Do Shifts in Late-Counted Votes Signal Fraud? Evidence from Bolivia

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Shifts in late-counted votes often spark unfounded claims of electoral fraud. These claims exploit the early-count mirage: the expedient illusion that, absent fraud, an early advantage will persist. We characterize the early-count mirage and evaluate associated fraud claims in four disputed elections, focusing on the case of Bolivia in 2019. When late-counted votes delivered a narrow victory for the incumbent, fraud accusations followed—with dramatic political consequences. But we find that the vote-share trend can be explained without invoking fraud and that the allegedly suspicious shift in late-counted votes was actually an artifact of methodological and coding errors on the part of electoral observers. We document similar patterns in the other three cases. The details are context specific, but the core insights are general: time trends from legitimate vote-counting processes are far more varied—and errors in influential analysis far more frequent—than election skeptics allege.

The order in which votes are counted is anything but random. Voters who have sat through election night in Brazil or Colombia or the United States, for example, know that results from the first few precincts rarely resemble the final outcome.[1] Yet politicians often point to this fact as evidence of fraud. In doing so, they encourage and exploit what we call the *early-count mirage*: the expedient illusion that, absent fraud, an early advantage will persist through the end of the count.

After the 2020 presidential election in the United States, for example, Donald Trump denounced the "brazen and outrageous election theft" that allegedly occurred after 10:00 p.m. on election night, when late-counted votes delivered key states for his opponent (NPR 2021). In Mexico in 2006, likewise, presidential candidate Andrés Manuel López Obrador charged that his margin shrank "in an inexplicable way" on the morning after the poll, leading to a narrow loss (Antenangeli and Cantú 2019, 142). And in Ecuador's 2017 presidential election, Guillermo Lasso's running mate claimed that "with 82.2% of the vote counted, it was impossible to revert the trend [in our favor]"—as effectively happened in the final tally (Páez Benalcázar 2017).

This dynamic is dangerous. In the United States, the early-count mirage contributed to the storming of the Capitol and to enduring doubts about the integrity of the 2020 presidential election (Eggers, Garro, and Grimmer 2021). In Mexico, it fueled months of protests (El País 2006). In Ecuador, it so thoroughly undermined perceptions of electoral integrity that, in the first round of the subsequent election, the third-place candidate denounced a "satanic pact" behind fraud (Nodal 2021).

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Rodríguez acknowledges the generous support for this research from the Center for Inter-American Policy and Research of Tulane University and the Kellogg Institute for International Studies of the University of Notre Dame. Replication files are available in the *JOP* Dataverse (https://dataverse.harvard.edu/dataverse/jop). The empirical analysis has been successfully replicated by the *JOP* replication analyst. An appendix with supplementary material is available at https://doi.org/10.1086/719639.

1. One example from each case illustrates the point. In the 2018 Brazilian presidential election, Fernando Haddad earned just 27% of the first 93% of votes counted but more than 43% of the last 7% (TSE 2018). In the 2018 Colombian presidential election, Iván Duque earned a 13-point lead in the first 93% of the vote but just a 5-point lead in the last 7% (Registraduría de Colombia 2018). In the 2018 US congressional election, Young Kim (candidate for California's 39th district) held a 3-point lead with 65% of the vote counted—but ultimately lost by 3 points (Li, Hyun, and Alvarez 2021).

Published online September 5, 2022.

The Journal of Politics, volume 84, number 4, October 2022. Published by The University of Chicago Press for the Southern Political Science Association. https://doi.org/10.1086/719639

The early-count mirage can deepen the well-documented tendency to disbelieve and delegitimize electoral losses (Anderson et al. 2007; Bush and Prather 2017; Calvo et al. 2020; Cantú and García-Ponce 2015), which in turn can spark protests and violence (Little, Tucker, and LaGatta 2015; Luo and Rozenas 2018; Tucker 2007). Ultimately, the early-count mirage can erode a fundamental tenet of democracy: the acceptance of legitimate electoral results (Przeworski 1991).

We characterize the early-count mirage and reevaluate associated fraud claims in four disputed elections, focusing on one especially consequential case. While the details are context specific, the fraud claims suffer from similar flaws and are therefore susceptible to common correctives. In all four cases, fraud claims tied to the early-count mirage fail in the face of one general insight: time trends from legitimate vote-counting processes are far more varied—and errors in influential analysis far more frequent—than election skeptics allege.

We bring this insight to the Bolivian presidential election of 2019, where late-counted votes secured a razor-thin first-round reelection victory for left-populist incumbent Evo Morales. The runner-up alleged fraud (Mesa 2019). The Organization of American States (OAS), which observed the election, expressed "deep concern and surprise at the drastic and hard-to-explain change in the trend of the preliminary results revealed after the closing of the polls" (OAS 2019c). Subsequent OAS analysis cited "a massive and unexplainable surge in the final 5% of the vote count"—in other words, a discontinuous jump in the incumbent's vote share after 95% of votes had been counted (OAS 2019a, 94).

The political consequences were far-reaching. In large part because of the fraud allegations, and in an environment marked by polarization and incumbent power grabs, the Bolivian military asked Morales to resign; he fled to Mexico.

Our analysis reveals two problems with these consequential claims of fraud. The first is a problem of theory: Bolivian electoral administration is such that we would expect a leftward shift in vote share, and, moreover, we would expect sharp changes in vote share at specific moments in the count—although not, as it happens, at 95% of the count, the point at which the OAS claimed to find evidence of a jump. The second is a problem of method: the "massive and unexplainable surge in the final 5% of the count" (OAS 2019a, 94) was, we show, actually an artifact of methodological and coding errors. We also discuss issues with other quantitative claims of fraud in this election, later published by academic researchers.

These problems are not unique to Bolivia. We consider the early-count mirage in three additional cases: Mexico in 2006, the United States in 2020, and Honduras in 2017. In all three cases, fraud claims based on late-counted votes undermined the legitimacy of a presidential election; in all three cases, the fraud claims were unfounded.[2] The precise causes of shifts in late-counted votes are context specific. But flaws in the fraud claims follow a common pattern: either they fail to acknowledge an innocuous explanation for the shift in late-counted votes, or they fail to analyze the electoral data correctly. Recognizing the diversity of time trends from legitimate vote-counting processes addresses the first failure; prompt review and replication addresses the second.

We advance a growing body of work on fraud-claim forensics. Political scientists have developed sophisticated tools for fraud detection (e.g., Alvarez, Hall, and Hyde 2009; Hicken and Mebane 2017; Myagkov, Ordeshook, and Shakin 2009). A smaller literature assesses politicians' (often unsophisticated) claims of fraud. Recent efforts debunk the myth of widespread double voting in the United States (Goel et al. 2020), Donald Trump's fraud allegations (Cottrell, Herron, and Westwood 2018; Eggers et al. 2021), and claims of fraud in the Mexican presidential election of 2006 (Antenangeli and Cantú 2019). Similarly, work on the "blue shift" in the United States provides an innocuous explanation for the fact that late-counted votes often favor Democrats (Cottrell et al. 2018; Curiel, Stewart, and Williams 2021; Foley 2013; Foley and Stewart 2020; Li et al. 2021). We build on this work by conceptualizing a widespread source of fraud claims—the early-count mirage—and by illustrating how to assess such claims.

