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 games 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 in turn can trigger protests. By exploiting as-if random variation in the results of 15,102 close football games conditional on betting odds, we find that compared to draw games, close losses of African players' teams increase peaceful protests in their original countries while not changing the likelihood of riots or armed conflicts. The effect is particularly large for non-ethnic protests targeted at a central government. Close losses also temporarily decrease people's trust in their country's leader. By contrast, close victories do not have equivalent or compensating effects on protests or public opinion. These results suggest asymmetric misattribution: people in Africa unreasonably blame domestic politicians for bad luck in European football games, prompting protests; but they do not credit politicians with football victories.

> Karl Marx said "Religion is the opiate of the masses." He was wrong. It's sports.
> —Bill Scharrer[1]

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." Another Arsenal supporter, Jude, does not even deny the risk of escalating violence: "What often makes me angry in football centers [places to watch 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

1. Scharrer 2017.

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I believe it can cause physical fights one day."[2] Clearly, the outcomes of 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 the effect of results in professional football in Europe on conflicts in Africa. In doing so, we challenge previous studies on conflict, which until recently have ignored the roles of politically irrelevant factors like sports events.[3] Most of the standard frameworks—including grievance theses,[4] contentious politics frameworks,[5] collective action theories,[6] and bargaining models[7]—give the impression that politically irrelevant events should not matter, because they would not obviously affect the costs, benefits, information, or outcomes of conflict. We, however, argue that psychological cues do have an effect.

To this end, sports events provide unique analytical advantages. Other dramatic events, like natural disasters, are also influential. However, disasters provide citizens with objective information about the government's performance in mitigatory and response policies.[8] Thus, even without any effects from psychological cues, natural disasters can affect information, and thus citizens' rational choices, and hence conflicts. By contrast, as Ashworth, Bueno de Mesquita, and Friedenberg acknowledge,[9] sports events provide no objective information about politicians' competence.[10] The political irrelevance of sports events allows us to isolate the effects of psychological cues from those of rational updates.

Moreover, unlike Card and Dahl and related studies on the economic and social consequences of sports events,[11] we analyze a politically, geographically, and causally distant relationship between football in Europe and conflicts in Africa. While the literature focuses on domestic relationships, the effects of "local" sports events are rarely exclusively local; hundreds of millions of people in Africa, for instance, breathlessly watch football games played in Europe. Given the globalization of the sports industry, it is imperative to understand the intercontinental spillover of major sports events.

2. Quoted in Ka, Od, and I 2018, 7, 8.
3. Bertoli 2017; Depetris-Chauvin, Durante, and Campante 2020.
4. Gurr 1970.
5. McAdam, Tarrow, and Tilly 2003.
6. Tullock 1971.
7. Fearon 2004.
8. Ashworth, Bueno de Mesquita, and Friedenberg 2018. See also Achen and Bartels 2017, 2018; Fowler and Hall 2016; Healy and Malhotra 2009, 2010, 2013; Malhotra and Kuo 2008; Ramos and Sanz 2020. Other studies examine Christmas lotteries (Bagues and Esteve-Volart 2016), sunshine (Bassi 2013, 2019), and foreign aid reception (Cruz and Schneider 2017).
9. Ashworth, Bueno de Mesquita, and Friedenberg 2018.
10. Busby and Druckman 2018; Busby, Druckman, and Fredendall 2016; Fowler and Montagnes 2015; Graham et al., forthcoming; Healy, Malhotra, and Mo 2010, 2015; Miller 2013.
11. Card and Dahl 2011. Previous studies analyze the effects of sports on court judgments (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 (Priks 2010; Wann et al. 2001), crimes (Ge, Barbieri, and Schneider 2021; Kalist and Lee 2016; Marie 2016; Munyo and Rossi 2013; Rees and Schnepel 2009), and unhealthy eating (Cornil and Chandon 2013), among other things.

Last but not least, unlike recent studies on big international sports events such as the World Cup and the Africa Cup of Nations,[12] we focus on European professional football to ensure the political irrelevance of the events. Even though we do not find compelling evidence except for a few correlational studies,[13] one might still suspect that politicians in Africa could affect the results of international competitions via budgetary allocations and sports policies (for example, selection of managers and coaches).[14] 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 psychological factors.

One such factor is moods; losing a game can affect emotional status, causing anger, distress, shame, sadness, and anxiety.[15] People can join protests to vent their negative mood. Conversely, people may bask in the glory of their team's victory. Another factor is misattribution; people can directly or indirectly misattribute "bad luck" in a game to their politicians and thus stage protests.[16] By contrast, after a game victory, people may give undeserved 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.[17]

We examine these mechanisms in two sets of analyses. In the first, we identify the effect of game results in Europe on protests in Africa by exploiting as-if random variation in the results of close football games conditional on pre-game betting odds. Analysis of 15,087 close games and 40 African countries reveals that compared to draws, losses of African players' teams increase peaceful demonstrations in their original countries while not changing the likelihood of riots or armed conflicts. The effect of close losses is particularly large for non-ethnic demonstrations targeted at a central government.

Once we establish the causal relationship, we dissect the mechanisms by using individual-level surveys. By exploiting as-if random coincidences of survey interviews and football games, we show that compared to draws, close losses temporarily decrease people's trust in their political leaders, while winning or losing games have almost no effect on people's moods. Overall, these findings, as well as extensive analysis of effect heterogeneity and robustness, indicate that football games in Europe have sizable effects on social conflicts in Africa.

12. Bertoli 2017; Depetris-Chauvin, Durante, and Campante 2020; Rosenzweig and Zhou, 2021.
13. Jiménez 2016; Leeds and Leeds 2009.
14. There are a few counterexamples in history, such as prewar Italy, which dominated the World Cup in the 1930s but was defeated in World War II. Moreover, FIFA bans government interference in its countries' football associations.
15. Hirshleifer and Shumway 2003; Meier, Schmid, and Stutzer 2016.
16. Achen and Bartels 2017, 2018; Fowler and Hall 2016.
17. Alrababah et al. 2019; Depetris-Chauvin, Durante, and Campante 2020; Mousa 2020.

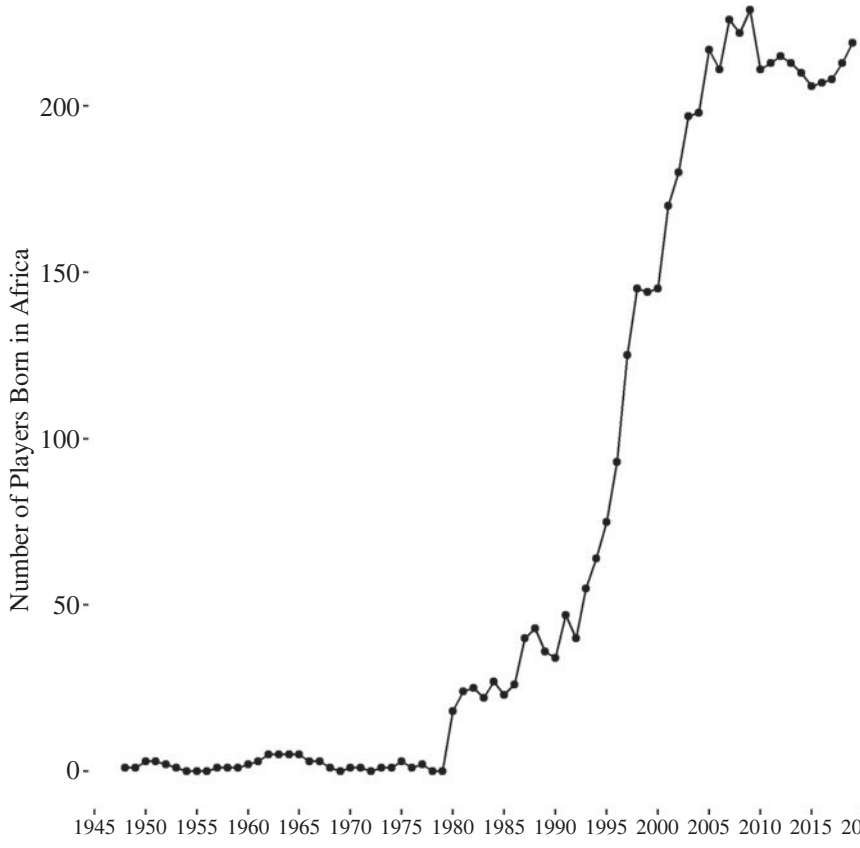
Africa and European Professional Football

European professional football is an important entertainment in the daily lives of Africans. Partly due to the relative lack of alternatives, soccer is the most popular sport in Africa.[18] This feature distinguishes it from Asia, where other sports are also popular, and also from Latin America, which has vigorous domestic leagues. According to a 2011 survey, 71 percent of people in Africa have an interest in soccer.[19] And around 276 million people in Sub-Saharan Africa—about a quarter of the entire population—regularly watch the Premier League.[20] Moreover, as the anecdotes at the beginning of this paper reflect, many Africans are devoted fans of European professional football. As Ka, Od, and I describe, "The European football fellowship is a new 'religion', another kind of 'ethnicity', and other form of 'ritual'."[21] Due to the absence of cable networks and the high cost of good display devices, most Africans use so-called viewing centers, where they pay an entrance fee to view live football games on satellite television (or over the Internet from the 2010s). Because of the zealous fandom, ubiquitous coverage on satellite television, and low entrance fees, since the late 1990s viewing centers have spread across the continent, even to rural villages.[22]

Fandom's rapid growth is the flip side of the increasing presence of African players in European football. In 1970 only one African player played in the top five leagues; in 2005, there were 217 who did (Figure 1). In the period of our analysis (the 2005 to 2018 seasons), 990 Africans play in the top five leagues, 79 percent of games have at least one African player, and two players per game originally come from Africa. Among those players, 63 percent are from Western Africa, followed by Eastern (24%), Northern (10%), and Southern Africa (3%).[23] Similarly, 47 percent of them play in Ligue 1, followed by Serie A (16%), Premier League (14%), Bundesliga (11%), and La Liga (11%). These patterns reflect soccer's overwhelming popularity in Western Africa,[24] where most countries are former French colonies.

