), it is unclear that French protest targets should respond uniquely negatively to violent protest. In fact, if indeed protest targets choose how to respond to violent protest on the basis of whether these targets' constituents perceive the protesters to be in the right, we may actually see even larger negative results in countries such as the United States where cultural attitudes towards protest and especially violent protest are more hostile than in France and

where there is little to be gained from conceding. In addition to culture, institutions and forms of government also may play a critical role in mediating the response to violence. While non-democratic regimes may be thought to be most concerned with being seen as weak (and so may refuse to concede in the face of violence), in relatively democratic France we find that concessions are also not often granted in the face of protest violence. In weakly institutionalized environments where exerting political influence via established channels of voting and lobbying is less common, and where there is less disapproval for going outside these channels, we may actually see violence playing a more effective role in bargaining. To test any comparative differences more and better data is needed. Especially important for future micro-empirical analysis is the creation of comprehensive data sets that attempt to record not only the characteristics of protest, but, that also do the hard work of recording protest outcomes, both in the short run and long run. As in many cases, these data improvements will enable more sophisticated empirical - and we hope causal - analysis.

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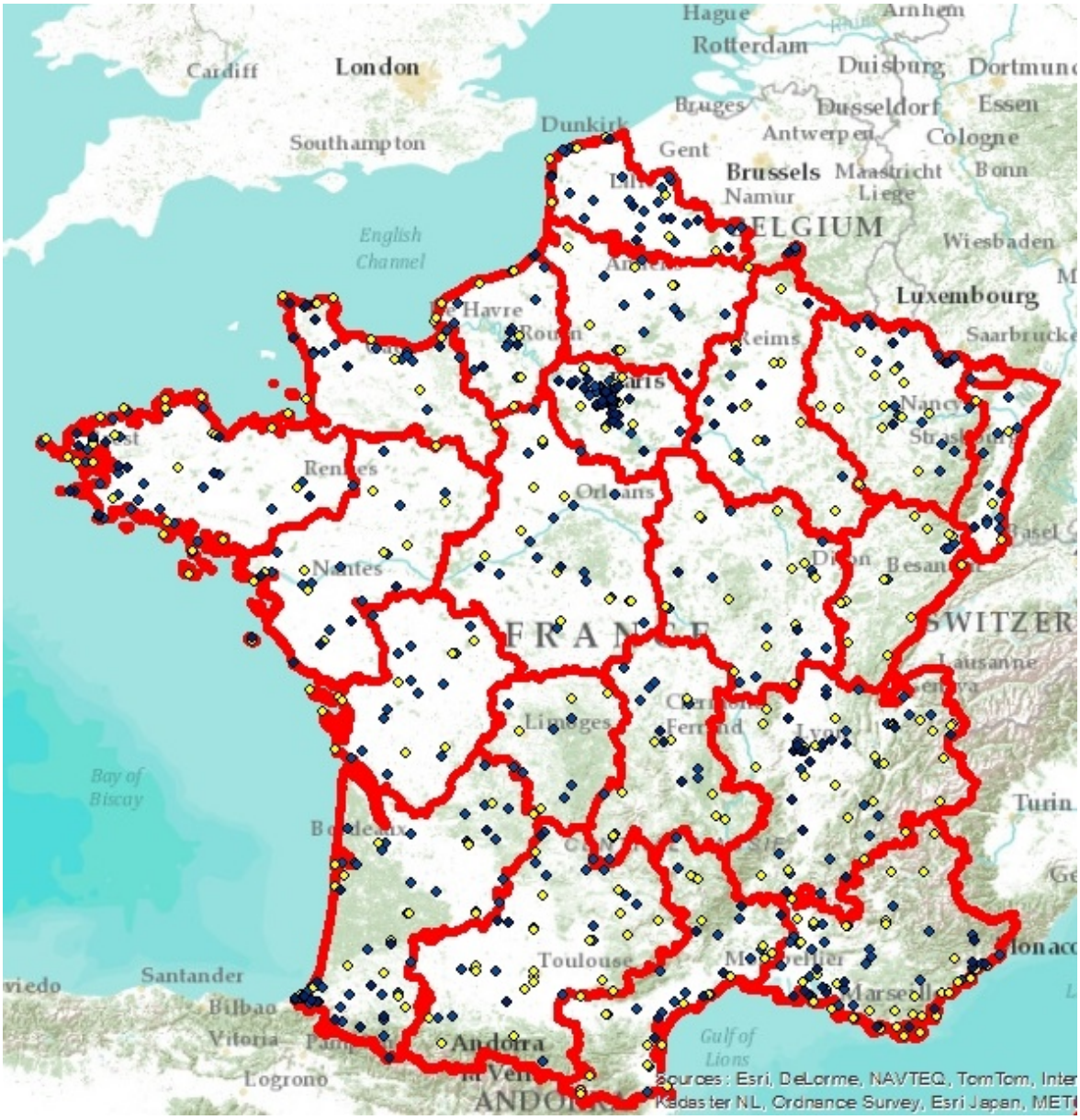