We also contribute to an empirical debate over the Bolivian presidential election of 2019 (Escobari and Hoover 2019; Johnston and Rosnick 2020; Minoldo and Quiroga 2020; Newman 2020; Nooruddin 2020a, 2020b, 2020c; OAS 2019a; Rosnick 2020a, 2020b; Williams and Curiel 2020).[3] Our analysis does not establish the absence of fraud in this election; rather, we establish that we do not require fraud in order to explain the quantitative patterns that helped indict Evo Morales—and changed the course of Bolivian history.

THE EARLY-COUNT MIRAGE

We define the early-count mirage as *the expedient illusion that, absent fraud, an early advantage will persist through the end of the count*. In some cases, this early advantage takes the form of a simple lead in votes counted early. Trump, for example, claimed fraud merely by noting that, in several key states, his initial lead over Biden disappeared by the time all votes were

2. For the United States and Mexico, the fraud claims have been discussed elsewhere, and our contribution is to present them in a unified framework. For Honduras (and for Bolivia), to the best of our knowledge, we are the first to highlight certain errors underlying the accusations.

3. Mebane (2019) uses his own toolkit to estimate the extent of fraud (rather than responding to or building on claims from the OAS).

counted (NPR 2021). In other cases, the early advantage is more subtle. In Ecuador in 2017, for instance, the losing candidate complained that an early trend in his favor should have continued (Páez Benalcázar 2017). Whether based on early vote levels, trends, or more sophisticated extrapolation, the early-count mirage appears when the first electoral returns create the deceptive image of one competitor's eventual victory.

One way to understand the early-count mirage is to place it in the framework of hypothesis testing (Eggers et al. 2021). Fraud claims that exploit the early-count mirage implicitly formulate a null hypothesis—there was no fraud—and reject it in favor of the alternative that there was fraud, using a test statistic tied to time trends in vote share. Returning to Trump's statements, we might think of his implicit test statistic as *the difference between a candidate's final margin and his early margin*, and the rejection region as all negative values of the test statistic: a shrinking margin over the course of the count constitutes grounds for rejecting the null hypothesis of no fraud. In Ecuador in 2017, the implicit test statistic was closer to *the difference between a candidate's actual final margin and the margin obtained by extrapolating early trends*, again with a rejection region < 0.

This framework suggests a two-step approach to assessing fraud claims tied to the early-count mirage. The first step is to evaluate whether the (implicit) test statistic provides a valid test of the null hypothesis of no fraud. Researchers have done this work for the "blue shift" in the United States (Cottrell et al. 2018; Curiel et al. 2021; Foley 2013; Foley and Stewart 2020; Li et al. 2021), showing that late-counted votes often lean Democrat because of ballot type: mail-in ballots are disproportionately Democrat, and mail-in ballots are often counted at the end. This finding invalidates using the blue shift itself as a test statistic.

Vagaries of vote-counting processes in other contexts can similarly provide innocuous explanations for shifts in late-counted votes. In the 2021 presidential runoff in Peru, for example, the vote-share trend was nonmonotonic, moving leftward for most of the count as the share of rural votes increased and then ticking rightward in the final 3% of the count (fig. H.4)[4]—a reversal that might appear anomalous were it not for the fact that right-leaning votes from abroad are counted last (because embassies send them by diplomatic pouch; Infobae 2021). Elsewhere, vote-share trends are driven by factors such as physical distance from a precinct to electoral authorities (Antenangeli and Cantú 2019), poll worker characteristics (Challú, Seira, and Simpser 2020; Spencer and Markovits 2010), and communication technology, which can determine the speed of electronic transmission of results (as in Venezuela; pers. comm. with Eugenio Martinez). These causes are context specific, but the associated fraud claims suffer from a common flaw: they fail to recognize that an allegedly suspicious shift in late-counted votes might arise even in the absence of fraud.

The second step is to evaluate whether the testing procedure was executed correctly. Even when fraud claims employ a valid testing procedure—that is, even when they highlight a result that would be sufficiently unlikely under the null hypothesis of no fraud—the analysis may suffer from methodological or coding errors (as Eggers et al. [2021] document for several US fraud claims). In one sense, this is unremarkable: fraud claims often rest on time-sensitive analysis of complex data sets, and mistakes plague even peer-reviewed research undertaken in the absence of deadlines or public scrutiny (e.g., Ziemann, Eren, and El-Osta 2016). Yet the pervasiveness of basic errors in politically influential analysis is surprising, as the cases we study emphasize.

In short, fraud claims tied to the early-count mirage are vulnerable to one of two general correctives: documenting the (sometimes strange) time trends that emerge from legitimate vote-counting processes, and reviewing or replicating the quantitative analysis. In what follows, we bring these correctives to the controversial case of Bolivia in 2019. We find, first, that details of Bolivia's vote-counting process invalidate the testing procedures used to claim evidence of electoral fraud—and, moreover, that the test statistics were calculated incorrectly.

THE EARLY-COUNT MIRAGE IN BOLIVIA

Tensions ran high in advance of Bolivia's 2019 presidential election. The incumbent, Evo Morales, first elected in 2005 as part of Latin America's pink tide (Falleti and Parrado 2018), was controversially seeking a fourth term in office.[5] He led in the polls, but it was not clear whether he would avoid a runoff by earning a 10-point margin over the runner-up. A runoff would likely have been close (ANF 2019).

At 7:50 p.m. on election night (October 20, 2019), Bolivia's electoral authority reported that, with approximately 83% of voting booths counted, Morales (the incumbent) held a 7.87-point lead over the runner-up—not enough to avoid a

4. Figures A.1–A.3, B.1, C.1, D.1–D.3, F.1, G.1, and H.1–H.4 are available online.

5. Bolivia's 2009 constitution imposed a two-term limit—itself more favorable to the incumbent than the previous rule of no reelection (Corrales 2016, 8)—but in 2013 courts allowed Morales to run for a third term, on the grounds that his first term did not count because it began before the new constitution. In 2016, Morales held a referendum on his proposal to eliminate term limits all together, and voters defeated it. He was able to run in 2019 only because Bolivia's highest court ruled that term limits violated the American Convention on Human Rights (Anria and Cyr 2019).

runoff (Bolivia tv 2019). Publication of updated results was then unexpectedly suspended for nearly 24 hours.[6] By that time, the electoral authority announced, Morales had gained a 10.15% lead (Los Tiempos 2019a)—just enough to avoid a runoff.[7] This sequence of events conjured the early-count mirage. Opposition leaders cried fraud (AFP 2019), electoral observers sounded alarm bells (OAS 2019c), and Bolivia "exploded in protest" (Kurmanaev and Castillo 2019).

