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Do Politically Irrelevant Events Cause Conflict? The Cross-continental Effects of European Professional Football on Protests in Africa

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Abstract

We examine whether politically irrelevant events can cause conflicts, by analyzing the effects of professional football in Europe on protests in Africa—an unintended spillover across the continents. By expanding psychological theories, we argue that the outcomes of the football games in Europe can affect African people's subjective evaluation of domestic politicians, which can in turn trigger protests. A regression discontinuity analysis of 15,102 close football games (2005-2019) reveals that a close loss of a European football team to which an African player belongs nearly doubles the rate of protest in his home country. The effect is particularly large for non-ethnic protests targeted at a central government. Moreover, people who are interviewed immediately after a close loss express 23% less trust in his/her country's leader on average. By contrast, close victories do not have equivalent or compensating effects on protests or public opinion. These results suggest asymmetric misattribution; people in Africa blame domestic politicians and protest for the bad luck in the European football games, while they do not credit politicians or eschew protesting after victories.

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JEL classification: D74, D91, Z29

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Karl Marx said "Religion is the opiate of the masses." He was wrong. It's sports.

Bill Scharrer (2017)

In recent decades, professional football in Europe has emerged as the "second religion" in Africa. In the words of Musa in Nigeria, "European football has become part of my life, it is more than just a source of entertainment but it's like my second religion" (Ka, Od, and I 2018, 8). Another Arsenal fan, Jude, does not even deny the risk of escalating violence; "What often makes me angry in football centers [places for watching European football broadcasts] is the insult and unnecessary mockery. There are times people will tell you something that will annoy you just because their team beat yours. … this kind of negative words sincerely stir anger in one, and I believe it can cause physical fights one day" (ibid, 7). Clearly, the outcomes of the football games played over 5,000 kilometers away have a profound impact on the lives of Nigerians. Could this "second religion" cause conflicts—something more than just brawls—in Africa? If so, by what mechanisms?

We answer these questions by analyzing whether transitory psychological cues—the results of professional football in Europe—can affect conflicts in Africa. In doing so, we challenge previous studies on conflict, which have largely ignored the roles of politically irrelevant factors like sports events until recently (Bertoli 2017; Depetris-Chauvin, Durante, and Campante 2020). Most of the standard frameworks—including grievance theses (Gurr 1970), contentious politics frameworks (McAdam, Tarrow, and Tilly 2003), collective action theories (Tullock 1971), and bargaining models (Fearon 2004)—give the impression that politically irrelevant events should not matter because they would not obviously affect costs, benefits, information, or outcomes of conflict. We, however, argue that the psychological cues do affect them.

To this end, sports events provide unique analytical advantages. Other dramatic events, like natural disasters, are highly influential. However, disasters often provide citizens with objective information about the government's performance in mitigatory and response policies (Ashworth, Bueno de Mesquita, and Friedenberg 2018).[1] Thus, even without any effects from psychological cues, natural disasters can affect information, rational choices of citizens, and hence conflicts. By contrast, as Ashworth et al. (2018) acknowledge, local sports events provide no objective information about politicians' competence (Healy, Malhotra, and Mo 2010; 2015; M. K. Miller 2013; Fowler and Montagnes 2015; Busby, Druckman, and Fredendall 2016; Busby and Druckman 2018; Graham et al. Forthcoming). The political irrelevance of local sports events allows us to isolate the effects of psychological cues from those of rational updates.

Moreover, unlike Card and Dahl (2011) and related studies on the economic and social consequences of sports events,[2] we analyze a politically, geographically, and causally distant relationship—football in Europe and conflicts in Africa. While the literature focuses on domestic

[1] See Achen and Bartels (2004; 2018), Malhotra and Kuo (2008), Healy and Malhotra (2009; 2010; 2013), Fowler and Hall (2016), and Ramos and Sanz (2020) among others. Other studies examine Christmas lotteries (Bagues and Esteve-Volart 2016), sunshine (Bassi 2013; 2019), and foreign aid reception (Cruz and Schneider 2017).

[2] Previous studies analyze the effects of sports on judicial judgements (Eren and Mocan 2018), electoral turnout (Potoski and Urbatsch 2016), stock returns (Edmans, García, and Norli 2007), sexual assaults (Lindo, Siminski, and Swensen 2018), hooliganism (Wann et al. 2001; Priks 2010), crimes (Rees and Schnepel 2009; Munyo and Rossi 2013; Kalist and Lee 2016; Marie 2016; Ge, Barbieri, and Schneider 2021), and unhealthy eating (Cornil and Chandon 2013) among others.

relationships, the effects of "local" sports events are rarely exclusively local; hundreds of millions of people in Africa, for instance, breathlessly watch football games held in Europe. Given the globalization of the sports industry, it is imperative to understand the intercontinental spillover of major sports events.

Last but not least, unlike recent studies about big international sports events such as the World Cup and the Africa Cup of Nations (Bertoli 2017; Depetris-Chauvin, Durante, and Campante 2020; Rosenzweig and Zhou forthcoming), we focus on European professional football to ensure the political irrelevance of the events. Because politicians in Africa can affect the results of the international competitions via budgetary allocations and sports policies (e.g., selection of managers and coaches), international sports events can potentially provide objective information about their competence. By contrast, African politicians have no meaningful influence over the results of professional football in Europe. This means that if the professional football games in Europe affect conflicts in Africa, the reasons must be something other than rational updates—such as emotional factors.

One such factor is moods (Hirshleifer and Shumway 2003; Meier, Schmid, and Stutzer 2016); losing matches can affect transitory emotional statuses, causing anger, distress, shame, sadness, and anxiety. People can join protests to vent the negative moods. Conversely, people may bask in the glory of their team's victory, having a positive feeling of elation. Another factor is misattribution (Achen and Bartels 2004; 2018; Fowler and Hall 2016); people can directly or indirectly attribute the "bad luck" in the sports games to their politicians and thus stage protests. By contrast, after the victory of favored teams, people may give underserved credit to politicians. Finally, the collective experiences of victories and losses can unite football fans across ethnic cleavages, which may halt conflicts, especially those related to ethnic issues (Alrababah et al. 2019; Depetris-Chauvin, Durante, and Campante 2020; Mousa 2020).

We examine these mechanisms by conducting two sets of empirical analyses. In the first analysis, we identify the causal effect of game results on protests in Africa by exploiting as-if random variation in the results of the close football games conditional on pre-game betting odds (Card and Dahl 2011). The analysis of 15,102 close games and 40 African countries reveals that a close loss of a team to which an African player belongs increases the likelihood of protests in his original country, while a close win has little to no effect. The effect of close losses is particularly large for non-ethnic protests targeted at a central government.

Once we establish the causal relationship, we dissect the mechanisms by applying a regression discontinuity design (RDD) to individual-level surveys. By exploiting as-if random coincidences of survey interviews and football games, we show that the close loss of a football team to which an African player belongs makes people in his original country less supportive of their political leaders, while winning or losing games have nearly zero effect on people's moods. Overall, these findings, as well as the extensive analyses about effect heterogeneities and robustness, provide credible evidence about the effects of psychological cues—and, more specifically, the misattribution mechanism.

Case: Africa and European Professional Football

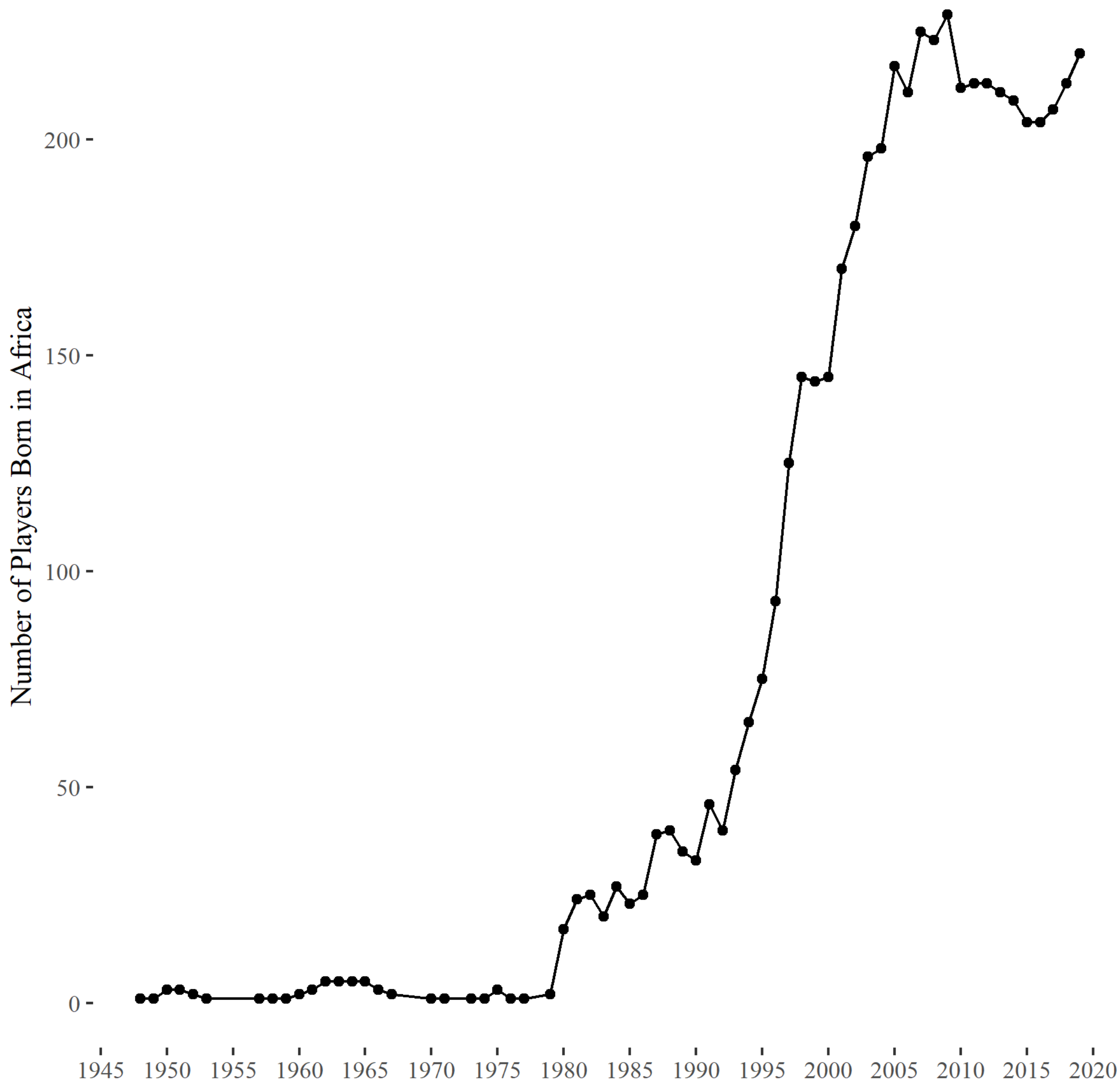
European professional football is an important entertainment in the daily lives of Africans. Partly due to the relative lack of alternatives, soccer is overwhelmingly the most popular sport in Africa (Kombol and Kombol 2015; Ka, Od, and I 2018). This feature distinguishes it from Asia, in which other sports are also popular, and Latin America, which has vigorous domestic leagues. According to a survey conducted in 2011, 71% of people in Africa have an interest in soccer (Malik 2021).