In this paper, we focus on the results for football teams to which an African player belongs. People can support teams without African players, but the presence of co-national players is a strong indicator of fandom. For example, the popularity of Chelsea in Côte d'Ivoire is often attributed to the presence of national players, such as Didier Drogba and Solomon Kalou.[25] Similarly, the presence of Nwankwo Kanu is said to make Arsenal popular in Nigeria.[26] Because this is an important

18. Ka, Od, and I 2018; Kombol and Kombol 2015.
19. Malik 2021.
20. Monks 2016.
21. Ka, Od, and I 2018, 2.
22. Kombol and Kombol 2015.
23. In Appendix A1, we also provide the number of players from each country.
24. Ungruhe and Schmidt 2020.
25. Kwenda 2015.
26. Ibid.



Notes: The figure shows the numbers of players who were born in Africa and belong to teams in the top five European leagues

FIGURE 1. *The rapid increase of African players in European professional football*

assumption in the interpretation of our findings (though we did not come up with compelling alternatives), we test the relationship in Appendix A2. We find that the presence of co-national players is indeed a significant predictor of fandom. Later, we also check robustness by analyzing players' appearance in games.

The Psychological Origins of Social Conflicts

We argue that the results of football games provide a psychological cue and thus affect the likelihood of conflict. To be clear, we do not argue that football games would cause full-fledged civil wars, which depend on organizational and strategic

dynamics. However, social conflicts (like peaceful demonstrations and violent riots) depend on individuals' participation and are hence subject to people's cognitive biases. In fact, many protests are small and thus do not require collective action.[27] Among the protests occurring in Africa in 1990 to 2017, 37 percent had less than a hundred participants, and only a third had more than a thousand.[28]

Importantly, psychological explanations are complementary to standard theories of protests. For instance, deviant behavior by even just a few individuals can change the strategic calculus of other rational people or organizations, and thus spark large-scale protests. As Kuran states, the participation of a few deviant people can marginally improve the prospect of a protest and thus induce other rational people to join in, which in turn may lead to the participation of even more.[29] This can quickly snowball into unexpectedly large protests.[30] Similarly, the results of football games can provide opposition parties or movements with psychological cues, which can be used to mobilize people for protests.[31] Thus, even though psychological cues may or may not affect armed conflicts specifically, they can cause both small- and large-scale protests.[32]

Null Expectation: Rational Update

From the perspective of a standard *rational update* model,[33] the results of professional football games in Europe should not affect protests in Africa 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 Europe would only become meaningful over decades. This means that those policies do not immediately reveal the politicians' competence. Thus, any immediate effect of European professional football should be explained by reasons other than rational update.

Mechanism 1: Mood

A straightforward explanation is that the results of the football games influence moods—that is, transitory feeling states[34]—which in turn affect the likelihood of protests. On the defeat of a favored team, people can lose excitement and confidence, and

27. Harris and Hern 2019.
28. Salehyan and Hendrix 2017.
29. Kuran 1991.
30. Bikhchandani, Hirshleifer, and Welch 1992; Chenoweth and Belgioioso 2019.
31. McAdam, Tarrow, and Tilly 2003.
32. This certainly does not mean that the effects of European football would be limited to protests; as numerous studies show, sports events have broad consequences (see note 11). Our argument is that sports events can affect even costlier political actions—protests.
33. Ashworth, Bueno de Mesquita, and Friedenberg 2018; Lohmann 1993.
34. Hirshleifer and Shumway 2003; Meier, Schmid, and Stutzer 2016.

feel sad or even angry. Although people can vent their negative moods through various means (for example drinking, gambling, crime, and violence),[35] joining protests can also alleviate them.[36] Conversely, a victory for their team can relieve people's negative moods and provide positive feelings of enthusiasm and elation, and hence halt protests. Thus, the *mood mechanism* predicts that the loss (win) of an African player's team will increase (decrease) protests in his original country.

The literature on social psychology, however, implies asymmetric effects. The basking-in-reflected-glory and cutting-off-reflected-failure theses state that people tend to associate themselves with a glorious outcome of their favored team and thus obtain positive moods, while they tend to discount such an association to maintain their mental status when their favored team experiences a failure.[37] The *asymmetric mood mechanism* therefore predicts that winning games foster positive moods and hence reduce protests, while losing games have no effect.

Mechanism 2: Attribution

Other (not mutually exclusive) mechanisms are based on misattribution—that is, unjustified credit and blame. While the mood mechanism primarily pertains to emotion, attribution relates to (distorted) 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.[38] Although direct misattribution may or may not be realistic, attribution can also be indirect.[39] Because contemporary political processes are complicated, people often use their subjective welfare heuristically to evaluate politicians' performance. From this perspective, losing matches can hurt the subjective welfare of people, which in turn can be blamed on politicians and thus lead to protests. Or, on the victory of favored teams, people might give politicians undeserved credit, forgiving their objective incompetence.[40] The *misattribution mechanism* predicts that losing games will trigger protests while winning games will curb them.

The social psychology literature implies asymmetric effects. *Success/failure bias* refers to the human tendency to blame others for failures (even when they are not responsible) while crediting successes to oneself (even when one is not responsible).[41] People can then perceive their favored team's victory as a personal success and thus may not credit politicians (or refrain from protesting), while they directly or indirectly attribute their favored team's losses to domestic politicians (and join protests). This egoistic attribution, which we call the *asymmetric*

35. See note 11 for previous studies.
36. Jasper 2011.
37. Cialdini et al. 1976.
38. Gilbert and Malone 1995 call this the correspondence bias.
39. Achen and Bartels 2017 call this blind retrospection; see note 8 for debate.
40. Healy, Malhotra, and Mo 2010.
41. Miller and Ross 1975.

misattribution mechanism, suggests that losing games will cause blaming and protests, but winning games will not improve people's attitudes toward politicians or reduce protests.[42]

Mechanisms 3: Identity

Finally, the glory or misery shared with co-national players may unite fans under the national identity, which in turn may make existing ethnic cleavages less salient and halt protests, especially those related to ethnic issues.[43] As the rally-around-the-flag effect suggests,[44] both collective glory and suffering unite people. The *rally mechanism* predicts that both winning and losing games will reduce (ethnic) protests. Table 1 summarizes the causal mechanisms and predictions.

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. One empirical challenge is that the results of European football games are not randomly assigned. For instance, better sports and education policies may be associated with both a larger number of star players from a country and greater social stability. This endogeneity might create a spurious correlation. We address this problem by using a natural experiment in both analyses: 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.[45]

Another problem is that points are scored less frequently in soccer than in other sports. As a result, one goal means a big difference, and thus even winners and losers of close games can be systematically different. In fact, even though we compare the narrowest margin of wins and losses,[46] the later balance check indicates that the natural experiment improves the covariate balances but does not do so perfectly.

In the event data analysis, we therefore follow Card and Dahl's well-established approach by conditioning on pre-game bookmaker betting odds (decimal odds).[47] Betting odds are the inverse of expected probabilities of game results (a 50% probability of winning gives 2.00 odds). Thus, to the extent that betting odds provide

42. To be sure, any attribution of this type is irrational; people unreasonably blame or praise politicians for irrelevant events.
43. Alrababah et al. 2019; Depetris-Chauvin, Durante, and Campante 2020; Mousa 2020.
44. Mueller 1970; Ramos and Sanz 2020.
45. Lefgren et al. 2019.
46. Games with penalty shoot-outs (only in the Champions League) are also included.
47. Card and Dahl 2011. See Anderson 2017 for methodological details, and note 11 for applications.

accurate predictions of game results, we can directly observe the treatment assignment probability 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 "unexpected" and thus as-if random. Moreover, because inaccurate betting odds can cause large financial losses, bookmakers have strong incentives to measure them accurately.[48] Even though it is sometimes difficult to predict the results of football games, our design is valid to the extent that the bookmakers make an accurate prediction *or* the result of a close game is random. As a robustness check, we limit the comparison to close games of similar betting odds *and similar numbers of shots on target* to focus on even closer games.

TABLE 1. *Causal mechanisms and predictions*

		Predicted effect onprotests	
	Mechanism	*Loss*	*Win*
Information	Rational update	0	0
Mood	Mood	+	−
	Asymmetric mood	0	−
Attribution	Misattribution	+	−
	Asymmetric misattribution	+	0
Identity	Rally	−	−

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

We also use the difference-in-differences (DiD) by taking the first difference of the outcome variable between a few days before and after a football game. This eliminates confounders that do not change over that period (for example, reporting biases in conflict data). Because DiD can overstate effect sizes,[49] we also report the results without first differencing and those with lagged dependent variables (LDV) models in a robustness check.[50]

Finally, we account for potential reverse causality (anticipation of protests might affect player performance and violent plays, which in turn could affect game

48. Wunderlich and Memmert 2018.

49. Angrist and Pischke 2009.

50. Even though recent studies have proposed various extensions of DiD (see Xu 2022 for a review), especially those related to the problems in two-way fixed effects (de Chaisemartin and D'Haultfœuille 2020; Imai and Kim 2019, 2021; Imai, Kim, and Wang, 2021), DiD is the least important element in our identification (the natural experiment and conditioning are the critical parts). Moreover, we use a simple two-by-two DiD, which is free from the problems related to two-way fixed effects. Finally, as later analyses indicate, the effects of football games disappear in three days, so it is unlikely that the effect of a game would be carried over to the next game.

results)[51] as well as alternative interpretation (policies in African countries might affect the performance of African players and thus game results) by controlling for player performance in a robustness check.[52] The remaining variation in the game results comes from the performance of the African players' teammates. It is unlikely that the performance of non-African players would be affected by conflicts or policies in an African country.