The OAS audited the election and, three weeks after the poll, released a preliminary report that raised a number of concerns, one of which was a suspicious shift in late-counted votes (OAS 2019b, 9). That afternoon, Morales agreed to new elections (Collyns 2019). But just hours later, under intense public pressure, Bolivia's military and police chiefs asked him to resign (Kurmanaev, Machicao, and Londoño 2019). He flew to asylum in Mexico, claiming that he had been ousted in a coup. To assess these influential claims of fraud, we begin by describing the vote-share time trends that we would expect to observe in Bolivia in the absence of malfeasance.

The time trends that we would expect in the absence of fraud

Three details of Bolivian electoral administration are key to understanding the vote-share time trends that we would expect to observe in the absence of fraud.

First, the poll workers who count ballots are randomly selected from among voters registered *at each voting booth*. They are not self-selected volunteers or government employees or even randomly selected from among voters registered in the precinct as a whole (there are six voting booths per precinct, on average).[8] Rather, these poll workers—called jurors (*jurados*)—are legally required to serve. This means that jurors' socioeconomic characteristics are highly correlated with those of the voters whose ballots they tabulate: where voters are highly educated, for example, jurors are likely highly educated, too. The jurors are responsible for checking voters' names against the registration list, distributing and receiving ballots, and, most importantly, counting the paper ballots (fig. H.1) and writing the totals on a paper tally sheet (*acta*; fig. H.2).

The second key detail concerns the order in which these tally sheets are submitted to Bolivia's preliminary-results system.[9] An on-site operator at each precinct takes a photo of each tally sheet—one per voting booth—and transmits the image to the electoral authority via a cell phone app. The operator also types the vote totals into the cell phone app. Conceptually, we can think of this *transmission time* as each voting booth's *reporting time*.[10]

But the vote totals typed into the cell phone app are not immediately added to the preliminary count. Instead, after transmission, verifiers at a central location view each tally-sheet image and retype the vote counts; if the figures match those entered by the on-site operator at each precinct, the tally sheet is verified and added to the preliminary count.

This is the third key detail of Bolivia's vote-counting process: tally sheets are not verified in the order in which they are received (i.e., transmitted through the cell phone app). Rather, verification operators view tally-sheet images in a random order from the pool of tally-sheet images transmitted thus far (NEOTEC 2019, 5). This means that, during periods when verification largely kept up with transmission, the verification order would largely preserve the transmission order (because the pool of unverified images would remain small). But at any moment when verification lagged behind transmission, the verification order would not reflect the transmission order (see fig. A.2a). This is especially relevant after 8:06:59 p.m. on election night, when verification paused overnight (even as transmission continued). When verification resumed the following day, verifiers received tally-sheet images in a random order from a large pool.

In the absence of malfeasance, we would expect this system to produce three patterns that, in fact, we observe in the data.

1. A proincumbent shift in vote share as transmission progressed (i.e., as voting booths transmitted results through the app). The reason is straightforward. Voters and voting-booth jurors with higher levels of education should complete the voting and counting process marginally faster, and, in Bolivia, education is negatively correlated with support

6. Antenangeli and Cantú (2019) emphasize that such interruptions often cause controversy, even when they are the result of technical difficulty. In Bolivia, critics charged that the government used the "shutdown" to tamper with results; the government claimed that they never intended to tally 100% of the vote in the preliminary results system (Los Tiempos 2019b), while others cited an "enormity of technical fuck-ups" (https://www.reddit.com/r/BOLIVIA/comments/e983ts/why_the_trep_was_suspended/) and "lack of expertise" (*impericia*; Cambara Ferrufino 2019).

7. The final tally, announced on October 25, gave Morales 47.08% to Mesa's 36.51%, a margin of 10.57 points.

8. We use "voting booth" for *mesa* and "precinct" for *recincto*.

9. The preliminary system is called the TREP: Transmisión de Resultados Electorales Preliminares. Bolivia also has a definitive results system, Cómputo, which is much slower and counts tally sheets in an entirely different order. Discussion of late-counted votes centered on time trends from the preliminary results system, so that is our focus here; however, we briefly describe the definitive system below.

10. Even though the time stamps include minutes and seconds, only 8% of tally sheets have unique transmission time stamps. This makes sense given the overall speed of the preliminary system: more than 97% of the 34,555 tally sheets arrived between 4:30 and 9:00 p.m., an average of more than two per second (see fig. A.2). We sort tally sheets in a random order within each time stamp.