And around 276 million people in Sub-Saharan Africa—about a quarter of the entire population—regularly watch the Premier League (Monks 2016). Moreover, as the anecdotes at the beginning of this paper reflect, many Africans are devoted fans of European professional football. As described by Ka et al. (2018), "the European football fellowship is a new 'religion', another kind of 'ethnicity', and other form of 'ritual'" (page 2). Due to the absence of cable networks and the costs of quality display devices, most Africans use so-called viewing centers, at which they pay entrance fees to view live football games on satellite television, or the internet after the 2010s. Because of the zealous fandom, ubiquitous coverage on satellite television, and relatively cheap entrance fees, viewing centers have spread across the continent since the late 1990s, even to rural villages (Kombol and Kombol 2015).

The fandom's rapid growth is the flip side of the increasing presence of African players in European football. While only one African player was present in the top-five leagues in 1970, the number exploded to 217 in 2005 (Figure 1). For the period of our analysis (2005-2019), 1,050 African players are in the top-five leagues, 79% of the games have at least one African player, and two players per game originally come from Africa. Among those players, 62% come from Western Africa, followed by Eastern (24%), Northern (11%), and Southern Africa (3%).[3] Similarly, 47% of the players belong to Ligue 1, followed by Serie A (16%), Premier League (14%), Bundesliga (12%), and La Liga (11%). These patterns reflect soccer's overwhelming popularity in Western Africa (see Ungruhe and Schmidt 2020), and that the majority of Western African countries are former French colonies.

[3] In Appendix 1, we also provide the number of players from each country.

Figure 1. Rapid Increase of African Players in European Professional Football



NOTE: The figure shows the numbers of players who are born in Africa and belong to teams in the top-five European leagues.

In this paper, we focus on the results of a football team to which an African player belongs. Even though people can support teams without African players, the presence of co-national players is a strong indicator of fandom. For example, the popularity of Chelsea in Cote d'Ivoire is often attributed to the presence of national players, such as Didier Drogba and Solomon Kalou (Kwenda 2015). Similarly, the presence of Nwankwo Kanu also is said to make Arsenal popular in Nigeria (ibid). Because this is an important assumption in the interpretation of our findings (though we cannot come up with compelling alternative interpretations), we empirically test the relationship in Appendix 2. We find that the presence of co-national players is indeed a significant predictor of fandom. Later, we also check the robustness by analyzing players' appearances in games.

The Psychological Origins of Social Conflicts

We argue that the results of a football team to which an African player belongs provide a psychological cue to people in his original country that affects the likelihood of conflict. To be clear, we do not argue that football games are likely to cause full-fledged civil wars, which depend on organizational and strategic dynamics. However, less organized conflicts like protests (including peaceful demonstrations and violent riots, which are both *social conflicts*. Social conflicts are one type of conflicts; see Salehyan and Hendrix 2017) depend on individuals' participation and are hence subject to people's cognitive biases. In fact, a sizable number of protests are small-scale and thus do not require collective action (Harris and Hern 2019); among all protests occurring in Africa for 1990-2017, 37% involve less than 100 participants, and an only smaller fraction (33%) involve more than 1,000 participants (Salehyan and Hendrix 2017).

Importantly, the psychological explanations are complementary with standard theories of protests. For instance, deviant behaviors of even just a few individuals can change the strategic calculus of other rational people or organizations by resolving collective action problems, and thus spark large-scale protests. As Kuran (1991) states, the participation of a few deviant people can marginally improve the prospect of a protest and thus induce other rational people to join the protest, which in turn may induce the participation of yet other people. This chain of bandwagoning can quickly snowball into unexpectedly large protests (see Bikhchandani, Hirshleifer, and Welch 1992; Chenoweth and Belgioioso 2019). Similarly, the results of football games can provide opposition leaders and organizations with emotional cues, giving them opportunities for protest mobilization (McAdam, Tarrow, and Tilly 2003). Thus, even though psychological cues may or may not affect armed conflicts specifically, they can cause both small- and large-scale protests.[4]

Null Expectation: Rational Update

From the perspective of a standard *rational update* model (Lohmann 1993; Ashworth, Bueno de Mesquita, and Friedenberg 2018), the results of the professional football games should not affect protests in African countries because they do not change material payoffs or provide any new information about the competence of domestic politicians. Any effect sports and education policies might have on the performance of African players in the European leagues would only become meaningful over decades. This means that those policies do not immediately reveal the politicians' competence; any immediate effect of European professional football should be explained by reasons other than the rational update.

Mechanisms 1: Mood

A straightforward explanation is that the results of the football games influence moods—i.e., transitory states of feelings (Hirshleifer and Shumway 2003; Meier, Schmid, and Stutzer 2016)—which in turn affect the likelihood of protests. Upon the defeat of a favored team, people can lose excitement and confidence, and feel sad or even angry. Although people can vent their negative moods through various means (e.g., drinking, gambling, crime, and violence),[5] joining protests

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[4] This certainly does not mean that the effects of European football would be limited to protests; as numerous studies show (see footnote 2), sports events have broad consequences. Our argument, however, is that the sports events can affect even costlier political actions—protests.

[5] See footnote 2 for previous studies.

can also alleviate them (Jasper 2011). Conversely, the victory of their team can relieve people's negative moods, provide positive feelings of enthusiasm and elation, and hence halt protests. Thus, the *mood mechanism* predicts that the loss (win) of a team to which an African player belongs increases (decreases) protests in his home country.

The literature on social psychology, however, implies a potential for asymmetric effects. The basking-in-reflected-glory and cutting-off-reflected-failure theses (Cialdini et al. 1976) state that people tend to associate themselves with a glorious outcome of their favored team and thus obtain positive moods, while they tend to cut such an association to maintain their mental statuses when their favored team experiences a failure. The *asymmetric mood mechanism* therefore predicts that winning games give euphoria and hence reduce protests, while losing games have no effect.

Mechanisms 2: Attribution

Other (not mutually exclusive) mechanisms are based on misattribution—i.e., unjustified blaming and crediting. While the mood mechanism primarily pertains to emotion, the attribution relates to information processing. In its simplest form (*direct* attribution), people can attribute bad luck in football games not only to the players, coaches, and managers in Europe but also, unreasonably, to domestic politicians (correspondence bias in Gilbert and Malone 1995). Although the direct misattribution may or may not be realistic, the attribution can also be *indirect* (blind retrospection in Achen and Bartels 2004).[6] Because contemporary political processes are complicated, people often use their transitory subjective welfare as heuristics to evaluate the performances of politicians. From this perspective, losing matches can hurt the subjective welfare of people, which in turn can be consciously or unconsciously attributed to politicians and thus lead to protests. By contrast,

[6] See footnote 1 for debates about Achen and Bartels (2004).

upon the victory of favored teams, people can give underserved credits to their politicians (Healy, Malhotra, and Mo 2010) and pardon politicians' objective incompetence. The *blind attribution mechanism* predicts that losing games trigger protests while winning games curb them.

The social psychology literature implies asymmetric effects. The success-failure bias (D. T. Miller and Ross 1975) refers to the human tendency to blame others for failures (even when they are not responsible for the failure) while attributing successes to his/herself (even when s/he is not responsible for the success). People may then perceive their favored team's victory as a personal success and thus not credit politicians or refrain from protesting, while they directly or indirectly attribute their favored team's losses to domestic politicians and join protests. The egoistic attribution, which we call *asymmetric attribution mechanism*, suggests that only losing games cause blaming and protests, while winning games do not improve people's attitudes toward politicians or reduce protests.

Mechanisms 3: Identity

Finally, the shared glory or misery with co-national players may unite the fandom under the national identity, which in turn may make existing ethnic cleavages less salient and halt protests, especially those related to ethnic issues (Alrababah et al. 2019; Depetris-Chauvin, Durante, and Campante 2020; Mousa 2020). As the rally-around-the-flag effect suggests (Mueller 1970; Ramos and Sanz 2020), both collective glory and suffering unite people. The *rally mechanism* predicts that both winning and losing games decreases (ethnic) protests. Table 1 summarizes the causal mechanisms.

Table 1. Causal Mechanisms and Predictions

	Mechanism	Predicted Effect on Protests	
		Loss	Win
Information	Rational update	0	0
Mood	Mood	+	–
	Asymmetric mood	0	–
Attribution	Blind attribution	+	–
	Asymmetric attribution	+	0
Identity	Rally	–	–

NOTE: 0, +, and – indicate no, positive (higher likelihood of protests), and negative (lower likelihood) effects respectively.

Event Data Analysis: Research Design

We test the hypotheses by conducting two sets of empirical analyses—an event data analysis at a macro level and a survey analysis at the individual level. An empirical challenge is, however, that the results of European football games are not always randomly assigned. For instance, better sports and education policies may be associated with both more star players from a country and social stability. The endogeneity might create a spurious correlation. We address this problem by applying an RDD in both analyses; that is, we compare a close win, draw, and loss of an African player's team. Because the fates of close games are easily swayed by random chance, such as shots that hit the post and own goals, the treatment assignment should be closer to as-if random.

A problem is, however, that compared to other sports, the scores are less frequent in soccer. As a result, one score can mean a big difference, and thus even winners and losers of close games can be systematically different. In fact, even though we compare the narrowest margins of wins and losses (wins or losses by a one-point margin),[7] the later balance check indicates that the RDD improves the covariate balances but does not do so perfectly.

[7] The games of penalty shoot-out (only in the Champions League) are also included.

In the event data analysis, we therefore follow Card and Dahl (2011)'s well-established approach by conditioning on pre-game bookmaker betting odds (decimal odds).[8] The betting odds are the inverse of expected probabilities of game results (50% of winning $= 2.00$ odds). Thus, as far as the betting odds provide precise predictions of game results, we can directly observe the treatment assignment probabilities and hence identify the causal effect. Our identification assumption is therefore that conditional on the pre-game betting odds, the results of close games are randomly assigned. Substantively speaking, if two games have similar betting odds but their results are different, the difference can plausibly be considered as "unexpected" and thus as-if random. Moreover, because imprecise betting odds can cause large financial losses, bookmakers have strong incentives to accurately measure the betting odds (see Wunderlich and Memmert 2018). In a robustness check, we also limit the comparison to the close games of similar betting odds *and similar numbers of shots on target* to focus on even closer games.

Additionally, we use the difference-in-difference (DiD) specification by taking the first difference of the outcome variable between a few days before and after a football game of a certain result (win, lose, or draw). This eliminates confounders that do not change a few days before and after football games (e.g., reporting biases in conflict data). Because the DiD can potentially overstate the effect sizes (Angrist and Pischke 2009), we also report the results without first differencing and those with lagged dependent variables (LDV) models.