Sample and Unit

The unit of analysis is a pair of a player *i* and a football game *j*. Figure 2 illustrates the 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 game *j*.[53] 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 game *j*—is also uniquely defined. We use a player, instead of his birth country, as a unit to control for individual performance in a robustness check.[54] Although the event data analysis 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.

Our sample includes all players whose birthplaces are African countries, and the football games in the top five European leagues (La Liga in Spain, Premier League in England, Serie A in Italy, Bundesliga in Germany, and Ligue 1 in France) and the Champions League (group and knockout stages; only teams from the top five leagues) between the 2005–06 and 2018–19 seasons.[55] In the following analysis, we drop 272 games in which both sides had players from the same African country,[56] and limit the sample to close games.[57] The resulting data contain 61,527 observations, including 951 players from 40 African countries in 184 teams playing 15,087 close games. For simplicity, we split the sample into the games in which African players' teams drew or closely lost ($N = 43{,}984$), and the games in which they drew or closely won ($N = 43{,}084$).[58] The summary statistics are given in Appendix A4.

51. Miguel, Saiegh, and Satyanath 2011.
52. We control for African players' goals, assists, yellow cards, and red cards in a robustness check.
53. We use birth countries because each player has only one birth country and thus the data configuration becomes simple. We later conduct a robustness check using player citizenship.
54. If there are multiple players from a given African country on 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 the country-game level.
55. The betting odds data are available only after 2005, and COVID-19 started in 2020.
56. We later conduct a robustness check by further dropping observations if there are both close losses and wins on the same day for a given country.
57. We later conduct a robustness check by including non-close games as well.
58. The sample contains (1) games in which African players' teams closely won, (2) draw games, and (3) games in which African players' team closely lost. The split is then between (1) + (2), versus (3) + (2). We split the sample because matching on a trichotomous variable is not straightforward.

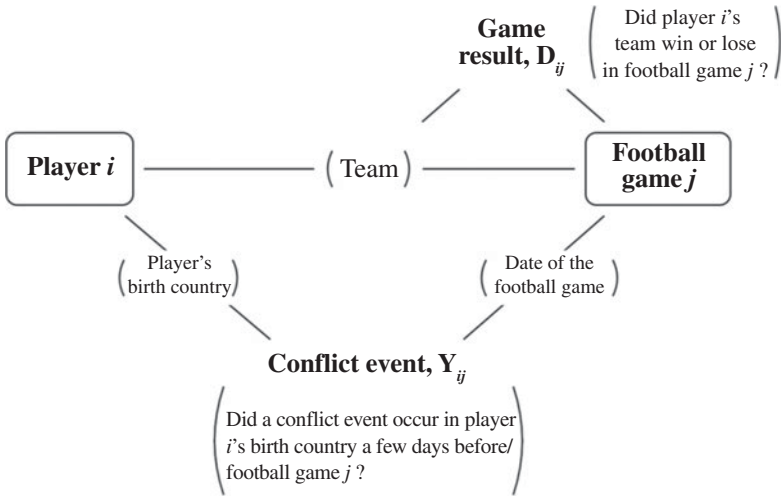


FIGURE 2. *Unit of analysis and variables*

Treatment Variable

The treatment variable D_{ij} is a dichotomous variable that is 1 if African player i's team loses or wins in game j and 0 if the result is a draw. We code the treatment status based on African players' team affiliations instead of their appearance in games.[59] To be sure, we later conduct a robustness check by splitting the sample into those with and without African players' appearance. The data were scraped from Transfermarkt.com on 7 April 2021.

Outcome Variables

The outcome variable ΔY_{ij} is the difference in the daily probability (%) of conflict incidence in player i's birth country before and after football game j: $\Delta Y_{ij} = 100\left(\frac{\sum_{t\in\{1,\ldots,T\}} y_{ijt}}{T} - \frac{\sum_{t\in\{-T,\ldots,-1\}} y_{ijt}}{T}\right)$, where y_{ijt} is a dummy of conflict incidence in player i's birth country t days after game j, and T is a time window.[60] We omit observations on the day of the game ($t = 0$) because the treatment status is indeterminate (hours before a game are controlled, while hours after a game are treated). We use a three-day window ($T = 3$) because the games should have only

59. In Appendix A2, we show that the affiliation of an African player with a team significantly increases the number of fans in Africa.

60. We later conduct a robustness check by using alternative measures of the outcome variable.

temporary effects and because a three-day window effectively contains an entire week (3 + 1 + 3 = 7).[61] In robustness checks, we use different time windows.

Incidences of protest are derived from Armed Conflict Location and Event Data (ACLED) and the Social Conflict Analysis Database (SCAD), which are standard data sets of conflict events.[62] For ACLED, we include all demonstrations and riots if the exact date information is available.[63] 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).[64] Although SCAD contains richer information about each event, it substantially underreports the number of events. For instance, while according to ACLED, about 10 percent of the observations experienced demonstrations within three days after the football game, the number is less than 3 percent in SCAD. Moreover, SCAD does not contain information about the precision of event dates,[65] which is crucial for our daily-level analysis, and the data set 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 target, issue, and size.

Although both data sets 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."[66] 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 on pre-game betting odds in the top five leagues are BetBrain's average bookmaker betting odds from Football-Data.co.uk (scraped 1 January 2022). These are averages of twenty bookmaker websites, providing the most consistent data on betting odds for every football game played in the 2005–2019 period. Because the data set does not cover the Champions League, we supplement it with Odds Portal's average betting odds (<https://www.oddsportal.com>, scraped 5 April 2021). The treatment assignment probability (%) is calculated as $P_{ij} = 100\frac{\text{odds}_{\text{draw},ij}}{\text{odds}_{v,\,ij}+\text{odds}_{\text{draw},ij}}$ for $v \in \{\text{loss, win}\}$ and game j of player i's team.

61. Because football games are played every week, time windows larger than 3 result in overlaps.
62. ACLED 2019; Raleigh et al. 2010; Salehyan and Hendrix 2017.
63. For the definitions and measurements, refer to ACLED 2019. Exact dates are available for 89 percent of the protest and riot events.
64. For definition and measurement, refer to Appendix A3 and to Salehyan and Hendrix 2017.
65. The event dates in SCAD are best estimates by the coders. But SCAD does not indicate the precision of these estimates.
66. Weidmann 2016, 208.

Specification

With the directly measured assignment probabilities, we conduct nearest-neighbor matching without replacement.[67] We use the unstandardized caliper size of five percentage points (we later conduct robustness checks with different calipers). The matching yields 12,168 losing and draw games (91% of the sample) and 11,943 winning and draw games (90% of the sample). The resultant samples include 35,481 and 34,033 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:

$$\text{LATT} = \text{E}[\Delta Y_{ij} | D_{ij} = 1,\ P_{ij} \approx p_{ij}] - \text{E}[\Delta Y_{ij} | D_{ij} = 0, P_{ij} \approx p_{ij}] \qquad (1)$$

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 on a value of p_{ij}.

Because our design accounts for confounders, we do not use fixed effects in the main analysis and leave it to robustness checks. The unit fixed effects are redundant because DiD accounts for static factors. Time fixed effects (year, month, week, day of the week) are also unnecessary as far as the treatment is as-if randomly assigned. For instance, even though the football games are usually played on weekends, we compare losing, winning, and draw games. As a result, the weekend effects are canceled out in the comparison.[68] This means that day-of-the-week fixed effects are redundant and only reduce the power of analysis. In fact, as we show in robustness checks, the estimates are nearly identical across various specifications with fixed effects. The fixed effects also make the estimates less interpretable and more dependent on modeling assumptions,[69] exacerbate the attenuation biases of measurement errors,[70] and limit external validity.[71]

The standard errors are two-way clustered by country and game. Clustering by country accounts for the cases where players from the same country belong to different teams, thereby yielding repeated values of the outcome variable. Clustering by game accounts for situations in which multiple players from Africa belong to teams in the same game, which also yields repeated values of the treatment variable.

67. Because the assignment probabilities are directly measured, we use the simple matching method.

68. In equation (1) the weekend effects, say ρ, exist both in the first and second terms of the right-hand side, and thus they cancel out ($\rho - \rho = 0$).

69. Imai and Kim 2019, 2021; Imai, Kim, and Wang, 2021.

70. Angrist and Pischke 2009, 225.

71. Hill et al. 2020. For similar reasons, we use DiD instead of the lagged dependent variable model, even though DiD can overstate effect sizes (Angrist and Pischke 2009; Keele 2020).

Event Data Analysis: Results

The analyses with the main specification indicate that both barely losing and barely winning games increase the likelihood of demonstrations (Table 2). Substantively, a close loss (win) increases the probability of demonstrations by 0.67 (0.62) percentage points. Because the average value of the outcome is 0.60 percentage points, the close loss and win double the 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.[72] The null effect on battles is consistent with our argument that armed conflicts are driven by organizational and strategic dynamics. The null or even slightly negative effect on riots implies that emotional cues can drive people to start protests, but they may avoid risky options and use relatively safe means, which is consistent with some of the previous findings about rationality in irrational behaviors.[73]

Figure 3 shows the results without first differencing. The outcomes are the incidence of demonstrations on each day from the game. The effects of close losses are pronounced one to three days after the game, and there is no significant effect on demonstrations before the game. By contrast, the effect of close wins is not statistically significant, and there are subtle (perhaps random) decreases in event probability one to two days *before* a game. DiD magnifies the subtle differences and makes the estimate positive in Table 2.[74] In fact, the effect of winning disappears in a later robustness check with an LDV model, which provides a conservative bound.[75] Overall, we find that barely losing games increase demonstrations, while the effect of winning games is uncertain. These results are not consistent with the rational update, asymmetric mood, or rally mechanism, leaving the mood, misattribution, and asymmetric misattribution mechanisms as possibilities.