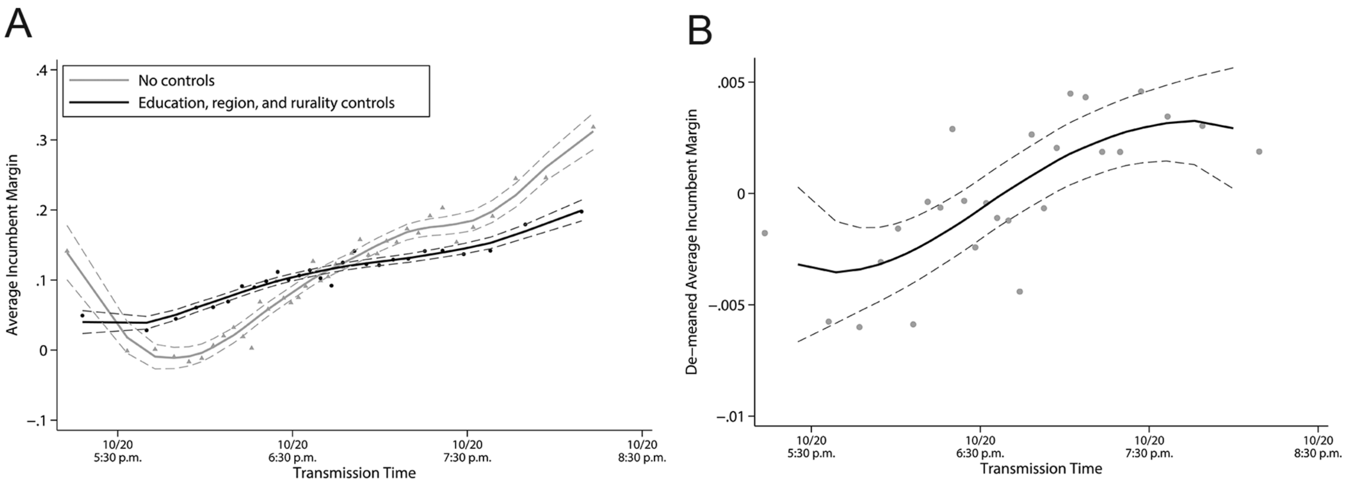

Figure 1. Predictable proincumbent trends in vote share: *A*, education, rurality largely explain pro-incumbent shift; *B*, pro-incumbent shift exists even within precincts. *A*, Gray line marks estimates of the incumbent's average margin over time; black line marks our estimate of this relationship after controlling for local educational attainment and other characteristics (eq. [1]). *B*, Reveals a slight pro-Morales trend even within precincts. On both panels, points mark the average Movimiento al Socialismo (incumbent) margin over Civic Community (runner-up) in optimal (data-driven) bins of the transmission time (Cattaneo et al. 2019), using data from Bolivia's $N = 34{,}555$ tally sheets. For the 377 tally sheets without transmission times, we assign the median transmission time in the respective municipality; other reasonable choices do not change the result (fig. B.1). Solid gray line in *A* and black line in *B* mark local linear fits following Calonico, Cattaneo, and Farrell (2018). Solid black line in *A* marks estimates of f(Time) from equation (1), using the estimator proposed in Robinson (1988). Dashed lines mark 95% confidence intervals. *A* trims the top and bottom 2% of observations; for a version without trimming, see figure B.1a.

for incumbent Evo Morales (Madrid 2012, 36). (Recall that jurors are randomly selected from among voters registered at each booth.) Critically, small absolute differences in reporting time correspond to very large relative differences in reporting time because the system is so fast: nearly all voting booths (97.35%) transmit results between 4:30 and 9:00 p.m. (see fig. A.2). In the densest part of this reporting window, a mere 10-minute delay in transmission time would move a voting booth from the 36th to the 46th percentile of transmission time. This is all to say that the education-reporting time gradient need not be very steep, in absolute terms, in order to produce a marked proincumbent shift in vote share as reporting progressed.

And indeed, we observe a strong proincumbent shift in vote share as reporting progressed. The gray line in figure 1*A* plots Morales's average margin (not cumulative margin) over the runner-up as a function of voting booth reporting time (i.e., transmission time). Morales's average margin rose through most of the active reporting window, from near 0 to approximately 40 percentage points by the end of the evening.[11] Consistent with the notion that this trend arises because less educated voting-booth jurors take longer to report—and because less educated voters support Morales at higher rates—we observe that the trend flattens considerably when we simply control for a small set of socioeconomic characteristics, crudely measured at the municipality level. Specifically, the gray line in figure 1*A* plots our estimate of $f(\text{Time}_{bpm})$ from

$$\text{Margin}_{bpm} = f(\text{Time}_{bpm}) + \beta \boldsymbol{X}_{bpm} + \varepsilon_{bpm}, \qquad (1)$$

where $\boldsymbol{X}$ are characteristics of booth b in precinct p in municipality m: measures of education at the municipality level, a vector of proxies for rurality, the number of registered voters, and an indicator for all voting booths located in the Bolivian lowlands (which lean against the incumbent; Anria 2018, 64–67).[12] We estimate f(Time) using the semiparametric estimator proposed by Robinson (1988). Additional controls further flatten the vote margin time gradient (app. B). In short, Bolivia's blue shift is predictable rather than anomalous.

2. Discontinuous changes in vote share at certain verification times (although not at the time highlighted by the OAS). Because verification operators view tally-sheet images in a random order from the pool of tally sheets transmitted

11. The gray line in fig. 1 is fit only to observations in the estimation sample for which we have socioeconomic covariates, and thus it excludes precincts outside Bolivia (such as embassies). The time trend including all observations is quite similar; see fig. B.1e.

12. Specifically, the measures of education at the municipality level are (i) proportion of adults who are literate, (ii) proportion of adults who completed primary school, and (iii) proportion of adults who completed secondary school. The measures of rurality are (*a*) the (log) number of registered voters per square kilometer around precinct p, (*b*) the (log) proportion of the population employed in agriculture in municipality m, (*c*) the (log) population of municipality m, and (*d*) an indicator taking the value 1 if municipality m is the capital of its department.

thus far—and because one candidate's vote share increases over time—we would expect a discontinuous change in vote share whenever verification lags behind transmission.[13] And indeed, the overnight pause in the process of verifying tally sheets naturally created a discontinuous jump in vote share (app. A) because tally sheets were verified in a random order after the interruption (although again, this was not the cutoff studied in OAS [2019a]).

3. A slight proincumbent shift even across voting booths within the same precinct. The distribution of voter education is unlikely to be exactly identical across voting booths within a precinct. This would be true even if voters were assigned to booths randomly within a precinct; the differences across booths are likely greater because Bolivian voters are assigned to booths by alphabetical order of surname—not randomly—and because surnames are tied to ethnicity in Bolivia (we elaborate this point below and in app. D). Ethnicity, in turn, is correlated both with education and with support for the incumbent (Klein 2011, 282; Madrid 2012, 36, 69). As noted above, we would expect less educated voters (and jurors) to report later and to support the incumbent at higher rates; education varies not only across precincts but across booths within a precinct. It would therefore be unsurprising to find a positive within-precinct correlation between the incumbent's margin and reporting time (i.e., transmission time).

And indeed, we observe a slight proincumbent shift in vote share even within precincts. Figure 1*B* plots demeaned incumbent margin—that is, the margin in voting booth *b* minus the average margin in precinct *p*, or ($\text{Margin}_{bp} - \overline{\text{Margin}}_p$)—against transmission time. Again, this pattern is consistent with the notion that booths with less educated voters favored the incumbent and also tend to report later, because the jurors are less educated, too (it is also consistent with other explanations; see below and app. D for additional discussion). In short, the within-precinct trend is not necessarily anomalous.

The time trends cited as indicative of fraud

With these expectations in mind, we follow the two steps outlined above to assess major claims of fraud in Bolivia's 2019 presidential election. For each claim, we first evaluate whether alleged anomalies in late-counted votes were actually anomalous; second, we revisit the execution of the empirical analysis. To paraphrase Eggers et al. (2021, 2), we find that purportedly anomalous facts about Bolivia's late-counted votes were either not anomalous or not facts.

An "inexplicable surge" in the last 5% of the preliminary count. Electoral observers from the OAS pointed to late-counted votes as indicative of fraud. On the evening after the election, when the Bolivian authorities first announced that the incumbent had cleared the 10-point margin required to avoid a runoff, the OAS mission issued a statement expressing "deep concern and surprise at the drastic and hard-to-explain change in the trend of the preliminary results revealed after the closing of the polls" (OAS 2019c). The preliminary report of the OAS audit, in turn, sounded alarm bells about a "highly unlikely trend in the last 5% of the vote count" (OAS 2019b, 9), a concern echoed at length in the final audit report (OAS 2019a).