Finally, we account for potential reverse causality (anticipation of protests might affect player performances and violent plays, which in turn could affect game results; Miguel, Saiegh, and Satyanath 2011) as well as alternative interpretation (policies in African countries would affect

[8] See Anderson (2017) for methodological details, and footnote 2 for applications.

performances of African players and thus game results) by controlling for player performances in a robustness check.[9] The remaining variation in the game results comes from the performances of African players' teammates. It is unlikely that the performances of non-African players are affected by conflicts or policies in an African country. Moreover, even though the football games are usually held on weekends, the close-game RDD and conditioning on betting odds well account for the weekend effects. To be sure, we include day-of-week fixed effects in a robustness check.

Sample and Unit

The unit of analysis is a pair of a player i and a football game j. Figure 2 illustrates the basic configuration of our data. Because each player has a single birth country and each game has a single date, we can uniquely define the outcome variable—the incidence of protests in player i's birth country a few days before and after a football game j. Similarly, because a player belongs to a single team on the day of a given football game, the treatment variable—the result of player i's team in a game j—is also uniquely defined. We use a player, instead of his birth country, as a unit to control for individual performances in a robustness check.[10] Although the event data analysis still largely rests on country-level variation and might therefore be subject to aggregation biases, we later supplement it with an analysis of individual-level surveys.

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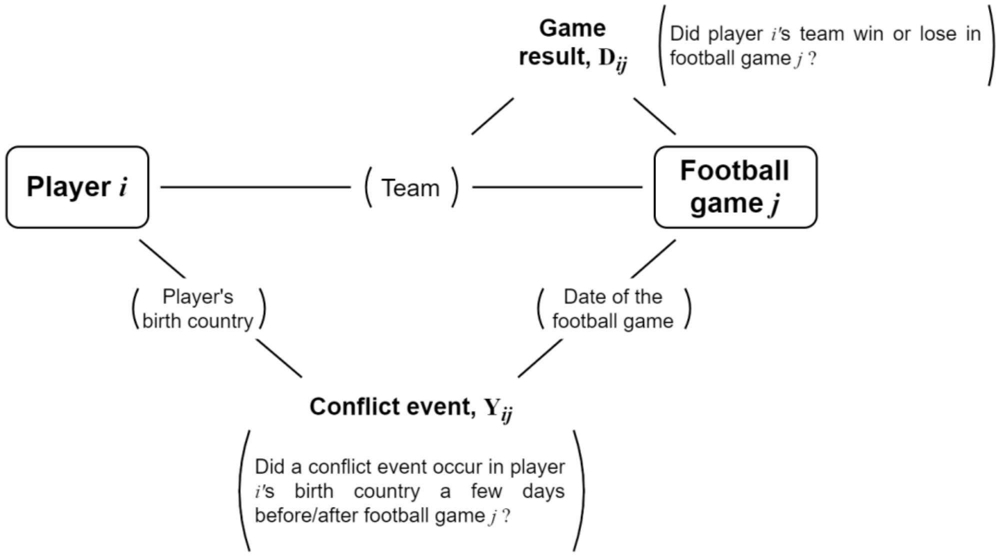
[9] We control for African player's goal, assist, yellow card, and red card.

[10] If multiple players from an African country exist in a single team, each player-game is counted as a separate observation. We account for the repetition by clustering the standard errors. In a robustness check, we also conduct an analysis aggregated at a country-game level.

Figure 2. Illustration of Data Configuration



Our sample includes all players whose birthplaces are African countries, and the football games in the top-five European leagues (Premier League in England, Ligue 1 in France, La Liga in Spain, Serie A in Italy, and Bundesliga in Germany) and Champions League (Group and final stages; only teams from the top-five leagues) between the 2005-2006 and 2018-2019 seasons.[11] In the following analysis, we drop 263 games in which both sides have players from the same African country, and limit the sample to close games.[12] The resulting dataset contains 61,554 observations, including 947 players from 40 African countries in 184 teams of 15,102 close games. For simplicity, we split the sample to that of close losses and draws ($N = 43{,}998$), and that of close wins and draws ($N = 43{,}097$).[13] The summary statistics are shown in Appendix 4.

[11] The betting odds data are available only after 2005, and COVID-19 started in 2020.

[12] We later conduct a robustness check by including non-close games as well.

[13] We split the sample because matching on a trichotomous variable is not straightforward.

Treatment Variable

The treatment variable D_{ij} is a dichotomous variable that is 1 if a team to which player i from an African country belongs loses or wins by a margin of one point in game j and 0 if the outcome is a draw. We code the treatment status based on African players' team affiliations instead of their appearances in games.[14] To be sure, we later conduct a robustness check by splitting the sample to those with and without African players' appearances. The data are scraped from Transfermarket.com on 7 April 2021 (Seidel 2020).

Outcome Variables

The outcome variable ΔY_{ij} is the difference in the daily probability (%) of conflict incidences in player i's birth country before and after football game j: $\Delta Y_{ij} = 100\left(\frac{\sum_{t\in\{1,\dots,T\}} y_{ijt}}{T} - \frac{\sum_{t\in\{-T,\dots,-1\}} y_{ijt}}{T}\right)$, where y_{ijt} is a dummy of conflict incidence in player i's birth country t days after a game j, and T is a time window. We omit the observations on the days of football games ($t = 0$) because the treatment status is indeterminate (hours before a game are controlled, while hours after a game are treated). We use a three-day time window ($T = 3$) because the football games should have only temporary effects and because the three-day time window effectively contains an entire week (i.e., $3 + 1 + 3 = 7$).[15] In robustness checks, we use different time windows.

The incidences of protests are derived from the Armed Conflict Location & Event Data (ACLED 2019) and the Social Conflict Analysis Database (SCAD; Salehyan and Hendrix 2017)—standard datasets of conflict events. For ACLED, we include all demonstrations and riots if the

[14] In Appendix 2, we show that an affiliation of an African player with a team (even without game appearances) significantly increases the number of the team's fans in Africa.

[15] Because football games are held every week, time windows larger than three results in overlaps.

exact date information is available.[16] We also create a separate variable of battles between armed groups for comparison. For SCAD, we collect demonstrations and their types (e.g., sizes, targets, and issues).[17] Although SCAD contains richer information about each event, SCAD substantially underreports the number of events. For instance, while according to ACLED, 9.51% of the observations experienced demonstrations within three days after the football games, the number is just 2.55% in SCAD. Moreover, SCAD does not contain information about the precision of event dates,[18] which is crucial for our daily-level analysis, and the dataset has not been updated since 2018. Thus, we use ACLED in the main analysis and supplement it with SCAD to disaggregate the demonstration events by targets, issues, and sizes.

Although both datasets depend on media reports and are thus subject to reporting biases, "as long as the measurement error is uncorrelated with the independent variables, measurement error in the dependent variable is not particularly problematic in a standard regression framework other than increasing the uncertainty around the estimates we obtain" (Weidmann 2016, 208). Moreover, because we use the first differences of the outcome variables, the reporting biases do not matter unless there are systematic differences a few days before and after a football game.

Control Variable

The data of pre-game betting odds in the top-five leagues are BetBrain's average bookmaker betting odds, which are available at Football-Data.co.uk (Buchdahl 2020). The BetBrain's odds

[16] For the definitions and measurements, refer to ACLED (2019). The exact dates are available for 89% of the protest and riot events.

[17] For the definition and measurement, refer to Appendix 3 and Salehyan and Hendrix (2017).

[18] The event dates in SCAD are the best estimates by the coders.

are averages of twenty bookmaker websites, providing the most consistent data of betting odds for every football game held in the 2005-2019 period. Because the dataset does not contain information about the Champions League, we supplement it with Odds Portal's average betting odds.[19] The treatment assignment probability (%) is calculated as $P_{ij} = 100 \frac{odds_{draw,ij}}{odds_{v,ij}+odds_{draw,ij}}$ for $v \in \{\text{loss}, \text{win}\}$ and game j of player i's team.

Specification

With the directly measured assignment probabilities, we conduct a nearest neighbor matching without replacement.[20] We use the unstandardized caliper size of 5 percentage points (we later conduct robustness checks with different calipers). The matching yields 12,176 losing and draw games (90.83% of the sample) and 11,954 winning and draw games (90.19% of the sample). The resultant samples include 35,490 and 34,110 player-game observations respectively.

With the matched samples, we estimate the average treatment effect that is local to a few days before and after a losing or winning game (local average treatment effect on the treated; LATT) by taking a difference:

$$\text{LATT} = \mathrm{E}\left[\Delta Y_{ij} | D_{ij} = 1, P_{ij} \approx p_{ij}\right] - \mathrm{E}\left[\Delta Y_{ij} | D_{ij} = 0, P_{ij} \approx p_{ij}\right]. \qquad \text{Eq. 1}$$

Here, i and j are a player from an African country and a football game respectively. We condition on the inverse of betting odds P_{ij} so that treated and control units have assignment probabilities centered around a value of p_{ij}.

The standard errors are two-way clustered by country and game. Clustering by country accounts for the possibility that players from the same country belong to different teams (in this

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[19] https://www.oddsportal.com (scraped on 5 April 2021).

[20] Because the assignment probabilities are directly measured, we use the simple matching method.

case, the values of the outcome variable are repeated). Clustering by game accounts for situations in which multiple players from Africa belong to teams in the same game (in this case, the values of the treatment variable are repeated). In the main analysis, we do not include fixed effects as DiD accounts for static factors. In robustness checks, we report the results with fixed effects.

Event Data Analysis: Results

The analyses with the main specification (Table 2) indicate that both barely losing and winning games increase the likelihood of demonstrations. Substantively, a close loss (win) increases the probability of demonstrations by 0.7152 (0.6821) percentage points. Because the average value of the outcome is 0.5970 percentage points, the close loss and win double the increasing rate of demonstrations. By contrast, the effects on riots and battles are statistically and substantively minimal; the effect sizes are less than half of that of close losses on demonstrations, and all of the signs are negative.[21] The absence of the effect on battles is consistent with our argument that armed conflicts are driven by organizational and strategic dynamics. The absence of the effect on riots implies that when emotional cues can drive people to start protests, they may use relatively safe peaceful means, which is consistent with some of the previous findings about rationality in irrational behaviors (Ge, Barbieri, and Schneider 2021).

[21] The average values of the outcomes of riot and battles are 0.6750 and 0.4387 respectively.