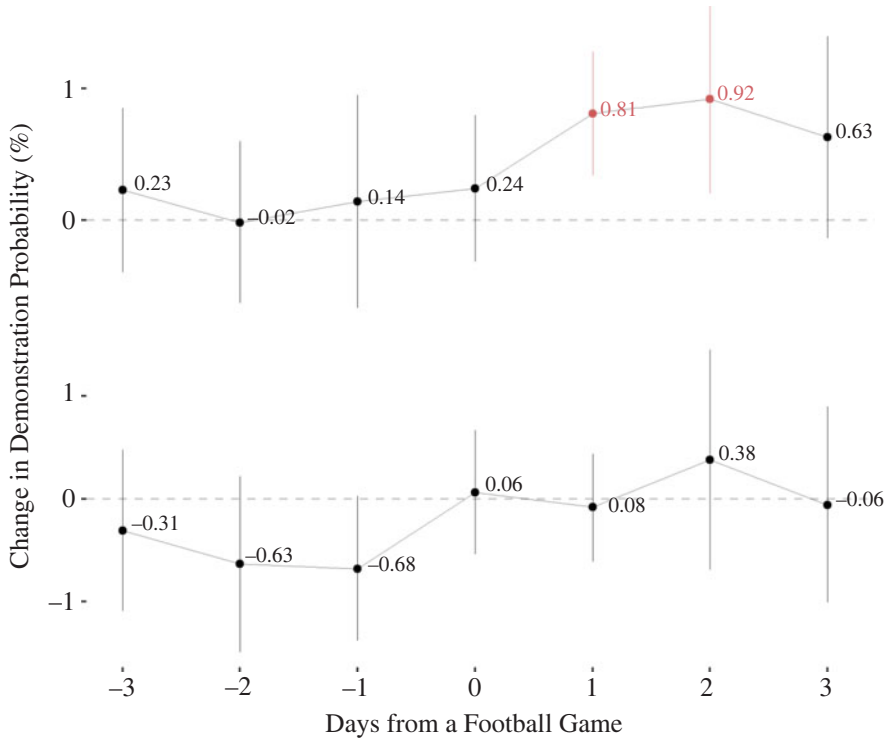
We also disaggregate the targets, issues, and sizes of demonstrations by using SCAD, which provides more detailed information about each event.[76] The findings hold with this alternative data set (first column of Figure 4). Moreover, the effects of close losses are particularly large for non-ethnic demonstrations targeted at a central government.[77] We do not see differences due to the size of the demonstration. These results are inconsistent with the rally mechanism (null effect on ethnic demonstrations) and provide suggestive evidence for the attribution mechanisms (the existence of targeting). Because central governments are more visible, they can be an easy target of misattribution.

72. The average values for riots and battles are 0.67 and 0.44, respectively.
73. Ge, Barbieri, and Schneider 2021.
74. See Angrist and Pischke 2009, 243–47 and Keele 2020 on the problems of DiD.
75. Angrist and Pischke 2009.
76. For details of the event categories, see Appendix A3.
77. We obtain similar results on ethnic conflicts with ACLED (Appendix A7).

TABLE 2. *Effect of close losses and wins on conflict incidence*

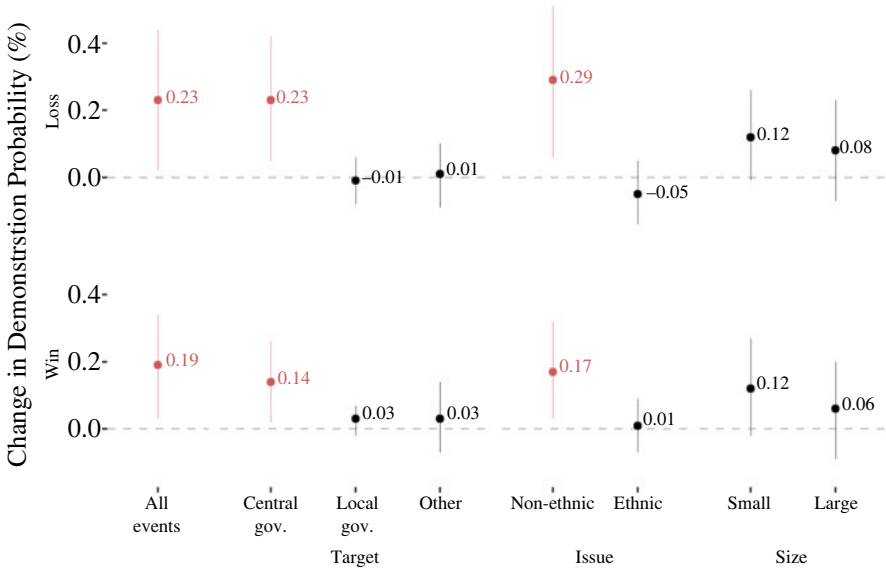
	ΔDemonstrations		*ΔRiots*		*ΔBattles*	
Close loss	0.67*		−0.31†		−0.20	
	(0.24)		(0.18)		(0.24)	
Close win		0.62*		−0.38		−0.04
		(0.29)		(0.23)		(0.36)
N	35,481	34,033	35,481	34,033	35,481	34,033

Notes: Coefficient estimates, with corresponding standard errors in parentheses. The standard errors are two-way clustered by player's birth country and game. The sample includes 12,168 (11,943) games, 931 (929) players, 184 (184) teams, and 40 (40) countries for the comparison of losing (winning) and draw games. *$p < .05$; †$p < .10$.



Notes: The figure shows the estimated effect of close losses (*top*) and wins (*bottom*) on the likelihood of demonstrations (%) for a range of days before and after a football game. The vertical bars are 95% confidence intervals.

FIGURE 3. *Effect by days from a football game (event data analysis)*



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

FIGURE 4. *Effect by target, issue, and size of demonstration (SCAD data)*

Assumption Checks

To check the validity of the assumptions, we first regress the treatment variable on the assigned probabilities based on the betting odds (simple linear regression; Table 3). If the betting odds are accurate measures of assignment probability, the coefficient should be close to 1, while non-systematic measurement errors can cause biases toward 0. If the coefficient is larger than 1 or negative, it indicates the existence of systematic measurement errors and thus invalidates the design. In samples of all games, the coefficients are indeed close to 1, indicating the accuracy of the measurement. Predicting close games is slightly more difficult, reflecting the randomness of their results.

Second, we check whether African players' teams in the matched pairs have similar characteristics in a covariate that is not used in the matching.[78] To this end, we compare their differences in rank within football leagues. The team ranks are stan-

78. Matching eliminates differences in the inverse of betting odds. The mean differences are 0.16 percentage points for close losses and draws and −0.02 percentage points for close wins and draws.

TABLE 3. Prediction of game results by observed assignment probability

	Loss		*Win*	
Assignment prob. (loss)	0.98*	0.71*		
	(0.03)	(0.03)		
Assignment prob. (win)			0.87*	0.67*
			(0.03)	(0.04)
Sample	All games	Close games	All games	Close games
N	61,476	43,984	59,447	43,084

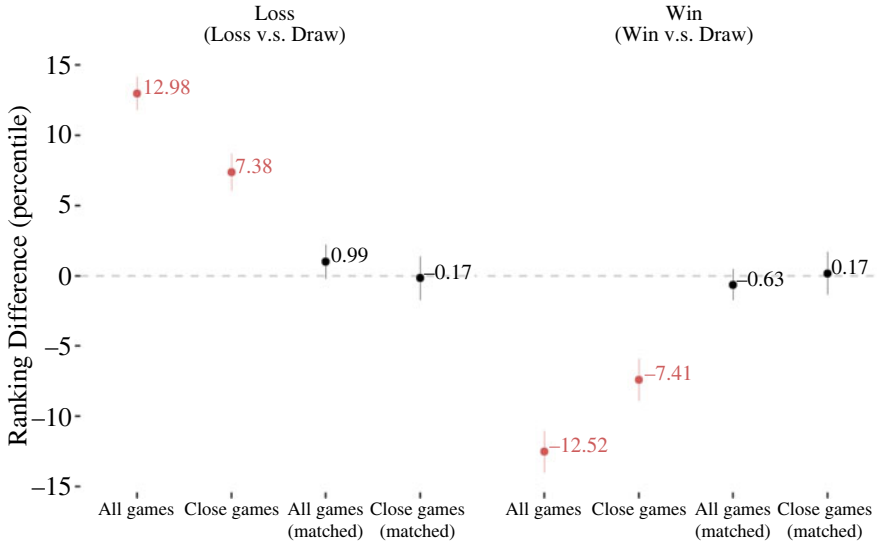
Notes: Coefficient estimates, with corresponding standard errors in parentheses. The samples are those before matching. The standard errors are two-way clustered by player's birth country and game. *$p < .05$; †$p < .10$.

dardized to a 0–100 percentile scale (the smaller the number, the higher the rank), and from the rank of a losing or winning team we subtract the rank of a matched team in a draw game. Teams with high ranks are more likely to win and less likely to lose (Figure 5). Subsetting to close games shrinks the differences (second column of each pane), but there remain differences of over seven percentile points. Matching on betting odds, however, greatly reduces the differences. Even with non-close games (third column of each pane), the imbalances become small, though the team ranks are still different by one percentile point and the *p*-value is only marginally above the conventional threshold ($p = .12$). Combining the natural experiment and matching (last column of each pane) eliminates the remaining imbalances.