Specifically, the final report of the OAS audit team claimed that Morales's vote share jumped discontinuously after 95% of the vote had been verified (OAS 2019a, 88).[14] The report did not articulate a theory of fraud that would produce the alleged jump at 95% of the vote verified—indeed, the report labels 95% "an arbitrary point" (10).[15] But the implicit notion was one of centralized tampering: realizing that Morales was not on track to win by more than 10 points, presumably, his agents crudely added votes in all booths that had yet to be verified in the preliminary system. Hence, his victory was "only made possible by a massive and unexplainable surge in the final 5% of the vote count. Without that surge . . . he would not have crossed the 10% margin that is the threshold for outright victory" (94).

In other words, the OAS implicitly tested the null hypothesis of no fraud—or, at least, of no reason for concern

13. To see this, consider a simplified example. Imagine that there are 100 tally sheets and that the incumbent's vote share increases in transmission order, at a constant rate: he earns, say, 10% of votes on the tally sheet transmitted first, 10.5% of votes in the tally sheet transmitted second, 11% on the tally sheet transmitted third, and so on, until earning 60% of votes on the tally sheet transmitted last. Now imagine that tally sheets 1–50 are verified in the order in which they were received but that tally sheets 51–100 are verified in a random order (because verification paused between tally sheet 50 and tally sheet 51). If we plot the incumbent's vote share against the verification order, we should expect a jump between the fiftieth verified sheet (vote share = 25%) and the fifty-first verified sheet, which is drawn at random from the remaining tally sheets (expected vote share = 42.5%).

14. To be precise, the report claimed to find a discontinuous jump after 95% *of the eventually verified vote* was verified: 4.4% of votes were never verified in the preliminary results system. These are excluded from the figure in the OAS report (Nooruddin 2020a) and from our replication exercise.

15. The 95% cutoff studied in the OAS final report does not correspond either to 7:40:57 p.m., when the government stopped publishing updated results on the electoral authority website, or to 8:06:59 p.m., when the electoral authority stopped verifying tally sheets until the following day. See app. A for additional discussion.

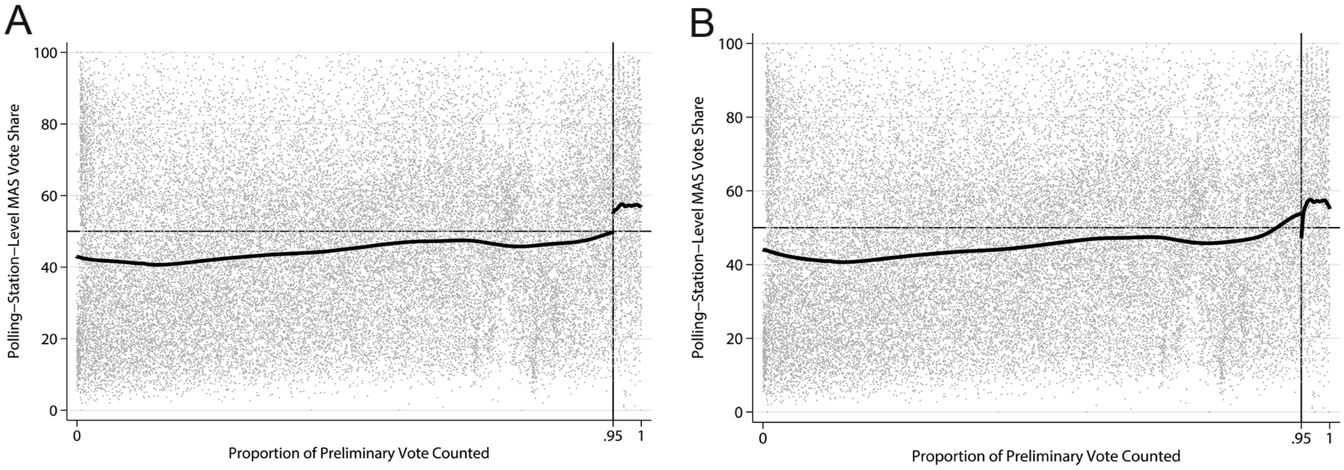

Figure 2. Method error and the alleged jump at 95%: *A*, local constant regression (replication of OAS [2019a], 88); *B*, local linear regression (no other changes). *B*, Shows that the apparent jump disappears when we simply use local linear rather than local constant regression. On both panels, dots mark the underlying raw data, with the preliminary results system verification time on the *X*-axis and the final (definitive results system) incumbent vote shares on the *Y*-axis, following Nooruddin (2020b). Lines mark lowess estimates with handpicked bandwidths, as implemented by Nooruddin (2020b). Again following Nooruddin (2020b), both panels use data from the $N = 33{,}038$ tally sheets verified in the preliminary results system, excluding the 4.4% of tally sheets that were never verified (see apps. A and C for additional discussion).

about fraud—against the alternative of cause for concern, using the test statistic "change in vote share at 8:03:59 p.m., when 95% of the vote was verified in the preliminary system." A large proincumbent change "strained credulity" (OAS 2019a, 94) sufficiently to reject the null. We argue both that the proposed hypothesis test is invalid and, moreover, that the test statistic was miscalculated.

The test is problematic because of the nature of the verification time series. As noted in the previous section, tally sheets were verified in a random order conditional on transmission, so any pause or delay in the verification process would naturally generate a jump in vote share (as in fact occurred at 8:06:59 p.m.; see app. A). To justify the hypothesis test proposed in the audit report, therefore, the OAS would not only have to articulate a theory of fraud consistent with a jump at 95% but also need to refute the alternative explanation that the jump arose as the benign and predictable consequence of the design of Bolivia's vote-counting system. Regardless, as we establish in the remainder of this section, the test statistic was miscalculated: there is no jump in vote share at the cutoff studied in OAS (2019a, 88).

In support of the claim of "a massive and unexplainable surge in the final 5% of the count," the OAS presented figure 2*A* (2019a, 88). But the apparent discontinuous jump in this figure—at 95% of votes verified in the preliminary system—is the artifact of using an estimator inappropriate for regression discontinuity analysis. The OAS created the smoothed line in figure 2*A* by estimating one local constant regression at each data point and connecting the predicted values.[16] One problem with this approach is that local constant regression often misrepresents the data at boundary points (i.e., at the edges). This boundary bias problem is well documented: "a polynomial of order zero—a constant fit—has undesirable theoretical properties at boundary points, which is precisely where regression discontinuity estimation must occur" (Cattaneo, Idrobo, and Titiunik 2020, 38).[17] In figure 2*B*, we instead use a local polynomial of degree 1 (i.e., local linear regression); this change alone is sufficient to eliminate the appearance of a jump.

The use of local constant rather than local linear regression is not the only problem with figure 2*A*. For one thing, this figure excludes the 4.4% of observations that never made it in to the preliminary count—contrary to the OAS's claim that "all analysis conducted below includes these additional [observations]" (OAS 2019a, 86). When we append these observations to the end of the preliminary results data, as the OAS claimed to do, there is no discontinuity at 95% (see app. C). For another, the local regressions underlying figure 2*A* use handpicked, arbitrary bandwidths. Moreover, the OAS presented

16. In particular, the OAS used Stata's lowess function, with the mean option, which implements local constant regression rather than local linear regression (running-mean smoothing rather than running-line least-squares smoothing, which is the default).