Table 2. Effects of Close Losses and Wins on Conflict Incidences

	ΔDemonstrations		ΔRiots		ΔBattles	
Close loss	0.7152^{*}		-0.2858		-0.0853	
	(0.2485)		(0.1830)		(0.2477)	
Close win		0.6821^{*}		-0.3723		-0.0618
		(0.3035)		(0.2407)		(0.3896)
N	35,488	34,108	35,488	34,108	34,295	34,108

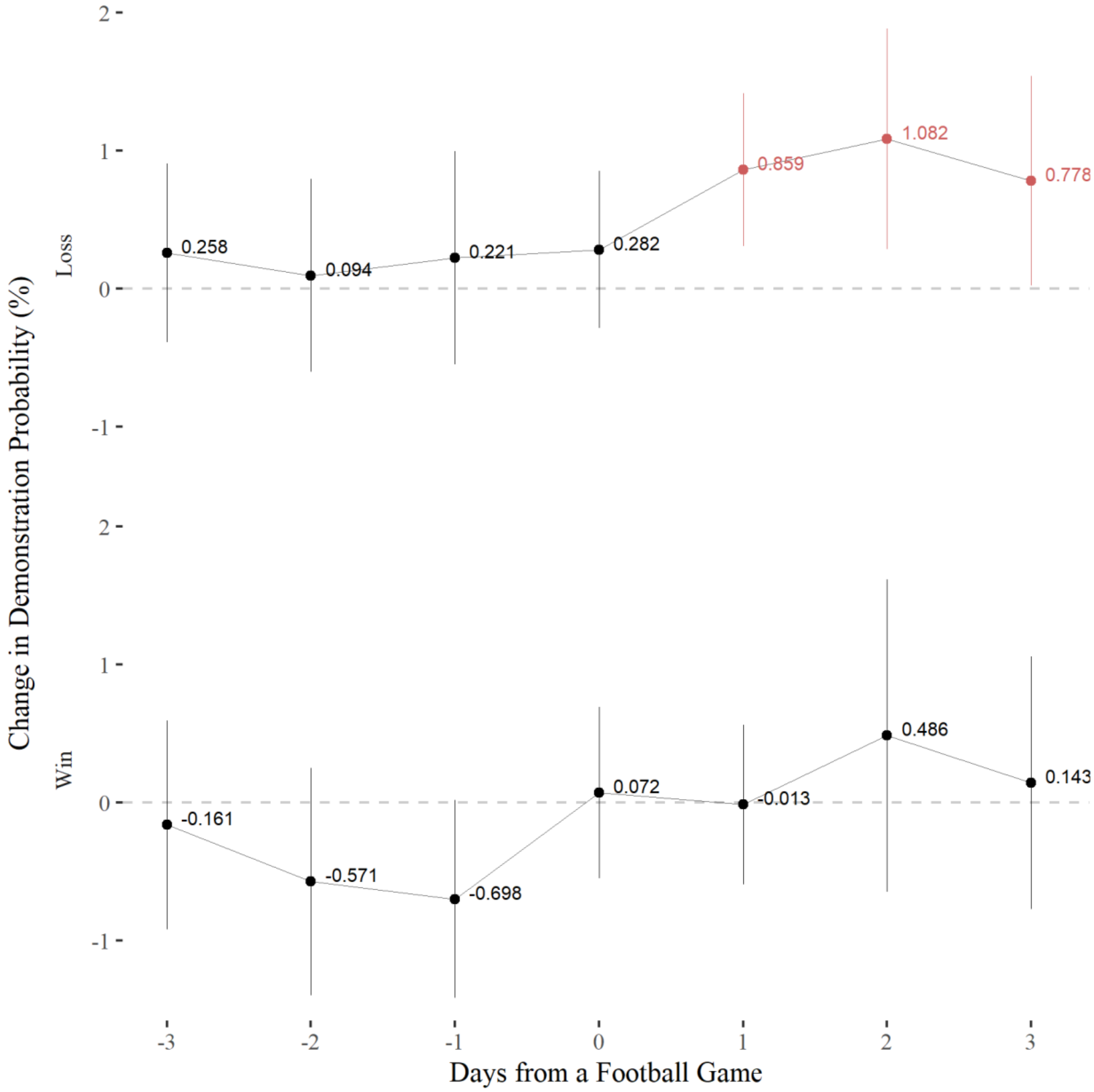
NOTE: The coefficient estimates and corresponding standard errors in parentheses. The standard errors are two-way clustered by player's birth country and game. $*$ $p < 0.05$; † $p < 0.10$.

Figure 3 shows the results without first differencing. The outcomes are the incidences of demonstrations in each day from a football game. The effects of close losses are pronounced one to three days after a football game, and there is no significant effect on demonstrations before football games.[22] By contrast, the effects of close wins are not statistically significant, and there are subtle, perhaps random, decreases in event probabilities on one to two days *before* a game. This implies that the estimated effect of wins in the main specification can potentially be an artifact of the DiD.[23] In fact, the effect of winning disappears in a later robustness check with an LDV model, which provides a conservative bound (Angrist and Pischke 2009). Overall, we find that barely losing games increases demonstrations, while the effects of winning games are uncertain. These results are not consistent with the rational update, asymmetric mood, and rally mechanisms, leaving the mood, blind attribution, and asymmetric attribution mechanisms as possibilities.

[22] For placebo tests with the past differences of the outcome variable, see Appendix 5.

[23] See Angrist and Pischke (2009, 243–47) and Keele (2020) about the problems of DiD.

Figure 3. Effects by the Days from a Football Game (Event Data Analysis)



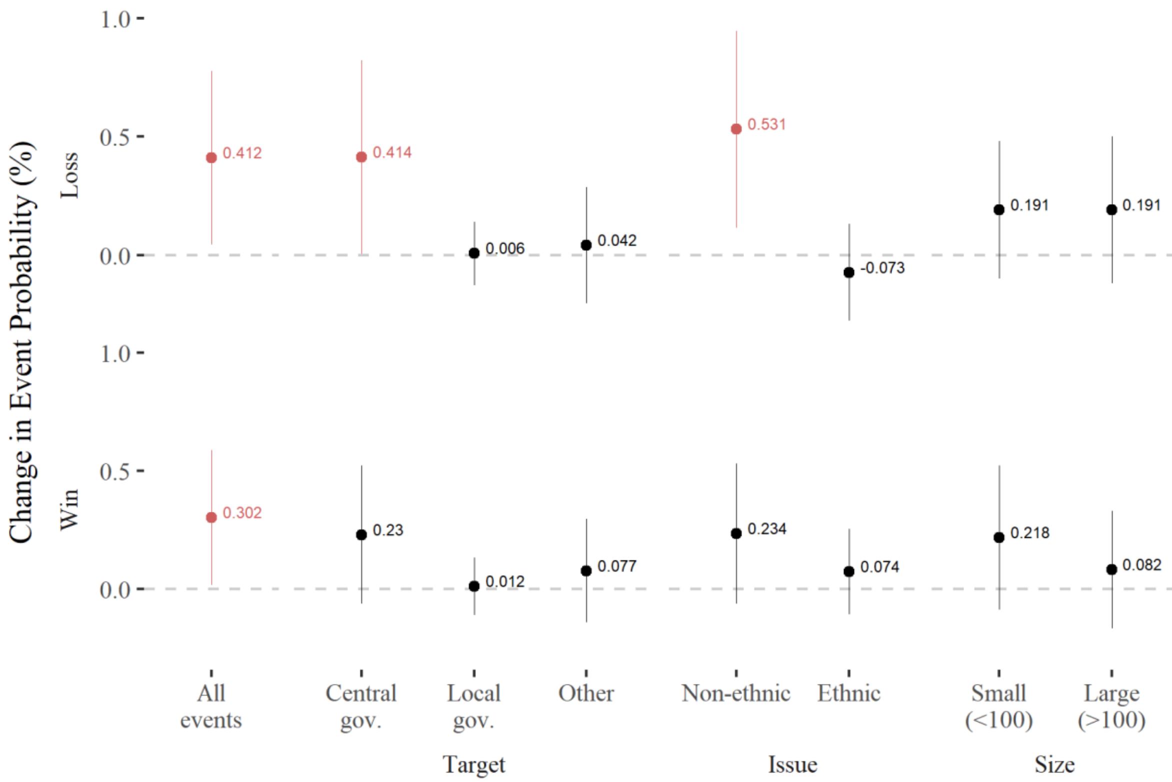
NOTE: The figure shows the estimated effects of close losses (top pane) and wins (bottom pane) on the likelihoods of demonstrations (%) for a range of days before/after a football game. The vertical bars are the 95% confidence intervals.

We also disaggregate the targets, issues, and sizes of demonstrations by using SCAD, which provides more detailed information about each event.[24] We first estimate the effects on all

[24] For details of the event categories, see Appendix 3.

types of demonstrations ("All events"). Figure 4 shows that our findings hold even with the alternative dataset ($p = 0.0278$). Moreover, the effects of close losses are particularly large for non-ethnic ($p = 0.0122$) demonstrations targeted at a central government ($p = 0.0481$).[25] We do not see differences due to the sizes of the demonstrations. These results are inconsistent with the rally mechanism (null effect on ethnic demonstrations) and provide suggestive evidence for the misattribution mechanisms (the existence of targeting). Because central governments are more visible, they can be an easy target of misattribution (see also the results of the survey analysis).

Figure 4. Effects by Targets, Issues, and Sizes of Demonstrations (SCAD)



NOTE: The figure shows the estimated effects of close losses (top) and wins (bottom) on demonstrations reported in SCAD. The vertical bars are the 95% confidence intervals.

[25] We also find similar results about ethnic conflicts with ACLED (Appendix 3 and 6).

Assumption Checks

To check the validity of the assumptions, we first regress the treatment variable on the pre-game betting odds (simple linear regression; Table 3). In a sample of close and non-close games, a one percentage point increase in the measured treatment assignment probability increases the actual treatment assignment probability by 0.9815 or 0.8740 percentage points, indicating the accuracy of the measurement. Predicting close games is slightly more difficult, reflecting the randomness of their outcomes; the equivalent change in the observed assignment probability increases the chances of losses and wins by 0.7135 and 0.6656 percentage points.[26]

Table 3. Prediction of Game Results by the Observed Assignment Probability

	Loss		Win	
Assignment prob. (loss)	0.9815* (0.0259)	0.7135* (0.0334)		
Assignment prob. (win)			0.8740* (0.0337)	0.6656* (0.0372)
Sample	All games	Close games	All games	Close games
N	61,511	43,997	59,478	43,095

NOTE: The coefficient estimates and corresponding standard errors in parentheses. The samples before matching. The standard errors are two-way clustered by player's birth country and game. $* p < 0.05$; $\dagger p < 0.10$.

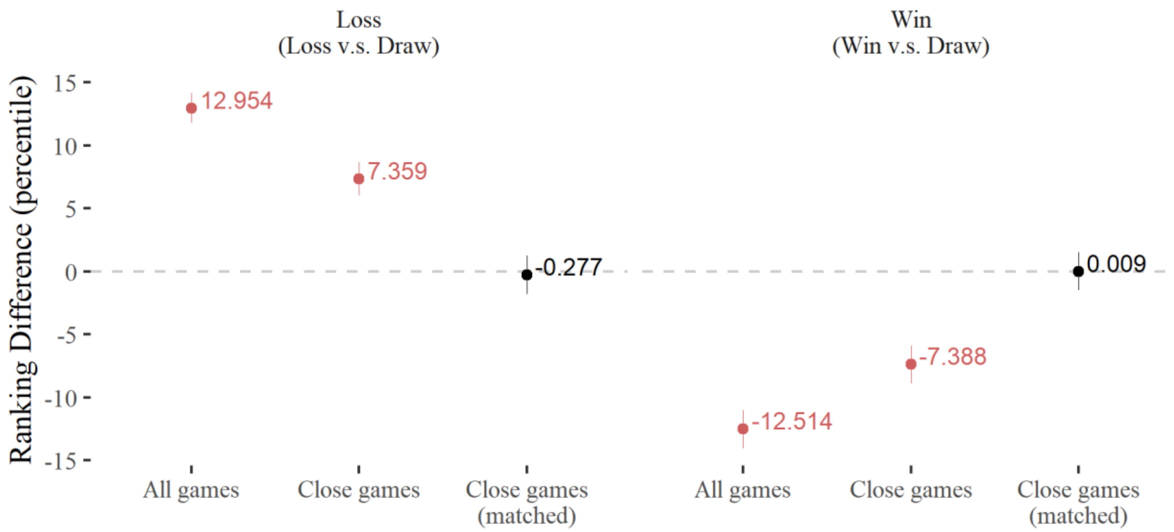
Second, we also check whether African players' teams in the matched pairs have similar characteristics in a covariate that is not used in the matching.[27] To this end, we compare their differences in the ranks within football leagues. The teams' ranks are standardized to a 0-100 percentile scale (the smaller the scale, the higher the rank) and we subtract the rank of a losing or winning team by the rank of a matched team in a draw game. As seen in Figure 5, teams in high

[26] The white noises in the predictors cause attenuation biases.