Finally, we conduct placebo tests. In Appendix A5, we confirm that close losses or wins have null effects on past demonstrations (in-time placebos). In Appendix A6, we also find that an African player's game has null effects on demonstrations in countries other than his original country (African countries except for his original country, and countries in another continent; in-place placebos). These results lend further credence to the main findings.

Effect Heterogeneity

We also check 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 in the Champions League (high-stakes games). Figure 6 reports the estimated effects of close losses and wins when we subset the samples by the terciles of players' season appearances (*left*) and by league (*right*). In the left pane, the results are maintained only when players are regularly on the pitch. In the right pane, the point estimates are over three times larger for the Champions League, though the effects are not statistically significant due to the small sample size (only about 3% of the observations are games in the Champions League). In Appendix A8, we further assess effect heterogeneity by splitting the Champions League games into group and knockout stages, looking



Notes: The figure shows the average team rank of losing (left) or winning (right) teams minus that of teams in draw games. The teams' ranks are standardized to 0–100 (smaller numbers mean higher rank).The vertical bars are 95% confidence intervals

FIGURE 5. *Differences in pre-game team ranks*

at the teams' participation in the Champions League, and incorporating the popularity of football teams in African countries.[79]

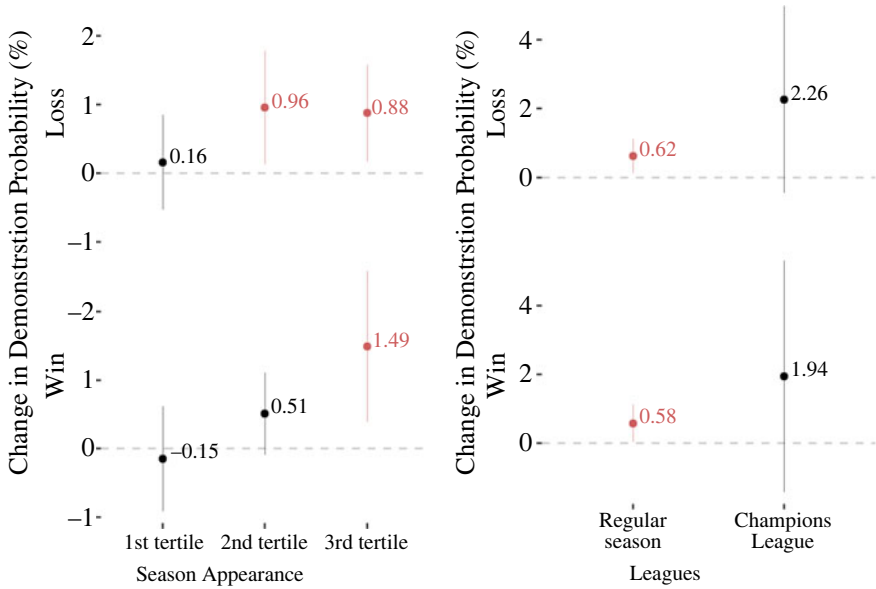
Robustness Checks

Finally, we conduct a series of robustness checks, which are summarized in Table 4 and detailed in Appendix A11. The results of losing games are robust to those changes, while the results of barely winning games are less stable. Importantly, when we use the LDV model, which provides a conservative bound on the causal effect,[80] the effect of winning becomes null, and the point estimate shrinks to one-third. These results imply that our findings about winning could be an artifact of DiD.

Overall, the event data analysis indicates that barely losing games increase peaceful demonstrations, while the effect of barely winning games is uncertain. These

79. The effect heterogeneity by prior expectations (reference dependence; Köszegi and Rabin 2006) is reported in Appendix A9. The effect heterogeneity by the margins of losses and wins is reported in Appendix A9 as well. The effect heterogeneity by leagues, regions, and time periods is reported in Appendix A10.

80. Angrist and Pischke 2009.



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

FIGURE 6. *Effect heterogeneity (event data analysis)*

results are consistent with the mood, misattribution, and asymmetric misattribution mechanisms (Table 1). The additional analyses—especially those about targets—provide some support for the asymmetric misattribution mechanism. The caveats are that we have not yet directly quantified attribution, moods, or identity, and that the country-level analyses can suffer from aggregation biases. In the following sections, we therefore analyze individual-level surveys.

Survey Analysis: Research Design

We examine the effect of the football games on mood, attitude toward politicians, and nationalistic sentiment by analyzing individual-level surveys from Afrobarometer.[81] However, as we mentioned, the natural experiment with close games is useful but insufficient for causal identification. Moreover, conditioning on pre-game betting odds is not reliable because there are so few football games during the periods of the Afrobarometer surveys.

81. Afrobarometer 2019.

TABLE 4. Robustness checks (event data analysis)

	Loss	*Win*	*Appendix*
Analysis based on player citizenship	+*	+*	Table A11-1
Subsetting to games with African players' appearance	+*	+*	Table A11-2
Omission of cases where there are both close losses and wins on the same day for a country	+*	+*	Table A11-3
Aggregated analysis at a country-game level	+*	+*	Table A11-4
Different transformations of the outcome	+*	+*	Table A11-5
Lagged dependent variable model	+*	null	Table A11-6
Matching on assignment probability and number of shots on target	+*	null	Table A11-7
Inclusion of non-close games	+*	null	Table A11-8
No matching	+*	+*	Table A11-9
Control for player performance, violent plays, and betting odds	+*	+†	Table A11-10
Time fixed effects	+*	+*	Figure A11-1
Player-specific fixed effects	+*[1]	+*[2]	Figure A11-2
Different caliper sizes	+*	+*	Figure A11-2
Different time windows	+*[3]	+*[4]	Figure A11-3
Leave-one-country-out tests	+*	+*[5]	Figure A11-4

Notes: *$p < .05$; †$p < .10$.

1. Significant at a 10% level with player-specific week fixed effects, and null with all of the player-specific time fixed effects.
2. Significant at a 10% level with player-specific year fixed effects.
3. Statistically significant at a 10% level for $T = 1$.
4. Null for $T = 1$.
5. Significant at a 10% level for 4 out of 40 cases.

We address these problems by exploiting as-if random coincidences of survey interviews and football games. That is, we compare people who are interviewed just *a few days* before or after a close football game.[82] Thus, the critical assumption is that the dates of survey interviews are not affected by the results of close football games in Europe. As Depetris-Chauvin, Durante, and Campante 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."[83] The dates of the games are also set at the start of each season and are unlikely to be affected by the Afrobarometer surveys. But because the 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

82. Muñoz, Falcó-Gimeno, and Hernández 2020.
83. Depetris-Chauvin, Durante, and Campante 2020, 1581. See Afrobarometer 2019 for details of survey implementation. See also Eifert, Miguel, and Posner 2010.

like those in the event data analysis.[84] We then link a survey respondent to the football game if the interview was held within three days before or after the game (we also use different time windows in robustness checks), and if players from the respondent's country belong to either team in the game.[85] 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 in 20 (21) teams between the 2005–06 and 2018–19 seasons.[86] Due to the lack of survey interviews held within the time window, the samples do not cover the Champions League. There are 10,398 observations for the sample of close losses and draws and 9,410 observations for the sample of close wins and draws. The summary statistics are available in Appendix 12.

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 African player i's team barely loses or wins game j and 0 if the result is a draw.[87] The second treatment variable, R_{jk}, takes a value of 1 if respondent k was interviewed after 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 on Depetris-Chauvin, Durante, and Campante,[88] we select eighteen Afrobarometer indicators that are relevant to the causal mechanisms and available in a majority of the survey rounds in the period of analysis (2005–2019; third to seventh rounds).[89] For the attribution mechanisms, we select eight indicators about trust in a leader (president or premier), members of parliament (MPs),

84. We later conduct a robustness check by using players' citizenships.

85. If there are multiple games within three days before or after an interview, we include the interview only if all of the games are played *either* before or after the interview. (If there are games *both* before and after an interview, the treatment 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 in the density tests.) We then select the nearest games and assign their treatment status 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.

86. As in the event data analysis, we drop games if both sides have players from the same country in Africa. We later conduct a robustness check by dropping observations where there are both close losses and wins on the same day for a given respondent. We also drop observations on the day of the game because their treatment status is indeterminate. Finally, we drop a game if all relevant interviews were conducted either before or after the game, because all respondents are either treated or control units.