17. See also Yu and Jones (1997), who conclude, "Detrimental boundary influence indeed exists when using local constant fitting in some cases, and it is this aspect which clinches the argument in favour of local linear smoothing" (165), as well as Fan and Gijbels (1996), secs. 2.2.3, 3.2.5, and 3.4.2, and Imbens and Kalyanaraman (2011), 935.

Table 1. No Evidence of Discontinuous Changes at Two Cutoffs

				Robust		Observations	
Cutoff	Date and Time	RD Estimate	BW	p	95% CI	Left	Right
.950	10/20/2019 20:03:59	.027	.040	.634	[−.044, .073]	1,291	1,345
.889	10/20/2019 19:40:57	−.018	.039	.721	[−.065, .045]	1,248	1,245

Note. The running variable is percentiles of verification time in the $N = 33{,}038$ tally sheets verified in Bolivia's preliminary results system; the outcome is incumbent (Movimiento al Socialismo) vote share, as recorded in the definitive results system (following Nooruddin 2020b). We use the nonparametric regression discontinuity (RD) estimator proposed by Calonico, Cattaneo, and Titiunik (2014). We cannot reject the null hypothesis of no treatment effect at two cutoffs: 95% of the vote verified in the preliminary system, the cutoff studied in OAS (2019a), and 7:40:57 p.m. on election night, when the government stopped posting updated results (the "shutdown"). BW = bandwidth; CI = confidence interval.

no formal test of the null hypothesis of continuity at 95% of the preliminary count. Our simple modification in figure 2*B* does not solve these problems; it merely illustrates the severe boundary bias problem created by the use of local constant regression in figure 2*A*.

To estimate the size of the jump at 95% of the preliminary count, we use the data-driven regression discontinuity estimator proposed by Calonico, Cattaneo, and Titiunik (2014). This approach estimates the treatment effect by running two local linear regressions precisely at the cutoff (one to the left, one to the right). We test for discontinuities at two points: (1) 95% of the preliminary count, that is, the point studied by the OAS in figure 2*A*, and (2) 7:40:57 p.m. on election night, when the electoral authority stopped publishing updated results.[18] We cannot reject the null of continuity at either of these two points (table 1). In appendix F, we show that this finding is robust to (*a*) the (random) sort order within identical time stamps, (*b*) polynomial degree, and (*c*) bandwidth. In sum, we find no evidence of the alleged discontinuous jump in Morales's vote share at 95% of the vote counted—the "surge" to which the OAS attributed his first-round victory (OAS 2019a, 94).

A "striking upward trend" in the last 5% of the definitive count. As additional evidence of the anomalous character of late-counted votes, the OAS presented graphs using a separate set of time stamps (in other words, using a different *X*-axis): time stamps from Bolivia's definitive results system, or Cómputo. The definitive system is slower and, at least in theory, more accurate than the preliminary system; it relies on physical delivery of tally sheets to each of nine electoral authority offices, rather than tally-sheet images transmitted through the cell phone app. The last 5% of observations in the preliminary-system verification time series are thus different from the last 5% of observations in the definitive-system time series. The OAS nevertheless presented graphs using the definitive-system time stamps as a kind of robustness check, stating that "we should analyze if the same patterns emerge if we use only the [definitive system] time stamps" (2019a, 91).

The OAS concluded that similar patterns do emerge in the analysis using the definitive-system time stamps (2019a, 91). In support of this statement, the OAS presented figure 3*A*, in which there is a "striking upward trend" (92) in Morales's vote share after 95% of votes are counted in the definitive results system. But this pattern is the artifact of a coding error. The OAS sorted the definitive-system time stamps alphabetically, such that 7:01 p.m. comes right after 7:01 a.m., rather than chronologically (as Rosnick [2020b] noted, after OAS replication materials were posted in response to an earlier draft of this article). Correcting this error eliminates the appearance of an anomalous late-breaking surge in the incumbent's vote share (fig. 3*B*).

Within-precinct variation as evidence of "a statistically significant case of electoral fraud." Researchers outside the OAS also pointed to late-counted votes as indicative of fraud in the Bolivian presidential election. Escobari and Hoover (2019) highlight within-precinct variation. Specifically, they note that the Movimiento al Socialismo performed better in voting booths reporting after the government stopped publishing results (postshutdown) than in voting booths *from the same precinct* that reported earlier (preshutdown), interpreting the difference as evidence of a "statistically significant case of electoral fraud" (1).

18. The last website update occurred at 7:40:57 p.m., in advance of the 7:50 p.m. press conference mentioned above.

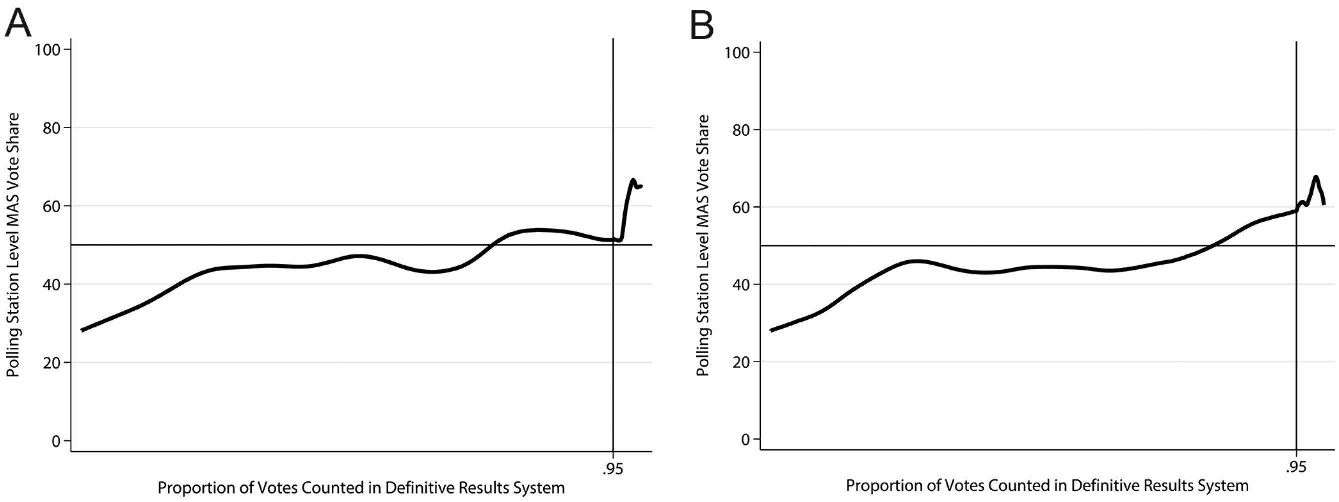

Figure 3. Coding error and the alleged "striking trend" (Rosnick 2020b). *A*, Reproduces OAS (2019a, 92), for which time stamps were mistakenly sorted alphabetically (7:01 p.m. follows 7:01 a.m.). *B*, Shows that the apparent "striking upward trend" disappears when time stamps are sorted chronologically, as noted in the press (Rosnick 2020b). Lines mark lowess estimates with handpicked bandwidths, as implemented by Nooruddin (2020b). Both lines are fit to data from the $N = 34{,}555$ tally sheets in Bolivia's definitive results system.