[27] Matching eliminates differences in the inverse of betting odds. The mean differences are 0.1571 percentage points for close losses and draws and 0.0032 percentage points for close wins and draws.

ranks are more likely to win and less likely to lose. Subsetting to close games reduces the differences, but there are still differences over seven percentile points. The matching on betting odds, however, eliminates the differences. This suggests that even though the RDD is not sufficient, conditioning on the pre-game betting odds addresses the remaining concerns.

Figure 5. Differences in Pre-game Team Ranks



NOTE: The figure shows the average team rank of losing (left pane) or winning (right pane) teams minus that of teams in draw games. The teams' ranks are standardized to 0-100 (the smaller the scale is, the higher the rank is). The vertical bars are the 95% confidence intervals.

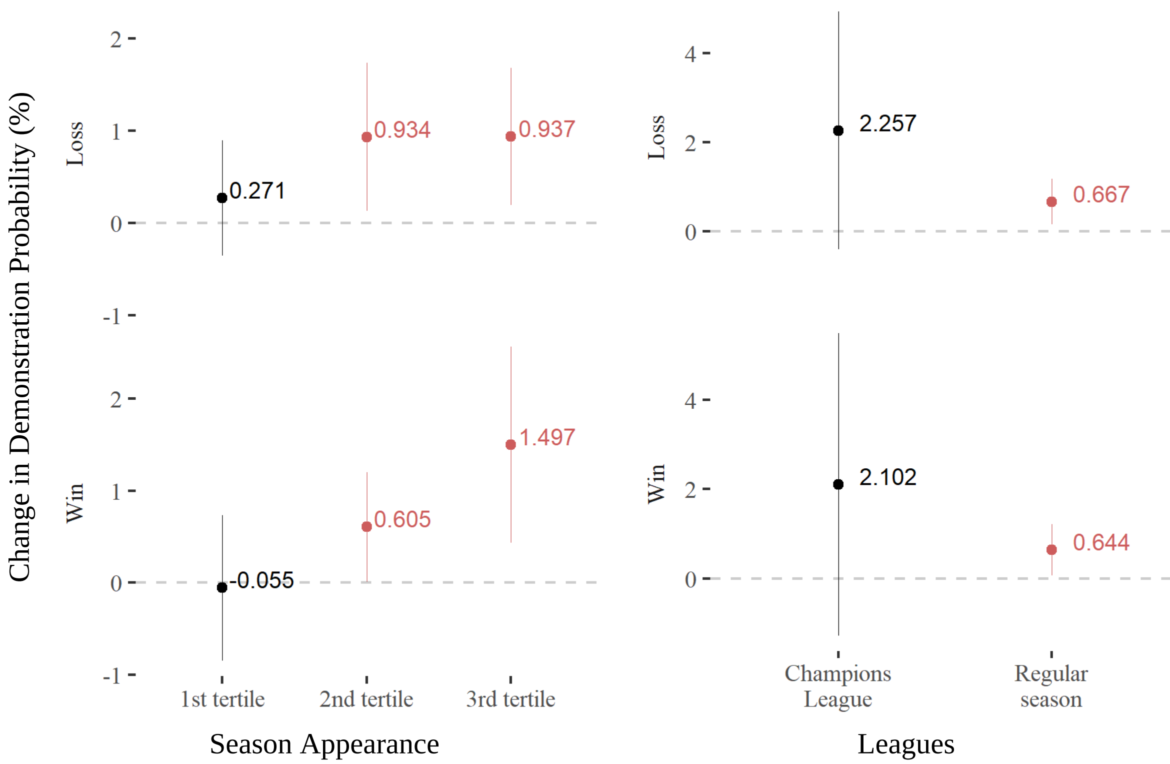
Effect Heterogeneities

We also check the face validity by subsetting the samples to substantively relevant cases: that is, games of players who are regularly on the pitch (regular players gain more attention), and games held at the Champions League (high-stake games).[28] Figure 6 reports the estimated effects of close losses and wins when we subset the samples by the tertiles of players' season appearances (left)

[28] The effect by prior expectations (reference dependence; Kőszegi and Rabin 2006) are reported in Appendix 7. The effects by leagues, regions and time periods are reported in Appendix 8.

and by the leagues (right). As seen in the left pane of Figure 6, the results are maintained only when players are regularly on the pitch. Similarly, the right pane of Figure 6 indicates that the effect sizes are over three times larger for the Champions League, though the confidence intervals are large due to the smaller number of observations (only about 3% of the observations are games held at the Champions League).

Figure 6. Effect Heterogeneities (Event Data Analysis)



NOTE: The figure shows the estimated effects of close losses (top) and wins (bottom) on the changes in the probabilities of demonstrations when the sample is split based on season appearances (left) and leagues (right). The vertical bars are the 95% confidence intervals.

Robustness Checks

Finally, we conduct a series of robustness checks, which are summarized in Table 4 and detailed in Appendix 9. The results of barely losing games are robust to any of the changes. By contrast, the results of barely winning games are less stable. Importantly, when we use the LDV model, which provides the conservative bound of the causal effect (Angrist and Pischke 2009), the effect of winning becomes null, and the point estimate shrinks (0.3405). The estimates of winning are also sensitive to the different configurations of samples. These results imply that our findings about winning can potentially be an artifact of DiD.

Table 4. Robustness Checks (Event Data Analysis)

	Loss	Win	Appendix
Omission of football games without African players' appearances	$+^{*}$	$+^{*}$	Table A9-1
Aggregated analysis at a country-game level	$+^{*}$	$+^{*}$	Table A9-2
Different transformations of the outcome	$+^{*}$	$+^{*}$	Table A9-3
Lagged dependent variable model	$+^{*}$	null	Table A9-4
Matching on assignment probabilities and the numbers of shots on target	$+^{*}$	null	Table A9-5
Inclusion of non-close games	$+^{*}$	null	Table A9-6
No matching	$+^{*}$	$+^{*}$	Table A9-7
Control for player performances, violent plays, and betting odds	$+^{*}$	$+^{*}$	Table A9-8
Player fixed effect	$+^{*}$	$+^{*}$	Table A9-9
Year-month fixed effect	$+^{*}$	$+^{\dagger}$	Table A9-9
Player-year-month fixed effect	$+^{*}$	$+^{*}$	Table A9-9
Month, day of week, and day of month fixed effects	$+^{*}$	$+^{*}$	Table A9-9
Different caliper sizes	$+^{*}$	$+^{*}$	Figure A9-1
Different time windows	$+^{*}$	$+^{\dagger 1}$	Figure A9-2
Leave-one-country-out tests	$+^{*}$	$+^{*2}$	Figure A9-3

NOTE: $* \, p < 0.05$; $\dagger \, p < 0.10$. Note 1: Null for $T = 1$. Note 2: Significant at a 10% level for one out of 40 cases.

Overall, the event data analysis indicates that the barely losing games increase demonstrations, while the effect of barely winning games is uncertain. These results leave the mood, blind attribution, and asymmetric attribution mechanisms (see Table 1). The additional analyses—especially those about targets—provide some support for the asymmetric attribution mechanism. The caveats are that we have not yet directly quantified attribution, moods, or identity, and that the country-level analyses can potentially suffer from aggregation biases. In the following sections, we therefore analyze individual-level surveys, which provide micro-level foundations.

Survey Analysis: Research Design

We examine the effects of the football games on moods, attitudes toward politicians, and nationalistic sentiments by analyzing individual-level surveys of Afrobarometer (2019). However, as we mentioned, the RDD with close games is insufficient for causal identification. Moreover, conditioning on pre-game betting odds cannot be implemented because there are so few football games held within a few days before the survey interviews.

We address these problems by exploiting as-if random coincidences of survey interviews and the football games (Muñoz, Falcó-Gimeno, and Hernández 2020). That is, we compare people who are interviewed just *a few days* before or after a close football game. Thus, the critical assumption of the survey-date RDD is that the dates of survey interviews are not affected by the results of close football games in Europe. As Depetris-Chauvin et al. (2020) argue, this is plausible as "the logistics involved in the implementation of the Afrobarometer survey (selection of the enumeration sites, setting up of the field teams, etc.) requires many months if not years of preparation, and are hardly related to the occurrence of sports events let alone to their unpredictable result" (1581; see Afrobarometer 2019 for details of survey implementation; see also Eifert, Miguel, and Posner 2010). The dates of the football games are also set at the start of each season and are unlikely to be affected by Afrobarometer surveys. Although the football games are usually held on weekends, we combine the survey-date RDD with the close-game RDD, which accounts for any confounding events held on weekends (we also include day-of-week fixed effects in a robustness check). Moreover, because the football games might affect non-responses in the surveys, we conduct balance checks, placebo tests, and density tests.

Sample and Unit

The unit of analysis is a triplet of a survey respondent, a player from a country in Africa, and a football game. The pairs of players and football games are organized like those in the event data analysis. We then link a survey respondent to the football games if an interview is held within three days before or after a football game (we also use different bandwidths in robustness checks), and if players from the respondent's country belong to either team in the game.[29] The resultant sample of barely losing (winning) and draw games contains 7,659 (7,011) respondents in 15 (12) African countries who are interviewed around the dates of 27 (26) football games of 23 (23) players between the 2005-2006 and 2018-2019 seasons.[30] Due to the lack of survey interviews held within

[29] If there are multiple games within three days before or after an interview, we include the interview only if all of the games are held *either* before or after the interview (if there are games *both* before and after an interview, the treatment assignment is ill-defined. If we were to include such observations as treated units, it would artificially increase the number of treated units and result in bunching). We then select the nearest games and assign their treatment statuses accordingly. If multiple games are selected, or if there are multiple co-national players in a game, we count each respondent-player-game as a separate observation. We account for the repeated observations by clustering the standard errors. We also check the robustness with the data aggregated at a respondent-game level.

[30] As in the event data analysis, we drop games if both sides have players from the same country in Africa. We also drop observations on the days of the football games because their treatment statuses are indeterminate. Finally, we drop a game if all relevant interviews are conducted either before or after the game because there is no within-game variation of the treatment status.

the bandwidth, the samples do not contain the Champions League. The numbers of observations are 10,398 for the sample of close losses and draws and 9,410 for the sample of close wins and draws. The summary statistics are available in Appendix 10.