87. We later conduct a robustness check by splitting the sample into those with and without African players' appearance.

88. Depetris-Chauvin, Durante, and Campante 2020.

89. The exact survey questions are given in Appendix A13.

local councils, ruling parties, opposition parties, police, army, and courts, as well as three indicators of the performance 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 friendliness, interest in the survey questions, cooperativeness, patience, ease, and honesty. Although ideally we would like to directly measure the positive and negative moods (e.g., excitement, elation, sadness, and anger) as in experimental studies,[90] the direct measures are not available. We therefore use the indirect attitudinal measures.[91] Even though each item does not directly relate to mood and only captures respondents' positive attitudes, respondents' moods can be reflected in their attitude, and thus the attitudinal measures can *collectively* capture respondents' mood. For this reason, we aggregate the attitudinal measures by calculating the average ("overall mood"). Although this measurement may not be perfect, we believe it is the best available in our observational setup. Finally, following Depetris-Chauvin, Durante, and Campante, 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 to 10 range.[92]

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:

$$\begin{aligned}\text{LATT} = (&\text{E}[W_k|D_{ij}=1,\ R_{jk}=1] - \text{E}[W_k|D_{ij}=1,\ R_{jk}=0]) \\ - &\ (\text{E}[W_k|D_{ij}=0,\ R_{jk}=1] - \text{E}[W_k|D_{ij}=0,\ R_{jk}=0]).\end{aligned} \tag{2}$$

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 game player i's team played within h days before or after respondent k's interview. For the reasons we mentioned in the event data analysis, the time window is set to three days ($h = 3$; see the Specification subsection of the event data analysis and note 61), and we conduct robustness checks with smaller windows. The observations are triangularly weighted by the days from a football game (respondents closer to games have larger weights). Even though this can also be considered a regression discontinuity design (RDD) with a discrete running variable (that is, survey date), we do not include the running variable because the time window is small and thus the regression can cause

90. Busby and Druckman 2018; Busby, Druckman, and Fredendall 2016.

91. Unlike Depetris-Chauvin, Durante, and Campante 2020, we do not use respondents' evaluation of the economy as a measure of mood. As we show in an additional analysis, the item is more closely related to the evaluation of welfare—a key factor in indirect attribution.

92. We also standardize the outcomes, and it does not change the results.

overfitting problems (see later robustness checks).[93] Finally, for the reasons we mentioned in the event data analysis (see the Specification subsection of the event data analysis), we do not use fixed effects or control variables and leave it for robustness checks.

We address the problems of multiple hypothesis testing by controlling the false discovery rate.[94] The standard errors are two-way clustered by country and game. As in 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 note 85).

Survey Analysis: Results

Figure 7 reports the results of the survey analysis.[95] 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 (Figure 7, *left*). Because the average value of trust in a leader is 5.33, a close loss decreases it by 23 percent. Barely losing games also sours people's attitudes toward MPs and ruling parties, and, to a lesser extent, local councils and courts. The effect of close losses is nearly zero for trust in opposition parties, police, and the army. These results suggest that people tend to blame visible politicians (leaders, MPs, ruling parties) who are supposed to be responsible for their 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 0.28 points, the effect is not statistically significant, and the effects on some of the other outcomes (for example, 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 misattribution mechanism.

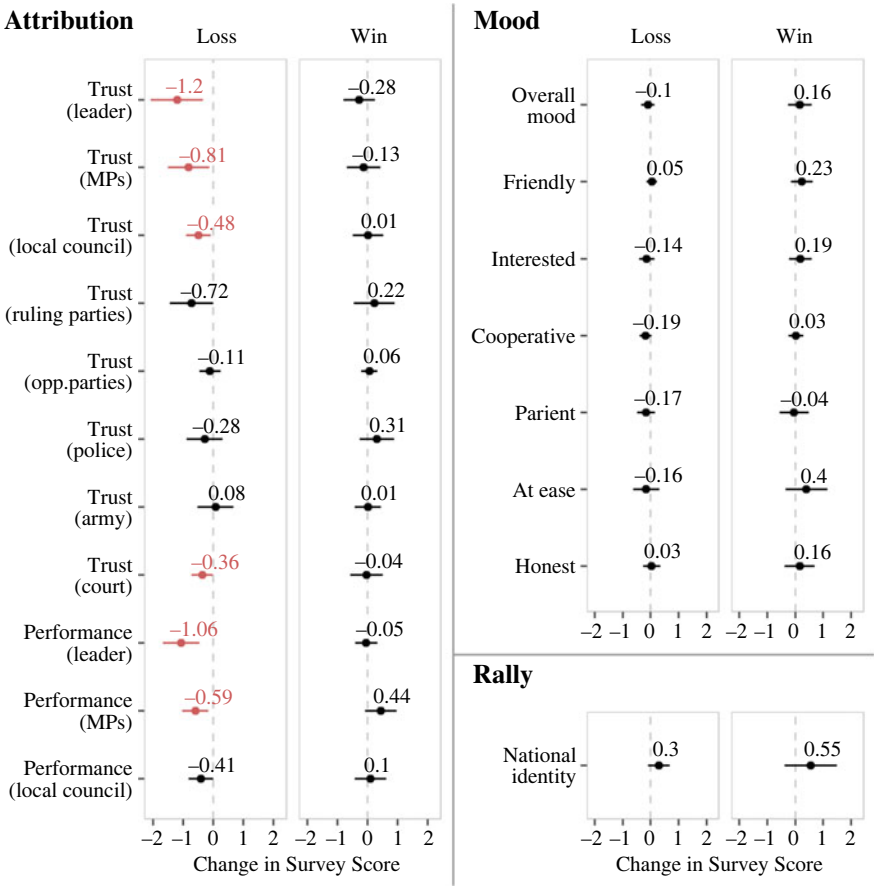
Turning to the mood mechanisms (Figure 7, *top right*), we do not see any large effect on respondents' observed attitudes. In fact, most of the point estimates and confidence intervals are tightly centered around 0, indicating precise nulls. Although we cannot exclude the mood mechanisms given the limitations of the measurement, we do not find strong evidence. Moreover, the mood mechanisms cannot explain why losing matches lowers trust in a leader while not changing trust in opposition parties,

93. Unlike continuous RDDs, automatic bandwidth selection cannot be used in discrete RDDs (Imbens and Kalyanaraman 2012). Imbens and Wager 2018 and Kolesár and Rothe 2018 propose inferential frameworks for discrete RDDs, but they do not provide methods for bandwidth selection. Cattaneo, Frandsen, and Titiunik 2015 propose a covariate-based method, but the method does not account for clustering. Without clustering SEs, the null hypotheses are over-rejected.

94. Benjamini and Hochberg 1995. Using the other methods of adjustment does not change the results. Applying the same adjustment to the event data analysis also does not change the results.

95. The detailed tables are available in Appendix A14. We do not find that having a football game itself has significant effects on the outcomes.

police, or the army. Without any attribution, bad moods should lower trust in any actors.



Notes: The figure shows the effect of close losses and wins on respondents' opinion in Afrobarometer (rescaled to 0–10). The horizontal bars are 95% confidence intervals (adjusted for false discovery rates).The sample includes 10,398 (9,410) observations, 7,659 (7,011) respondents, 26 (27) games, 23 (23) players, 20 (21) teams, and 15 (12) countries for the comparison of losing (winning) and draw games. Due to missing values in Afrobarometer, the actual sample sizes can be smaller.

FIGURE 7. *Effect of close losses and wins on public opinion*

Similarly, we find only an inconclusive result for the rally mechanism (Figure 7, *bottom right*). 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 misattribution mechanism. The differential effects on political trust indicate

that people directly or indirectly attribute bad luck in football games to incumbent politicians.

In Figure 8, we decompose the effect on trust in a leader by days from the game.[96] The effect of close losses is pronounced one day after the game. This result is consistent with the event data analysis, in which the effect of close losses is large one to three days after the game (Figure 3). This means that a close loss in a football game (which is usually played in the evening) affects people's perceptions the next day ($t+1$) and then triggers demonstrations on days $t+1$ to $t+3$. The lag of up to two days may indicate the time during which tiny unreported events grow into those reported in the media.

Mechanisms

The main results are consistent with the asymmetric misattribution mechanism. However, this may or may not mean that people *directly* attribute bad luck in football games to domestic politicians. A more realistic interpretation is that losing matches reduce respondents' subjective welfare, which in turn is blamed on politicians (*indirect* attribution). We therefore analyze the effect of football games on respondents' evaluation of the future, present, and past economy.[97] Losing games significantly lowers this evaluation (Figure 9). The fact that football games affect evaluations of the *past* economy implies that the results do not capture effects on the actual economy but that the games changed respondents' *subjective* evaluation of their economic welfare.

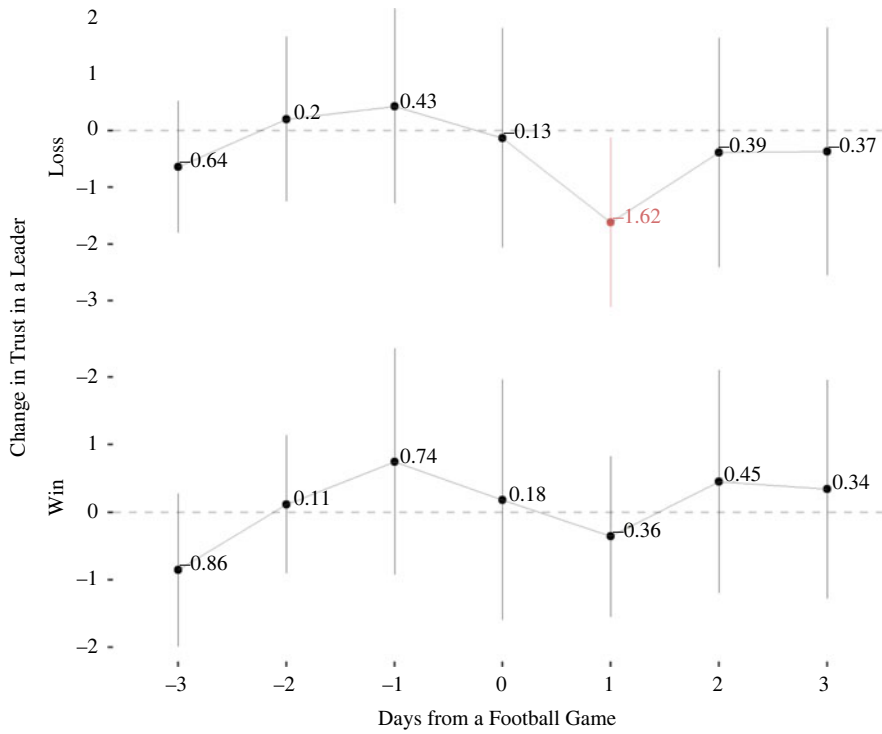
An alternative explanation is that the games attract people to viewing centers, allow them to talk with other people or politicians, and thus create opportunities for mass mobilization. We probe this channel by analyzing the effect on social interactions: the frequency of discussing politics, raising issues, and contacting government officials, MPs, or political parties. Neither losing nor winning games significantly affects the frequency of social interactions (Figure 10). Overall, even though the mobilization mechanism might be consistent with the results of the event data analysis, it is not supported by the survey data, or it cannot fully explain the asymmetric effects on leader approval.