In our view, these inferences are unjustified. The analysis in Escobari and Hoover (2019) compares two periods (pre and post) without accounting for a secular trend. As noted above, Bolivian electoral administration would lead us to expect a within-precinct correlation between vote share and reporting time, and, indeed, we observe this correlation in the data (see fig. 1*B*). We show in appendix D that accounting for this secular trend eliminates the appearance of an anomalous within-precinct pre-post difference in vote shares. In appendix D we also elaborate on the explanation proposed above (that voter characteristics vary sufficiently across voting booths within a precinct to produce a slight within-precinct correlation between reporting time and incumbent vote share).

In short, the pre-post difference estimated by Escobari and Hoover (2019) is not a test statistic that can be used to evaluate the null hypothesis of no fraud. Neither the secular trend nor our proposed explanation establishes the absence of tampering with late-reporting booths; rather, they imply that we do not need electoral manipulation in order to explain the within-precinct differences that these authors cited as evidence of foul play.

Consequences of Bolivia's early-count mirage

Statements from the OAS played an important role in the evolution of Bolivia's political crisis (ICG 2020, 3–4). Key political actors cited the OAS in calls for new elections and for Morales's resignation. The party of runner-up Carlos Mesa, for example, summarized the OAS reports as "evidencing the violation of basic principles essential for the transparency of this electoral process and a sudden and inexplicable change of the irreversible trend towards a second round" (Comunidad Ciudadana 2019). The opposition Committee for Santa Cruz even drafted a resignation letter for Morales and asked him to sign it; first on the committee's list of reasons was the fact that "as the OAS delegate said" there was "an inexplicable change in the vote trend" (CSC 2019). Moreover, the OAS secretary general called the results "irrefutable," saying that "yes, there was a coup d'etat in Bolivia . . . when electoral fraud was committed" and that those who committed fraud "had blood on their hands" (EFE 2019). This is all to say that fraud allegations contributed to Morales's exit and to the consolidation of an interim government under a little-known conservative politician whose government was accused of grave human rights violations (Anderson 2020; Europa Press 2020; HRW 2020).

THE EARLY-COUNT MIRAGE BEYOND BOLIVIA: EVIDENCE FROM THREE CASES

The early-count mirage is not unique to Bolivia. In Mexico, the United States, and Honduras, presidential candidates, electoral observers, and the international press made unfounded claims of fraud based on shifts in late-counted votes. The details vary across cases, but the fraud claims suffer from common flaws. Either they fail to recognize how legitimate vote-counting processes could produce an allegedly suspicious time trend, or they fail to correctly execute the empirical analysis.

Mexico, 2006

The Mexican presidential election of 2006 was decided by less than six-tenths of 1 percentage point. The runner-up, Andrés Manuel López Obrador (AMLO) refused to acknowledge the result, accusing the government of fraud and even setting up a makeshift government of his own in Mexico City's central plaza. AMLO's fraud accusations stemmed from two features of the trend in vote share (Antenangeli and Cantú 2019). First, he noted that his rival pulled ahead very early on—after just 26 voting booths had reported—and never again fell behind. AMLO and his allies viewed this as suspicious: in such a close election, they reasoned, wouldn't you expect more reversals over the course of the count? Second, AMLO questioned a nonmonotonicity in the early part of the count, asking why his rival took an initial lead and then fell behind, only to regain the advantage "in an inexplicable way" (142).

Antenangeli and Cantú (2019) study both accusations, finding that neither the number of reversals nor the initial nonmonotonicity were unusual under the null hypothesis of no fraud. First, using simulations, they show that, contrary to AMLO's intuition, we would not expect many reversals over the course of the count—even in such a close race. Second, they model the reporting time of each voting booth as a function of covariates (such as distance to district office, to which tally sheets are physically delivered) and examine observations with apparently anomalous reporting times. We might think of these voting booths as the population of potentially reordered observations—that is, those that might provide evidence in favor of AMLO's accusation. In fact, Antenangeli and Cantú find that removing or reordering these voting booths is of little consequence for the vote-share trends. Which is all to say that, if the government attempted to sort voting-booth-level tallies in order to give one candidate an early lead, it failed. Taken together, these findings underscore the point that legitimate vote-counting processes produce a wide variety of time trends.

The United States, 2020

The experience of the United States in the 2020 presidential election provides yet more evidence of this variation. In North Carolina, for example, Joe Biden held a lead 90 minutes after polls closed on election day—only to fall behind by dawn the next morning (Bronner, Wiederkehr, and Rakich 2020). This pattern was the predictable outcome of the state's decision to count mail-in ballots in advance (mail-in ballots typically lean Democrat; Foley 2013; Foley and Stewart 2020). In the critical state of Pennsylvania, in contrast, Donald Trump pulled ahead early on election night and held his lead for more than two days; it took 61 hours after the polls closed for Biden's vote count to surpass Trump's (Bronner et al. 2020). Again, this late-breaking Biden win was the predictable outcome of the fact that Pennsylvania does not begin processing mail-in ballots until election day. That fact did not stop Trump from referring to late-counted votes as "explosions of bullshit" with which the Democrats stole the election (NPR 2021). Nor did it stop a lawsuit against Pennsylvania from claiming that, in the absence of fraud, there was only a "one in a quadrillion" chance of the state's vote-share trend occurring (Eggers et al. 2021, 12). As Eggers et al. (2021) explain, this statement was based on the erroneous premise that Biden would capture the same number (not share) of early and late-counted votes—despite the fact that less than 10% of votes are defined as "late." In other words, in order for the data to pass the lawsuit's flawed test for electoral integrity, Biden's share of the vote would have had to increase by a factor of at least 9 between early and late-counted votes (e.g., from 11% to 99%). Here again, election skeptics rejected the null hypothesis of no fraud using test statistics entirely consistent with that null.

Honduras, 2017

The Honduran presidential election of 2017 provides additional examples of fraud claims likely based on coding errors and of fraud claims based on conceptual errors—both stemming from the early-count mirage. As in Bolivia, there were ex ante reasons to doubt the incumbent's commitment to electoral integrity; among other issues, incumbent-party poll workers were apparently encouraged to facilitate double voting for the incumbent and to nullify unfavorable ballots (*Economist* 2017a). And, as in Bolivia, early votes favored the challenger. The day after the election, with 57% of the vote counted, he held a 5-point lead, and then, after a series of unexplained delays in the count and deadly protests, electoral authorities announced that the incumbent had won by 1.3 percentage points (*Economist* 2017b).