Figure 1: Plot of unique protest locations (in blue) and weather stations

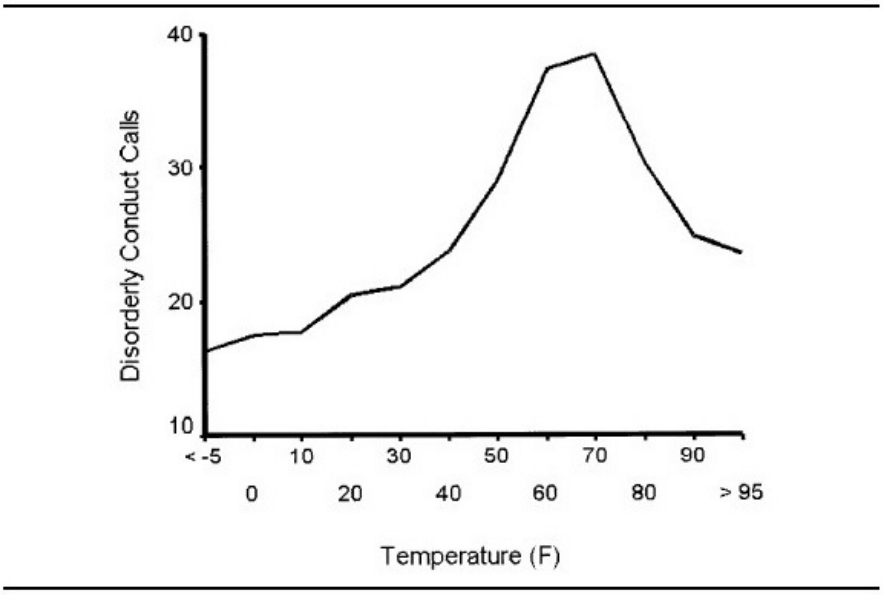

Figure 2: Disorderly Conduct Calls as a Function of Temperature

Figure 2: Disorderly Conduct Calls as a Function of Temperature (reprinted from Rotton and Cohn, 2000a)

Table 1: Descriptive Statistics

	Mean	**Std. Dev.**	**Observations**
Outcome			
Concession Made to Protesters	0.021	0.142	2087
Violence			
Violence (% violent days of protest)	0.182	0.371	2087
Instruments			
Protests with Rain on Some Day	0.51	0.5	2087
Protests with Avg. Temp. Above 60 and Below 75	0.338	0.473	2087
% of Days in Protest on School Holiday	0.189	0.38	2087

Note Violence is measured as the fraction of newspaper reports on a given protest throughout the protest's span that report the occurrence of property damage.