Treatment Variables

The first treatment variable is the same as that in the event data analysis: a dichotomous variable D_{ij} that takes 1 if a team to which player i from an African country belongs barely loses or wins game j by a margin of one score and 0 if the result is a draw.[31] The second treatment variable R_{jk} takes 1 if respondent k is interviewed after football game j and 0 otherwise.

Outcome Variables

The outcome variable W_k is respondent k's answer to a given survey question. Following and expanding Depetris-Chauvin et al. (2020), we select 18 indicators in Afrobarometer that are relevant to the causal mechanisms and available in a majority of the survey rounds in the period of analysis (2005-2019; 3rd to 7th rounds).[32] For the attribution mechanisms, we select eight indicators about trust in a leader (president or premier), members of parliaments (MPs), local councils, ruling parties, opposition parties, police, army, and courts, as well as three indicators of the performances of leaders, MPs, and local councils.

For the mood mechanisms, we use the interviewer's evaluation of the respondent's attitude during the interview, including their friendliness, interest in survey questions, cooperativeness, patience, ease, and honesty. Although ideally we would like to directly measure the positive and

[31] We later conduct a robustness check by splitting the sample to those with and without African players' appearances.

[32] The exact survey questions are in Appendix 11.

negative moods (e.g., excitement, elation, sadness, and anger) as in experimental studies (Busby, Druckman, and Fredendall 2016; Busby and Druckman 2018), the direct measures are rarely available in observational studies. We therefore use the indirect attitudinal measures.[33] Even though each item does not directly relate to moods and only relate to respondents' positive attitudes, respondents' moods can be reflected in their attitudes, and thus the attitudinal measures can *collectively* capture respondents' moods. For this reason, we aggregate the attitudinal measures by calculating the average ("overall mood"). Although the measurement may not be perfect, we believe this is the best among available in our observational setup. Finally, following Depetris-Chauvin et al. (2020), we use an item about national identity—a one-dimensional scale of ethnic-national identity. All of the survey answers are rescaled to a 0-10 range.[34]

Specification

With these variables, we estimate the average treatment effect local to respondents who answered questions a few days before and after close losses or victories, by taking double differences:

$$\begin{aligned}\text{LATT} = &\left(\mathrm{E}\left[W_k \mid D_{ij} = 1, R_{jk} = 1\right] - \mathrm{E}\left[W_k \mid D_{ij} = 1, R_{jk} = 0\right]\right) \\ &- \left(\mathrm{E}\left[W_k \mid D_{ij} = 0, R_{jk} = 1\right] - \mathrm{E}\left[W_k \mid D_{ij} = 0, R_{jk} = 0\right]\right).\end{aligned} \qquad \text{Eq. 2}$$

The main unit of analysis is respondent k. Player $i \equiv i_{(k)}$ refers to a player born in respondent k's country. Football game $j \equiv j_{(i,k,h)}$ refers to a football game of player i's team held within h days before or after respondent k's interview. For the reasons we mentioned in the event data analysis,

[33] Unlike Depetris-Chauvin et al. (2020), we do not use respondents' evaluation of economy as a measure of moods. As we show in an additional analysis, the item is more closely related to the evaluation of welfare—a key factor in the indirect attribution mechanisms.

[34] We also standardize the outcomes, and it does not change the results.

the bandwidth is set to three days ($h = 3$; see footnote 15), and we conduct robustness checks with smaller bandwidths.[35] The observations are triangularly weighted by the days from a football game. Given the small bandwidth and the potential for overfitting, we use the double-difference and leave the regressions with the running variable, fixed effects, and control variables to robustness checks.

We address the problems of multiple hypothesis testing by controlling the false discovery rates (Benjamini and Hochberg 1995).[36] The standard errors are two-way clustered by country and game. Similar to the event data analysis, this accounts for repeated observations of games and players. Because each respondent belongs to a single country, clustering by country also accounts for repeated observations of survey respondents (see footnote 29).

Survey Analysis: Results

Figure 7 reports the results of the RDD.[37] As seen in the left pane of Figure 7, people who answered the questions immediately after barely losing games tend to place less trust in a leader and under-evaluate the leader's performance. Because the average value of the trust in a leader is 5.327, a

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[35] The automatic bandwidth selection cannot be used in discrete RDDs (Imbens and Kalyanaraman 2012). Imbens and Wagner (2018) and Kolesar and Rothe (2018) propose inferential frameworks for discrete RDDs, but they do not provide methods for bandwidth selection. Cattaneo et al. (2015) propose a covariate-based method, but the method does not account for clustering. Without clustering SEs, the method finds covariate imbalances and thus doesn't yield an optimal bandwidth.

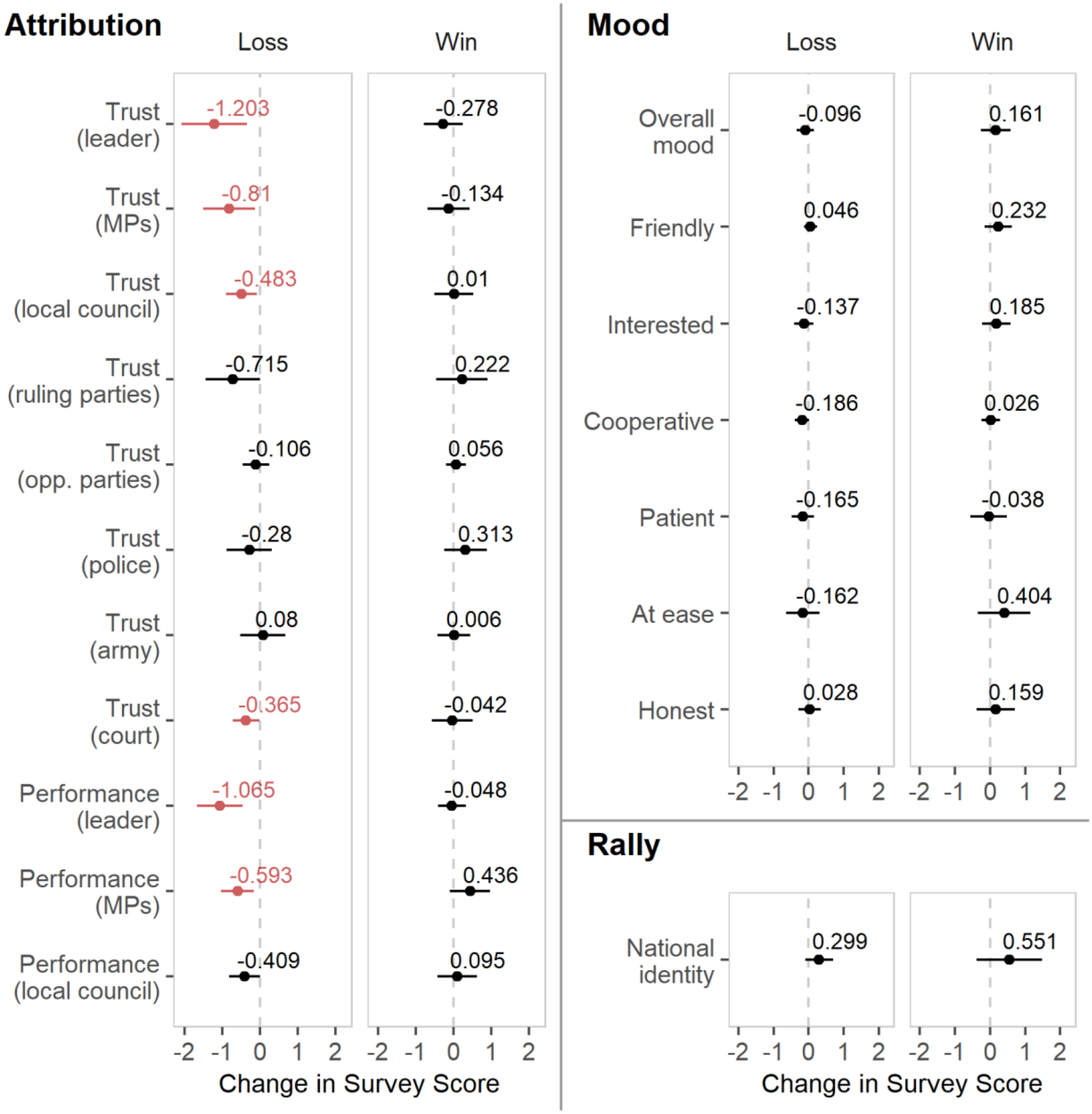
[36] Using the other methods of adjustment does not change the results. Applying the same adjustment to the event data analysis does not change the results.

[37] The detailed tables are available in Appendix 12. We do not find that having a football game itself has significant effects on the outcomes.

close loss decreases it by 23%. Barely losing games also lowers people's attitudes toward MPs and ruling parties, and, to a lesser extent, local councils and courts. The effects of close losses are nearly zero for trust in opposition parties, police, and the army. These results suggest that people tend to blame visible politicians (i.e., leaders, MPs, ruling parties) who are supposed to be responsible for the people's welfare.

By contrast, a close victory has little effect on trust or performance evaluation. Although close victories are estimated to decrease trust in a leader by 0.278 points, the effect is not statistically significant, and the effects on some of the other outcomes (e.g., trust in ruling parties and performance of MPs) are even positive. Thus, at least statistically, there is no evidence that people blame or credit politicians for close victories. These asymmetric effects of losses and wins are consistent with the asymmetric attribution mechanism.

Figure 7. Effects of Close Losses and Wins on Individual Attitudes



NOTE: The figure shows the effects of close losses and wins on respondents' attitudes in Afrobarometer (rescaled to 0-10). The horizontal bars are the 95% confidence intervals (adjusted for false discovery rates).

Turning to the mood mechanisms (top right corner of Figure 7), we do not see any large effect on respondents' observed attitudes. In fact, most of the point estimates and confidence intervals are tightly centered around zero, indicating precise null. Although we cannot exclude the mood mechanisms given the limitations of the measurement, we do not find strong evidence.