The OAS observed the poll, documented several irregularities, and called for new elections (OAS 2017). This call was based in part on quantitative analysis of the time trend in vote share. The trend was nonmonotonic; the opposition candidate's cumulative margin increased through the first part of the count and then declined, ultimately falling below zero. In and of itself, the report viewed this trend as potentially innocuous—"possibly the result of [opposition]-favoring areas reporting results earlier and being counted sooner"—rather than suspicious. But the report also claimed to show that "in every department [i.e., state], the same pattern [of collapse in the opposition's margin] is evident," a fact that "raised real doubts in [their] mind" (Nooruddin 2017, 4).

The apparent geographic homogeneity of the trend in vote margin made international news. The *New York Times*, for example, noted that a sharp swing away from the opposition candidate "occurred across all regions" (Malkin 2017). But the graphical evidence for this point, reproduced in appendix G, appears to be the result of an error. Graphs presented as time trends specific to each of Honduras's 18 departments actually seem to report the national time trend in vote share, projected onto the department-specific time stamps. The shape and scale of the vote-margin trend are not similar across departments in these graphs; they are identical—a pattern inconsistent with the reality of department-level vote margins. Rather, these graphs appear to reprint the national vote-margin trend for each department, varying only the *X*-axis (the time stamps).

Before the OAS report, the *Economist* magazine analyzed municipality-level electoral returns and also found "reasons to worry" (*Economist* 2017b). In particular, they noted that there was a late-breaking swing toward the incumbent in the majority of municipalities. Of course, as the *Economist* acknowledged, this could be the result of within-municipality variation in covariates such as urbanization: even within a small municipality (i.e., district), towns might report before villages. To refute this, the *Economist* might have shown that the most homogenous municipalities—those that are almost all rural or almost all urban—have swings just as large as those of more mixed municipalities. Instead, the *Economist* showed that the swing toward the incumbent is uncorrelated with the overall rurality of the municipality, a fact that has no bearing on the article's stated hypothesis.

None of this is to say that there was no fraud in the 2017 Honduran presidential election or even to say that there is no quantitative evidence of fraud. Rather, we highlight two independent quantitative analyses that, in our view, erroneously presented late-counted votes as indicative of possible malfeasance.

CONCLUSION

Governments rarely announce election results all at once. Instead, they release partial tallies as they trickle in, telling the public how things stand with 30% of precincts reporting, 70%, 90%, and so on. These updates respond to popular demand for information. But they can also entail an important and seldom-studied cost: what we call the early-count mirage, or the expedient illusion that, absent fraud, an early advantage will persist.

The early-count mirage generates a trade-off between transparency and certainty. Incremental reporting provides transparency, but waiting to announce the final result provides certainty. Dispelling the early-count mirage lowers the costs of transparency and thereby softens the government's trade-off between these two objectives.

We establish that fraud claims tied to the early-count mirage suffer from common flaws. Either they neglect the fact that legitimate vote-counting processes can produce apparently anomalous shifts in vote share over the course of the count, or their analysis suffers from methodological or coding errors. Refuting these claims therefore requires describing the vote-share trends that we would expect to observe in the absence of fraud, as well as careful replication of quantitative results.

We study the Bolivian presidential election of 2019, in which fraud claims tied to the early-count mirage played an important role in reversing the outcome of the election (ICG 2020). Our analysis reveals that these influential fraud claims rested on conceptual, methodological, and coding errors. And even where fraud claims tied to the early-count mirage do not overturn an election—as in Honduras, Ecuador, Peru, Kenya, local and national elections in Mexico, and local and national elections in the United States, for example—these claims can spark conflict and erode perceptions of democracy.

The Bolivian case underscores key findings from the literature on international election observation (e.g., Beaulieu and Hyde 2009; Bush and Prather 2018; Donno 2010, 2013; Hyde 2007, 2011; Hyde and Marinov 2014; Kavakli and Kuhn 2020; Simpser and Donno 2012). As in Bush and Prather (2017), third-party monitors powerfully shaped local perceptions of electoral credibility—especially those of political losers. Moreover, the controversial role of the OAS in Bolivia likely affected attitudes toward the OAS across the Americas, as well as decisions about whether and when to engage with the OAS on unrelated issues (consistent with Bush and Prather [2020] and Corstange and Marinov [2012]). Yet, the literature finds that intergovernmental organizations in general—and the OAS in particular—are less likely to question electoral integrity than nongovernmental organizations (Kelley 2009, 779; 2012). In Bolivia, we find, the early-count mirage was so powerful as to overcome that hesitancy. Beyond electoral observation, our findings speak to work that connects the agendas of international organizations with those of domestic political actors, especially in the shadow of elections (e.g., Schneider and Slantchev 2018).

Our analysis also has policy implications for electoral observers employing statistical analysis. First, an ex ante decision rule about when to use quantitative data would improve transparency and consistency. Second, using established tools for election forensics (e.g., Alvarez et al. 2009; Hicken and Mebane 2017; Myagkov et al. 2009), with established indicators of fraud, would reduce the prevalence of invalid tests. Third, instituting an internal or external review process for quantitative analysis would lower the probability of errors. Of course, review processes take time, and electoral observers often work on tight deadlines. One way to navigate this tension between time and quality would be to post replication data and code together with the electoral observers' report. Contested elections generate sufficient interest from quantitative researchers that conceptual or methodological errors would likely be caught quickly and communicated to politicians, journalists, the public, and the electoral observers themselves. Timely correction could avert some of the political consequences of unfounded claims of fraud.

Researchers can protect the legitimacy of the electoral process not only by detecting fraud but also by debunking false claims of fraud. We advance this agenda by conceptualizing the early-count mirage and articulating insights that can dispel it.

ACKNOWLEDGMENTS

We are grateful to Mike Alvarez, Santiago Anria, Marc Meredith, and Rocío Titiunik for detailed guidance. For research assistance, we thank Mateo Arbeláez, Estefanía Bolívar Méndez, Giancarlo Bravo, Adolfo De Lima, and Juan Vera. For comments, we thank María Eugenia Boza, Ernesto Calvo, Francisco Cantú, Matías D. Cattaneo, Tulia Falleti, Camilo García-Jimeno, Francisco Garfias, Etan Green, Justin Grimmer, Guy Grossman, David Hausman, Dan Hopkins, Richard Kronick, Yphtach Lelkes, Matt Levendusky, Michele Margolis, Edward Miguel, Jonathan Mummolo, David Rosnick, Josh Simon, Uri Simonsohn, Tara Slough, and workshop participants at the University of Pennsylvania, Berkeley, Tulane, Stanford, Data Colada, Polmeth, and University of California, San Diego. We thank Anatoly Kurmaneav and the *New York Times* for sharing data.

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