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,

96. We estimate the effect of close losses and wins for each day from a football game. Because the natural experiment with close games alone can produce biased estimates, we reduce the estimate by the pre-game average of the estimated effect to account for any nonrandom assignment of the game results. The results of the other outcomes are provided on request.

97. See Appendix A13 for exact survey questions. See note 91 about the relationship to moods.

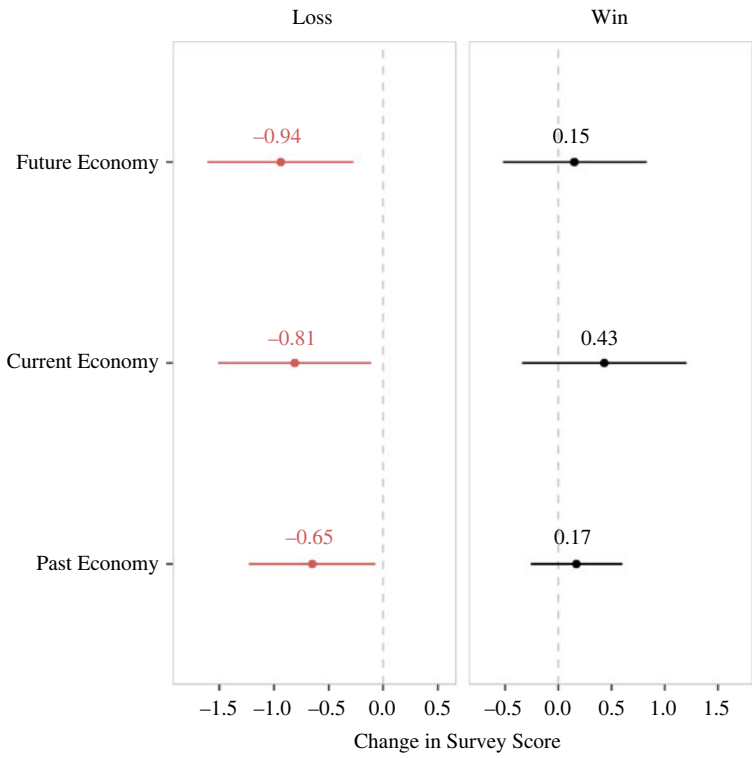


Notes: The figure shows the estimated effects of close losses (top) and wins (bottom) on trust in a leader (0–10 scale) for a range of days before and after a football game. The vertical bars are 95% confidence intervals.

FIGURE 8. *Effect by days from a football game (survey analysis)*

Christian, primary education, employment, and access to food, water, medical care, cooking fuel, and cash (all 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.[98] The last column shows the *p*-values of placebo tests, in which the outcome variable in (2) is replaced by each of the covariates. Finally, the *p*-values

98. Rubin 2001.



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

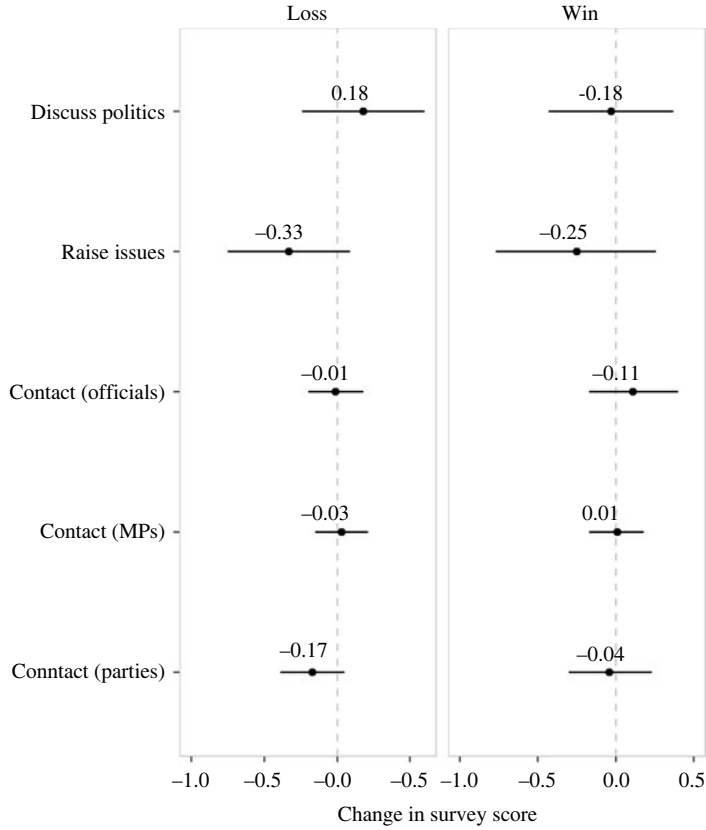
FIGURE 9. *Effect of close losses and wins on welfare evaluation*

of the density tests are reported at the bottom of each pane.[99] Although there are a few minor imbalances in four of the sixty-six balance metrics (6%), there is no consistent evidence of imbalance, and the density tests indicate no evidence of sorting. To be sure, we later check robustness by controlling for the covariates.

Effect Heterogeneity 1: Substantive Relevance

As in the event data analysis, we check face validity by subsetting to substantively relevant cases. Because the samples do not contain games in the Champions

99. In the density tests, we use the respondent as the unit of analysis because it is not straightforward to cluster the standard errors. We also include observations on the day of the football game because we cannot otherwise implement the density tests.



Notes: The figure shows the estimated effect of close losses (left) and wins (right) on frequency of discussing politics, raising issues, and contacting politicians or officials. The horizontal bars are 95% confidence intervals.

FIGURE 10. *Effect of close losses and wins on social interactions*

League (no game was played within three days before or after survey interviews), we only report the results by players' season appearances.[100] Consistent with the event data analysis, the effects of losses are large for the games of regular players, while the effects of victories are null (Figure 11).[101]

100. The other analyses on effect heterogeneity at a team level (for example, a team's participation in the Champions League or its popularity) are not feasible as the sample contains less than twenty teams.

101. The effect heterogeneity by prior expectations (reference dependence) is reported in Appendix A15. The effect heterogeneity by the margins of losses and wins is reported in Appendix A15 as well. The effect heterogeneity by regions and time periods is reported in Appendix A16.

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	2,590	7,758	36.13	37.45	−0.07	0.87	0.26
	Female	2,605	7,793	0.50	0.50	0.00	1.00	**0.08**
	Muslim	2,589	7,738	0.19	0.31	**−0.21**	0.71	0.46
	Christian	2,589	7,738	0.70	0.57	0.19	0.86	0.27
	Primary education	2,597	7,765	0.81	0.81	−0.01	1.02	0.73
	Employed	2,598	7,756	0.29	0.33	−0.07	0.93	0.59
	No food	2,601	7,787	1.42	1.02	**0.22**	1.12	0.75
	No water	2,602	7,788	1.26	1.08	0.10	1.05	0.37
	No medical care	2,593	7,766	1.51	1.18	0.17	1.03	0.55
	No cooking fuel	2,590	7,775	0.94	0.75	0.12	1.19	0.55
	No cash	2,594	7,777	2.34	1.87	**0.24**	0.79	0.62
							Density test *p*-value:	0.13
Close win vs. draw	Age	1,629	7,730	36.97	37.70	-0.03	1.08	0.16
	Female	1,644	7,766	0.51	0.50	0.01	1.00	0.69
	Muslim	1,620	7,700	0.31	0.38	-0.10	0.91	0.97
	Christian	1,620	7,700	0.62	0.52	0.13	0.95	0.69
	Primary education	1,637	7,742	0.86	0.82	0.07	0.84	0.96
	Employed	1,642	7,742	0.32	0.36	-0.05	0.95	0.27
	No food	1,643	7,761	0.94	0.89	0.03	0.99	0.38
	No water	1,643	7,762	1.08	1.03	0.03	0.91	0.15
	No medical care	1,640	7,749	1.04	1.08	-0.02	0.88	0.42
	No cooking fuel	1,634	7,747	0.75	0.73	0.02	0.97	0.56
	No cash	1,639	7,751	1.65	1.68	-0.02	0.93	0.25
							Density test *p*-value:	0.83

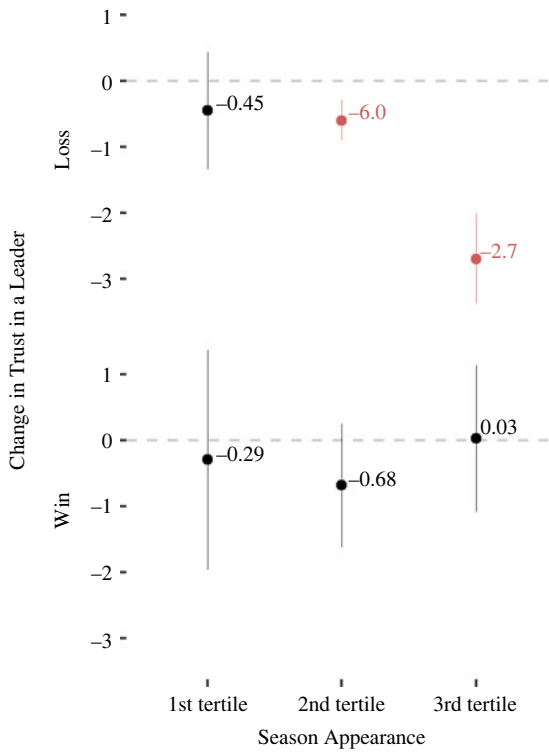
Note: If the standardized mean difference is larger than 0.2 or smaller than –0.2, if the variance ratio is larger than 2 or smaller than 0.5 (Rubin 2001), or if p is less than 0.1, the number is bolded.