Table 2: Protest Violence and Concession to Protesters (OLS Results)

Dependent Variable: Incidence of Concession			
	(1)	(2)	(3)
Violence	-0.0261***	-0.0247***	-0.0222***
	(0.00755)	(0.00528)	(0.00543)
Time trends	no	yes	yes
Additional Explanatory Variables	no	no	yes
adj. R^2	0.004	0.009	0.025
N	2087	2087	2087

Notes: Violence is measured as the fraction of newspaper reports on a given protest throughout the protest's span that report the occurrence of property damage. Additional explanatory variables include the number of newspaper reports on a given protest during the protest's span indicating the protest on that day took place in Paris. Robust standard errors are reported in parentheses, adjusted for protest target level clustering. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Weather, School Holidays and Protest Violence (First-Stage)

Dependent Variable: *Violence* (% violent days of protest)			
	(1)	(2)	(3)
Precipitation	-0.0538***	-0.0621***	-0.0520***
	(0.0135)	(0.0124)	(0.0148)
Temp	0.0357**	0.0353**	0.0347**
	(0.0167)	(0.0162)	(0.0151)
Holiday	0.0754***	0.0709***	0.0549**
	(0.0240)	(0.0240)	(0.0242)
Time trends	no	yes	yes
Additional Explanatory Variables	no	no	yes
adj. R^2	0.012	0.031	0.065
Robust F	19.12	23.07	13.30
N	2087	2087	2087

Note: Standard errors are reported in parentheses, adjusted for protest target level clustering.

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

Table 4: Effect of Protest Violence on Concession (IV-GMM Results)

Dependent Variable: Incidence of Concession			
	(1)	(2)	(3)
Violence	-0.162***	-0.157***	-0.145**
	(0.0506)	(0.0590)	(0.0621)
Time trends	no	yes	yes
Additional Explanatory Variables	no	no	yes
p-value (from chi-squared test)	[0.18]	[0.19]	[0.30]
N	2087	2087	2087

Note: Standard errors are reported in parentheses, adjusted for protest target level clustering.

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

Table 5: Weather-only Instruments and Overidentification Tests

Panel A: IV-GMM (Dependent Variable: Incidence of Concession)

	(1)	(2)	(3)	(4)
	No Educ.	Weather only	Temp as Exog.	Prcp as Exog.
Violence	-0.141**	-0.444***	-0.490***	-0.277**
	(0.0612)	(0.153)	(0.159)	(0.129)
Temp as Control			0.00695	
			(0.00671)	
Precipitation as Control				0.0135
				(0.00869)
p-value(chi-squared test)	[0.19]	[0.29]	–	–
N	2045	2087	2087	2087

Panel B: First Stage (Dependent Variable: *Violence*, % violent days of protest)

Precipitation	-0.0612***	-0.0631***	-0.0631***	-0.0631***
	(0.0125)	(0.0126)	(0.0126)	(0.0126)
Temp	0.0355**	0.0326**	0.0326**	0.0326**
	(0.0164)	(0.0161)	(0.0161)	(0.0161)
Holiday	0.0720***			
	(0.0240)			
adj. R^2	0.031	0.026	0.026	0.026
Robust F	24.68	24.67	25.03	4.08
N	2045	2087	2087	2087

Note: Standard errors are reported in parentheses, adjusted for protest target level clustering. Columns 3 and 4 report results from regressions in which *Temp* and *Precipitation* are included as exogenous variables, respectively, while using the other variable as the instrument for *Violence*. All regressions also include time trends.

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

IX Appendix

Table 6: Expanded OLS, IV, and First-Stage Results with Additional Variables

	(1)	(2)	(3)
	OLS	IV-GMM	First stage
Violence	-0.0222***	-0.145**	
	(0.00543)	(0.0621)	
Size(>1000)	-0.00657	-0.0377***	-0.151***
	(0.00421)	(0.00898)	(0.0290)
Duration	0.000656***	0.000664***	-0.000700***
	(0.000131)	(0.000177)	(0.000123)
Election	0.00401	0.0173	-0.0313
	(0.0153)	(0.0113)	(0.0333)
Days in Paris	0.00184	0.00228***	-0.00252***
	(0.00118)	(0.000619)	(0.000786)
Precipitation			-0.0520***
			(0.0148)
Temp			0.0347**
			(0.0151)
Holiday			0.0549**
			(0.0242)
Time trends	yes	yes	yes
adj. R^2	0.025	.	0.065
N	2087	2087	2087

Note: Standard errors are reported in parentheses, adjusted for protest target level clustering

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