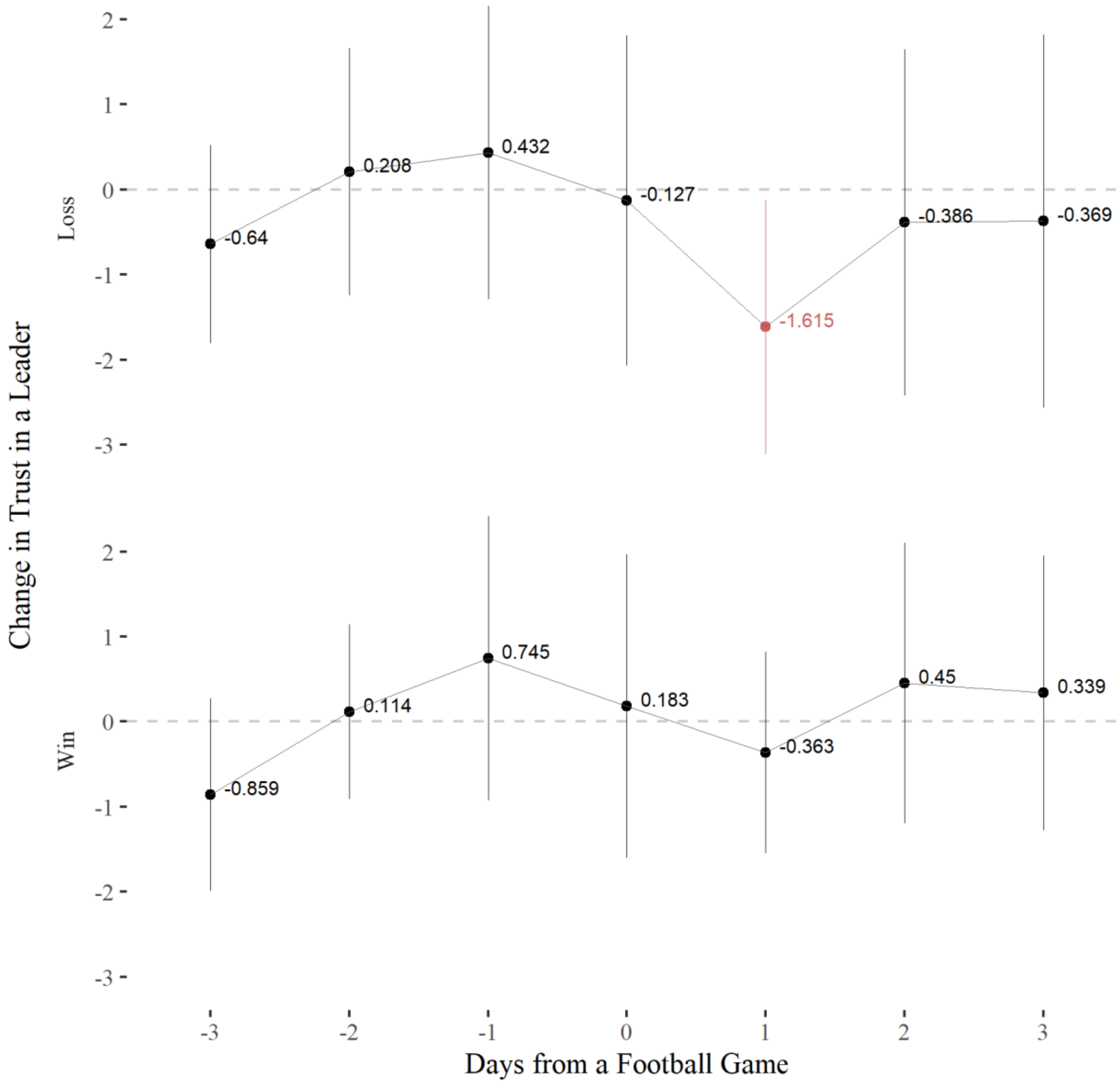
Moreover, without referring to the attribution mechanisms, the mood mechanisms cannot explain why losing matches lowers the trust in a leader while not changing the trust in opposition parties, police, and army—without attribution, bad moods should lower trust in any actors.

Similarly, we find only an inconclusive result for the rally mechanism (bottom right corner of Figure 7). Although the point estimates are positive and relatively large, they are not statistically significant. Thus, even though we cannot deny the possibility that both losing and winning games would unite people through nationalism, the statistical evidence is weak. Overall, the results are the most consistent with the asymmetric attribution mechanism. The differential effects on political trusts indicate that people directly or indirectly attribute bad luck in football games to incumbents.

In Figure 8, we decompose the effects on the trust in a leader by the days from a football game.[38] As shown in the figure, the effect of the close losses is pronounced a day after a football game. This result is consistent with the event data analysis, which shows that the effect of close losses is large one to three days after a football game (see Figure 3). This means that a close loss in a football game (which is usually held in the evening) affects people's perceptions the next day $(t + 1)$ and then trigger demonstrations on days $t + 1$ to $t + 3$. The lag up to two days may indicate the time during which tiny unreported events grow into those reported in the media.

[38] We estimate the effects of close losses and wins for each day from a football game. Because the close-game RDD alone can produce biased estimates, we subtract the estimates by the pre-game averages of the estimated effects to account for any non-random assignment of the game results. The results of the other outcomes are provided upon request.

Figure 8. Effects by the Days from a Football Game (Survey Analysis)

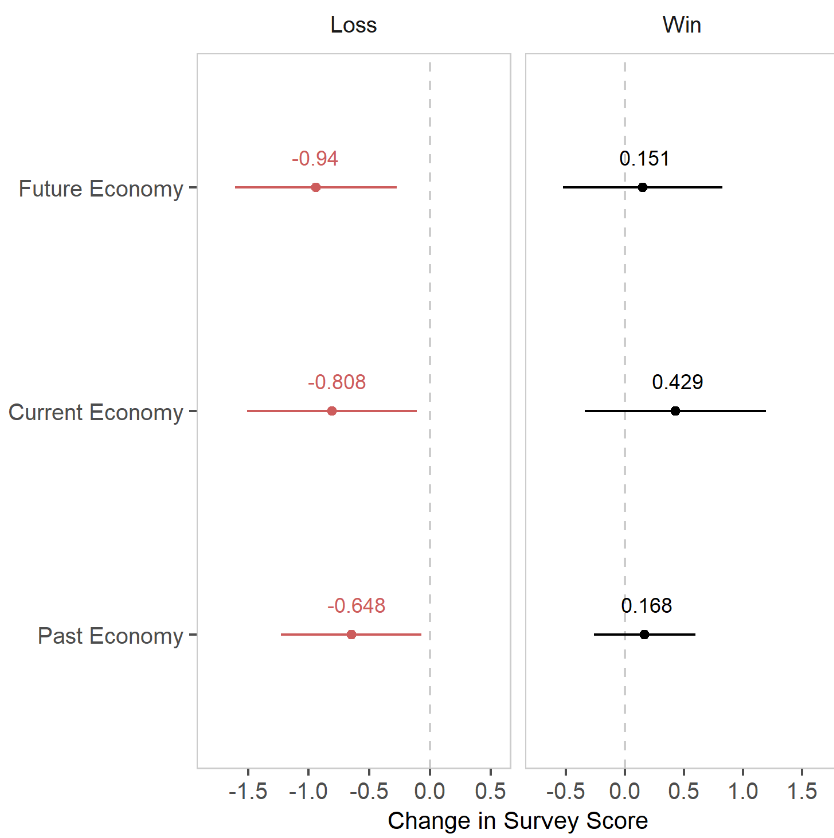


NOTE: The figure shows the estimated effects of close losses (top pane) and wins (bottom pane) on the trust in a leader (0-10 scale) for a range of days before/after a football game. The vertical bars are the 95% confidence intervals.

Direct or Indirect Attribution?

The main results are consistent with the asymmetric attribution mechanism. But this may or may not mean that people *directly* attribute the bad lucks in football games to domestic politicians. A more realistic interpretation is that losing matches decrease the respondents' subjective welfare, which in turn is attributed to politicians (*indirect* attribution). We therefore analyze the effects of football games on respondents' evaluations about future, present, and past economy.[39] As seen in Figure 9, losing matches significantly lower respondents' evaluation of the economy. The fact that the football games affect the evaluation of the *past* economy implies that the results do not capture the effects on the actual economy; it suggests that the football games changed respondents' *subjective* evaluation of their economic welfare.

Figure 9. Effects of Close Losses and Wins on Welfare Evaluation



NOTE: The figure shows the estimated effects of close losses (left pane) and wins (right pane) on the evaluation of future, current, and past economy. The horizontal bars are the 95% confidence intervals.

[39] See Appendix 11 for exact survey questions. See footnote 33 about the relationship to moods.

Assumption Checks

To check the plausibility of the identification assumption, we conduct balance checks, placebo tests, and density tests, all of which are summarized in Table 5. As covariates, we use the eleven objective indicators: age, female, Muslim, Christian, primary education, employment, and accesses to food, water, medical care, cooking fuel, and cash (dummies except for age). The third to sixth columns of Table 5 show the number of observations and average values of the covariates for the treated (those interviewed immediately after barely losing or winning games) and control (other respondents in a sample) groups. The seventh and eighth columns show the standardized mean differences and variance ratios of the treated and control groups. As a rule of thumb, a covariate is said to be balanced if the standardized mean difference is between -0.2 and 0.2, and the variance ratio is between 0.5 and 2 (Rubin 2001). The last column shows the p-values of placebo tests, in which the outcome variable in Eq. 2 is replaced by each of the covariates. Finally, the p-values of the density tests are reported at the bottom of each pane.[40] As seen in Table 5, even though there are a few minor imbalances in 4 out of 66 balance metrics (6.06%), there is no consistent evidence of imbalance, and the density tests indicate no evidence of sorting. We later check the robustness by controlling for the covariates.

[40] In the density tests, we use a respondent as a unit of analysis because it is not straightforward to cluster the standard errors. We also include the observations on the days of the football games, because we cannot otherwise implement the density tests.

Table 5. Balance Checks, Placebo Tests, and Density Tests

		N (treat)	N (control)	Mean (treat)	Mean (control)	Std. diff.	Var. ratio	Placebo p-value
Close loss vs. Draw	Age	2590	7758	36.13	37.45	-0.0660	0.8744	0.2639
	Female	2605	7793	0.5048	0.5040	0.0011	1.0002	**0.0843**
	Primary education	2589	7738	0.1881	0.3136	**-0.2069**	0.7096	0.4592
	Muslim	2589	7738	0.6964	0.5651	0.1941	0.8605	0.2708
	Christian	2597	7765	0.8086	0.8126	-0.0072	1.0166	0.7264
	Employed	2598	7756	0.2906	0.3338	-0.0660	0.9273	0.5896
	No food	2601	7787	1.4245	1.0209	**0.2242**	1.1190	0.7513
	No water	2602	7788	1.2648	1.0777	0.0956	1.0455	0.3687
	No medical care	2593	7766	1.5064	1.1778	0.1723	1.0254	0.5512
	No cooking fuel	2590	7775	0.9444	0.7525	0.1172	1.1863	0.5473
	No cash	2594	7777	2.3392	1.8719	**0.2398**	0.7917	0.6166
							Density test p-value:	0.1343
Close win vs. Draw	Age	1629	7730	36.97	37.70	-0.0349	1.0791	0.1594
	Female	1644	7766	0.5091	0.5023	0.0096	1.0002	0.6888
	Primary education	1620	7700	0.3142	0.3805	-0.0987	0.9146	0.9732
	Muslim	1620	7700	0.6154	0.5236	0.1316	0.9493	0.6870
	Christian	1637	7742	0.8583	0.8233	0.0677	0.8365	0.9590
	Employed	1642	7742	0.3197	0.3555	-0.0535	0.9498	0.2733
	No food	1643	7761	0.9422	0.8909	0.0311	0.9918	0.3815
	No water	1643	7762	1.0803	1.0330	0.0257	0.9067	0.1525
	No medical care	1640	7749	1.0433	1.0800	-0.0208	0.8760	0.4175
	No cooking fuel	1634	7747	0.7534	0.7294	0.0158	0.9671	0.5572
	No cash	1639	7751	1.6480	1.6804	-0.0160	0.9284	0.2520
							Density test p-value:	0.8279

NOTE: If the standardized mean difference is larger than 0.2 or smaller than -0.2, if the variance ratio is larger than 2 or smaller than 0.5 (Rubin 2001), or if the p-values are less than 0.1, the numbers are bolded.