Effect Heterogeneity 2: Demographic Covariates

For explorative purposes, we subset the data by demographic indicators (Figure 12).[102] First, we use educational attainment to see 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 because people with higher education tend to learn liberal ideas and hence are more motivated to blame leaders.

Second, we split the data by gender to see whether the results are driven by male respondents. Although the point estimate is slightly larger for men, the difference is small, and close losses affect both men and women (Figure 12, second pane). One possibility is that after losing games, husbands might use violence to vent frustration,[103] and their wives could attribute their sufferings to political leaders. Because Afrobarometer does not contain information about marital status, we leave it to future studies to further explore this channel.

102. The effect heterogeneity by other (less relevant) covariates is reported in Appendix A17.
103. Card and Dahl 2011.



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

FIGURE 11. *Effect heterogeneity 1 (survey analysis)*

Third, we subset the samples by Islamic religiosity to indirectly quantify the role of drinking; that is, the results of football games might affect alcohol consumption,[104] which in turn might induce cognitive biases. Because the Quran prohibits drinking, if there is an effect on Muslim people, the explanation should come from other reasons. The effect of close losses is indeed even more pronounced for Muslim people (Figure 12).

Fourth, we explore whether media usage induces any difference in the causal effects. To this end, we conduct subsample analyses for television and Internet use—primarily as a means to watch live football games. Although only items about media access to news are available in Afrobarometer,[105] they may still be used as proxies. There are

104. Lindo, Siminski, and Swensen 2018; Rees and Schnepel 2009; Wood, McInnes, and Norton 2011.
105. The survey question is "How often do you get news from the following sources: [television/internet]" (Afrobarometer 2019).

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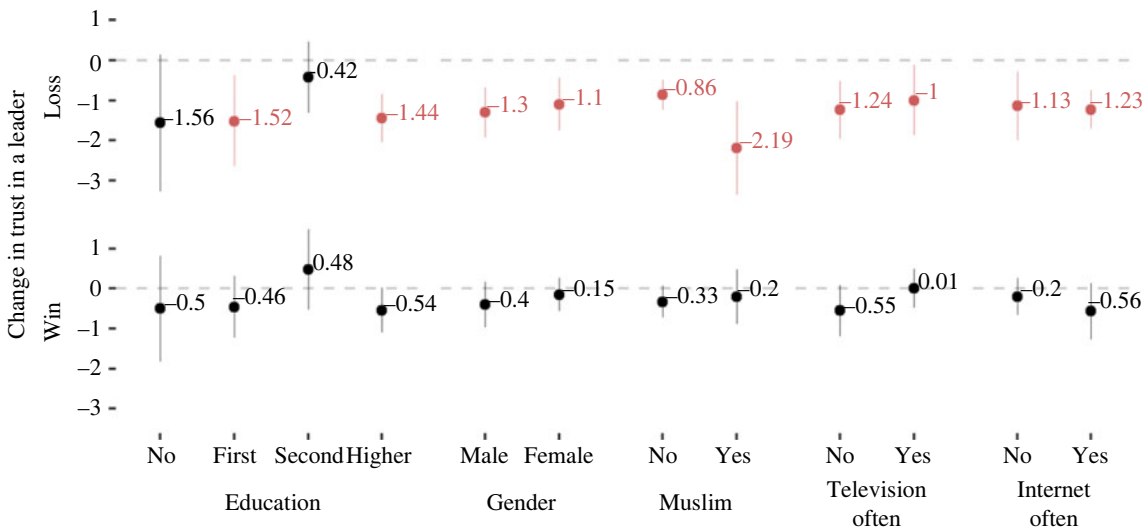

Notes: The figure shows the effect of close losses (top) and wins (bottom) on respondents' trust in a leader in subsamples. The vertical bars are 95% confidence intervals.

FIGURE 12. Effect heterogeneity 2 (survey analysis)

no large differences due to media usage (Figure 12, *right*). However, this could be due to the coarseness of the proxies. Note also that none of these covariates are exogenous, so the effect heterogeneity is not rigorously identified.

Robustness Checks

Finally, we conduct an array of robustness checks, which are summarized in Table 6 and detailed in Appendix A18. As seen in the table, while the results for close wins are unstable and mostly null, the effects of close losses are robust to most of the changes. The only exception is the inclusion of player-specific fixed effects but this is due to overfitting. In the survey analysis, we examine twenty-seven football games, of which only four are played by the same players. This means that the within-player variation in the treatment variable (the results of the football games) is tiny, and thus that the player-specific fixed effects are overly restrictive. Because the fixed effects are not essential in our identification strategy (see the Specification subsection of the event data analysis), we do not think this would pose particular problems.

Discussion

In this paper, we have analyzed the effects of psychological cues on conflicts and built hypotheses about the effects of far-away sports events on local protests. We find that compared to draw games, close losses of African players' teams increase peaceful demonstrations in their original countries, while they do not change the likelihood of riots or armed conflicts. Such losses are also followed by a temporary decline in people's trust in their political leaders. By contrast, close victories have no equivalent or compensating effects on conflict or public opinion.

These findings provide both evidence and counterevidence for the various hypotheses we have mentioned. First, the rational update mechanism is unlikely to be sufficient; it cannot fully explain why politically irrelevant events affect protests or people's opinion in Africa. Second, the mood and asymmetric mood mechanisms are not strongly supported by our analyses. Data are limited, but we see no discernible effect of football results on the observed attitudes of the respondents. Also, these moods cannot explain the differential effects on political trust. Third, the misattribution mechanism cannot explain the weakly positive and mostly null effect of close wins on demonstrations, or the asymmetric effects on political trust. Fourth, the rally mechanism is also not supported by solid evidence; European football may or may not help people in Africa overcome ethno-religious divisions, but our analysis shows that it does not decrease protests, including those related to ethnic issues. This implies that we cannot simply extend the findings on international sports events to European professional football.

That leaves asymmetric misattribution. The near-null effect of close victories is 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 unreasonably blame others—including not only coaches and managers but also local politicians—for the failures of their favored teams, close losses should lower their trust in politicians and thus increase protests. The additional analysis of subjective welfare implies that the asymmetric misattribution can be indirect: losing games lower people's subjective welfare, which in turn is blamed on politicians. Finally, the weak finding that close victories increase demonstrations might be explained by self-confidence. That is, if people perceive a victory as their own success, they may become more confident of their own capabilities, which in turn may motivate them to challenge authorities. This, however, is conjecture; the effect of close wins is not robust and may be a false positive.

TABLE 6. Robustness checks (survey analysis)

	Loss	*Win*	*Appendix*
Analysis based on player citizenship	_*	null	Table A18-1
Subsetting to games with African players' appearance	_*	null	Table A18-2
Omission of cases where there are both close losses and wins on the same day for a respondent	_*	null	Table A18-3
Aggregated analysis at a respondent-game level	_*	null	Table A18-4
Inclusion of non-close games	_*	_*	Table A18-5
Matching on betting odds	_*	_*	Table A18-6
Control for demographic covariates, player performance, violent plays, and betting odds	_*	null	Table A18-7
Control for the running variable	_*	null	Table A18-8
Time fixed effect[1]	_*	null[2]	Figure A18-1
Player-specific fixed effect[1]	null[3]	_*[4]	Figure A18-1
Different time windows	_*	_*[5]	Figure A18-2
Leave-one-country-out tests	_*	null[6]	Figure A18-3

Notes: *$p < .05$; †$p < .10$.

1. Due to the numerical instability with the fixed effects, the standard errors are two-way clustered by player (instead of country) and game.
2. Statistically significant at a 5% level with week fixed effects, and with all of the time fixed effects.
3. Statistically significant at a 5% level with player-specific week fixed effects; statistically significant at a 10% level with player-specific month fixed effects.
4. Null with player-specific week fixed effects.
5. Null for $h = 3$.
6. Significant at a 5% level for 1 of 16 cases, and at a 10% level for 1 of 16 cases.

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 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 governments to address social problems. Given these possibilities, we refrain from making hasty judgments. It is a task of future studies to analyze the welfare consequences of the spillover.

Moreover, to the best of our knowledge, this is the first study that extends the insights in the behavioral literature of voting[106]—that is, the effects 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,[107] they lack causal evidence with real-world data. 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 on the psychological origins of conflict.

Finally, to be clear, we do not intend to imply that people in Africa are particularly irrational. Previous studies have shown that people in the United States and elsewhere also use irrational attribution.[108] We extend the analysis to Africa. What this paper implies is therefore that human beings are similarly irrational, whether they live in Africa or elsewhere, so conflict studies and theories of international relations need to account for it.

Data Availability Statement

Replication files for this article may be found at <https://doi.org/10.7910/DVN/HWVUER>.

Supplementary Material

Supplementary material for this article is available at <https://doi.org/10.1017/S0020818322000261>.

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106. Healy and Malhotra 2013.

107. Davis and McDermott 2021; Hafner-Burton et al. 2017; Kertzer and Tingley 2018.

108. Busby and Druckman 2018; Busby, Druckman, and Fredendall 2016; Graham et al., forthcoming; Healy, Malhotra, and Mo 2010, 2015.

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