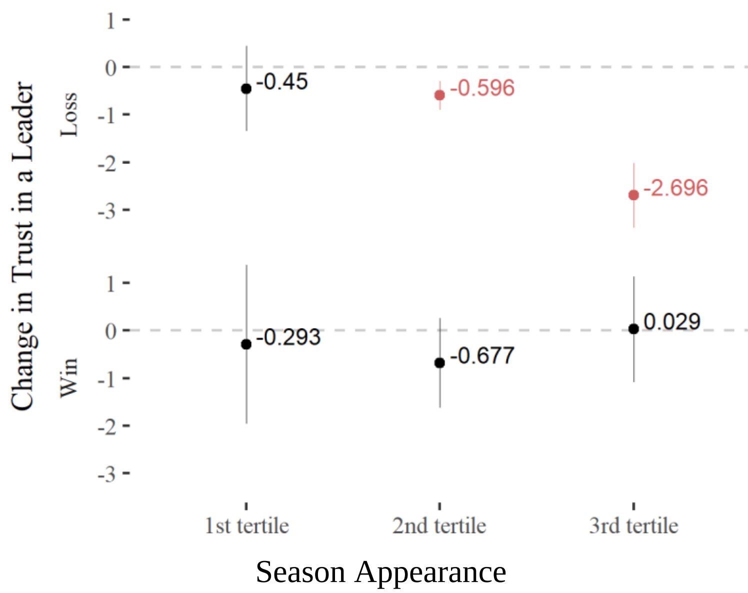
Effect Heterogeneities I: Substantive Relevance

Similar to the event data analysis, we check the face validity by subsetting to substantively relevant cases.[41] Because the samples do not contain the Champions League (no game was held within three days before or after survey interviews), we only report the results by players' season

[41] The effects by prior expectations (reference dependence) are reported in Appendix 13. The effects by regions and time periods are reported in Appendix 14.

appearances. Consistent with the event data analysis, Figure 10 indicates that the effects of close losses are large for the games of regular players, while the effects of close victories are null.

Figure 10. Effect Heterogeneities I (Survey Analysis)



NOTE: The figure shows the effects of close losses (top) and wins (bottom) on respondents' trust in a leader (rescaled to 0-10) in the tertiles of season appearances. The vertical bars are the 95% confidence intervals.

Effect Heterogeneities II: Demographic Covariates

For explorative purposes, we subset the data by demographic indicators (Figure 11).[42] First, we subset the data by educational attainment to examine whether education can reduce cognitive biases. The results provide some evidence—secondary education reduces misattribution—but we do not find equivalent results for higher education. This may be explained by the fact that people with higher education tend to learn liberal ideas and hence are more motivated to blame leaders.

Second, we subset the samples by Islamic religiosity to indirectly quantify the roles of drinking; that is, the results of the football games might affect alcohol consumption (Rees and

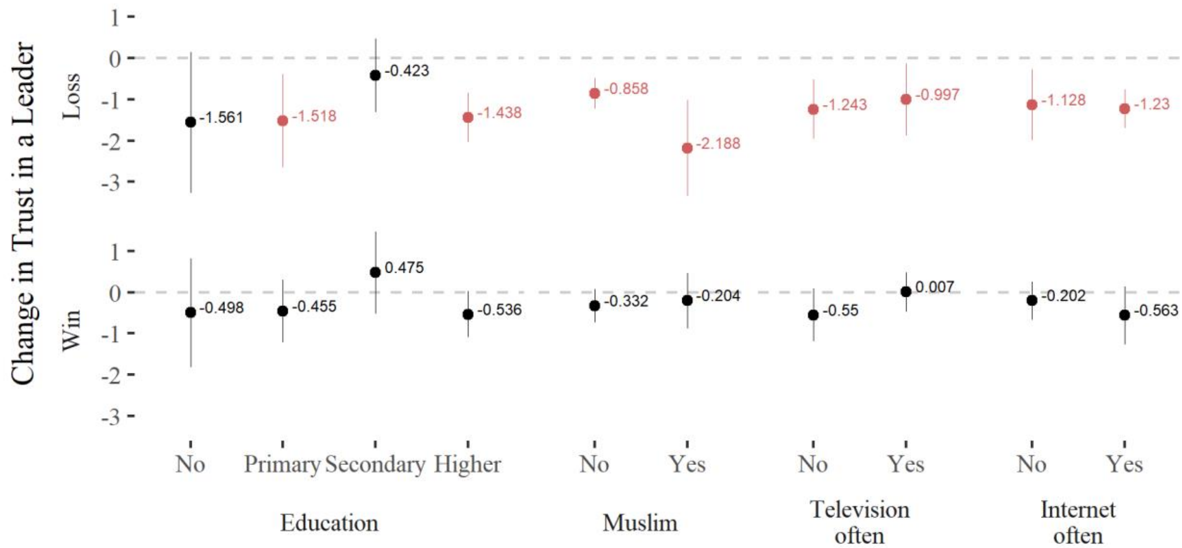
[42] The effect heterogeneities by other (less relevant) covariates are reported in Appendix 15.

Schnepel 2009; Wood, Mcinnes, and Norton 2011; Lindo, Siminski, and Swensen 2018), which in turn might induce cognitive biases. Because the Koran prohibits drinking, if there is an effect on Muslim followers, the explanation should come from other reasons. Figure 11 indeed shows that the effect of close losses is even more pronounced for Muslim people.

Third, we explore whether media usage induces any modification in the causal effects. To this end, we conduct subsample analyses for television and internet uses—primary as a means to watch live football games. Although only items about media access to news are available in Afrobarometer,[43] the indicators may still be used as proxies. The right panes of Figure 11 show no large differences due to media usage. This, however, can potentially be explained by the coarseness of the proxies. Note also that all of these covariates are not necessarily exogenous, and hence that the effect heterogeneities are under-identified.

[43] The survey question is "How often do you get news from the following sources: [television/internet]" (Afrobarometer 2019).

Figure 11. Effect Heterogeneities II (Survey Analysis)



NOTE: The figure shows the effects of close losses (top) and wins (bottom) on respondents' trusts on a leader in subsamples. The vertical bars are the 95% confidence intervals.

Robustness Checks

Finally, we conduct an array of robustness checks, which are summarized in Table 6 and detailed in Appendix 16. As seen in the table, while the results about close wins are unstable, the effects of close losses are robust to most of the changes. The only exception is the inclusion of player-fixed effects ($p = 0.1191$), while the even tighter specification (i.e., model with player-year-month fixed effects) yields a somewhat significant result ($p = 0.0538$). We surmise that the fixed-effect models overfit the data, making it difficult to calculate the clustered standard errors, and thus yield inconsistent results. In addition, recent studies show that fixed-effect models can potentially induce biases toward zero (Imai and Kim 2019).

Table 6. Robustness Checks (Survey Analysis)

	Loss	Win	Appendix
Omission of football games without African players' appearances	$-^{*}$	null	Table A16-1
Aggregated analysis at a respondent-game level	$-^{*}$	null	Table A16-2
Inclusion of non-close games	$-^{*}$	$-^{*}$	Table A16-3
Matching on betting odds	$-^{*}$	$-^{*}$	Table A16-4
Control for demographic covariates, player performances, violent plays, and betting odds	$-^{*}$	null	Table A16-5
Control for the running variable	$-^{*}$	null	Table A16-6
Player fixed effect[1]	null	$-^{*}$	Table A16-7
Year-month fixed effect[1]	$-^{*}$	null	Table A16-7
Player-year-month fixed effect[1]	$-^{\dagger}$	$-^{*}$	Table A16-7
Month, day of week, and day of month fixed effects[1]	$-^{\dagger}$	null	Table A16-7
Different bandwidths	$-^{*}$	$-^{*2}$	Figure A16-1
Leave-one-country-out tests	$-^{*}$	null[3]	Figure A16-2

NOTE: $* \, p < 0.05$; $\dagger \, p < 0.10$. Note 1: Due to the numerical instabilities with the fixed effects, the standard errors are two-way clustered by player (instead of country) and game. Note 2: Null for $h = 3$. Note 3: Significant at a 5% level for one out of 16 cases, and significant at a 10% level for one of 16 cases.

Discussion

In this paper, we have analyzed the effects of psychological cues on conflicts and built hypotheses about the effects of extraneous sports events on protests. The analyses provide both evidence and counterevidence to the hypotheses. First, the rational update mechanism is unlikely to be sufficient; it cannot fully explain why the politically irrelevant events affect protests or people's attitudes in Africa. Second, the mood and asymmetric mood mechanisms are not strongly supported by our analyses. Although the measurement is rather limited, the survey analysis shows that the results of the football games have no discernible effect on the observed attitudes of the respondents. The moods cannot also explain the differential effects on trusts. Third, the blind attribution mechanism cannot explain the weakly positive and mostly null effect of close winning on protests. Fourth, the rally mechanism is also not supported by solid evidence; European football might allow people in Africa to overcome ethno-religious divisions, but our analysis shows that it does not decrease protests or those related to ethnic issues. This implies that we cannot simply extend the findings about the international sports events (Depetris-Chauvin, Durante, and Campante 2020) to European Professional Football.

These results leave the asymmetric attribution mechanism. The near-null results of close victories are consistent with the proposition that people tend to perceive victories as their own success and thus do not credit politicians. By contrast, since people tend to blame others—including not only coaches and managers but also their politicians—for the failures of their favored teams, the close losses should lower their trust in politicians and thus increase protests. The additional analysis about people's subjective welfare implies that the asymmetric attribution can be indirect; the losing matches lower people's subjective welfare, which in turn is attributed to politicians. Finally, the weak findings that close victories increase protests might be explained by self-confidence. That is, if people perceive a victory as their own success, they may become more confident in their own capabilities, which in turn may motivate them to challenge authorities. This, however, is conjecture; the results of winning are not robust and may be false positives.

Substantively, our findings imply that European professional football has an unintended externality across the continents; football in Europe makes people in Africa blame their governments for their teams' losses and leads to protests. Fortunately, the effect is limited to peaceful demonstrations; we do not observe equivalent effects on violent riots or armed conflicts. Moreover, we cannot deny the possibility that European football provides a psychological cue for peaceful demonstration and hence incentivizes the government to address problems in some cases. Given these possibilities, we refrain from making any hasty judgments. It is a task of future studies to analyze the welfare consequences of the spillover.

Finally, to the best of our knowledge, this is the first study that extends the insights in the behavioral literature of voting (Healy and Malhotra 2013)—i.e., the relevance of seemingly irrelevant events—to conflict studies and international relations. Although previous studies have analyzed the psychological causes of conflicts by conceptual discussion, correlational analyses, and survey experiments (see Hafner-Burton et al. 2017; Kertzer and Tingley 2018; Davis and McDermott 2021), the causal evidence with real-world data has not been accumulated to the level of electoral studies. We have filled this gap by analyzing events that are not directly relevant to rational updates. Future studies will likely apply the research design to other topics and provide further evidence about the psychological origins of social conflict.

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