Reviewer: 1

The article examines how political instability impact investment. The author utilizes daily stock market data as a proxy for investment. This topic is explored intensively in the 1990s after the seminal article published by Robert Barro in QJE 1991. In the 1990s due to the limitation of data availability, most studies employed cross-section data. In the early 2000s, the availability of data became more substantial, scholars began to use panel data. The major contribution of this article is the utilization of daily data from the stock markets. The results are mostly similar to the findings in previous literature found in the 1990s. Particularly, Yi Feng (1997), "Democracy, Political Stability and Economic Growth", found that there are three types of regime changes: major, minor, and irregular. Major and minor regime changes raise stability because they signal positive policy change and political reform. Irregular regime changes such as coups and assassinations indicate instability. Thus, major and minor regime changes have a positive impact on investment and economic development, while irregular regime changes have a negative impact.

The country case studies in Venezuela and Egypt provided by the author confirms the data analysis. There are other economic indicators affected by political instability as mentioned in the conclusion. These indicators can only be examined using annual data to see the significant impact on the economy in the long run. However, using daily stock data in this study can be useful in the short run in the financial industry.

Overall, the article is very interesting and I recommend acceptance for publication in BAP.

We thank the reviewer for their positive assessment of the manuscript and its contribution to both the literature and practitioners, and are heartened to hear that they think the article should be published in B&P.

This is an interesting and substantively rich paper that evaluates how a wide variety of "political" shocks—broadly labeled by the authors as political instability—affects both abnormal returns in the stock market (via an event study approach) and the volatility of stock prices using "daily" data from stock indices from a global sample of countries, region-level stock indices and an emerging-market stock index. The political shocks that the authors focus on include coups, resignations, assassinations, and protests, as well as for authoritarian vs. democratic and pro vs. anti-business shifts. There are several interesting empirical results and insights reported throughout the paper. For instance, they find that stock market returns following resignations are large and positive to the tune of +4%. Stock market returns following coups and assassinations tend to be negative and smaller in magnitude. Interestingly, however, they find that a pro-business coup results in large positive returns and that authoritarian or anti-business regime changes are likely to lead to capital flight compared to democratic or pro-business changes.

There is much to like in this paper including the fact that it addresses an interesting question, develops and presents new techniques for assessing volatility in high frequency time-series data, and carefully evaluates the main claims. This is one of the few papers that I have seen that examines the impact of political shocks across such a wide sample—both in cross-sectional and temporal terms—of stock market indices. Furthermore, the authors introduce an interesting identification strategy that combines both event study and synthetic control methods. This empirical strategy allows researchers to carefully evaluate the "treatment" effect of political shocks on particularly returns from daily stock price movements. They also use a variety of well-established techniques to evaluate the effect of political shocks (i.e., instability) on stock price volatility. I enjoyed reading the paper and I believe it is a good fit for the Journal. There are some issues though that the authors should (ideally) address when revising the paper. These are as follows.

We thank the reviewer for their positive assessment of the manuscript and its (a) empirical contribution to the literature on market reactions to political shocks and (b) methodological contributions to the estimation of treatment effects using interrupted time series.

Issues that need to be Addressed or Clarified

1.While developing theoretical claims and arguments are not the main focus of their paper (which is understandable!), it will nevertheless help readers understand why, for example, the mean stock market returns is influenced by particularly the two main kinds of political events considered in greater length by the authors: the onset of coups (including pro-business coups) and anti-business regime change. Although one is agnostic about the "direction" of the proposed effect—i.e., whether it's positive or negative—it is useful to understand a priori from a theoretical perspective why *daily* stock market returns will be influenced by such events. Citing the background literature to develop the theoretical rationale, as done by the authors, is useful but not sufficient.

We thank the reviewer for their suggestion to clarify why daily stock returns would be theoretically expected to be influenced by sudden political events. We have now created two separate sections entitled "Estimation strategy" and "Impact of Political Instability on

Stock Returns." In the "Estimation strategy" section, we add additional discussion of the theoretical mechanisms underlying event studies that justify the method's use and the use of daily financial data. Specifically, we now note on p. 12:

"We estimate the effect of irregular regime changes on financial flows using an interrupted time series--also known as event study---approach, and examine the impact of political events on stock returns using daily financial data. Additional explanation of the theoretical logic of why mean *daily* stock market returns would be influenced by politically unstable events is warranted. Event studies rely on the theoretical assumption that investors anticipate the effect of sudden events (e.g., corporate announcements, regulatory policy changes, political events, etc.) on future profitability of a firm or firms in a market, and importantly, that these anticipated changes in profitability are *immediately* reflected in stock market prices. The approach therefore implicitly relies on the efficient market hypothesis, or the idea that in an efficient market, stocks instantly incorporate publicly available information into their prices (Fama 1965; Samuelson 1965). The immediate nature of this effect allows us to use the magnitude and direction of sudden fluctuations in granular daily data as a proxy for investors' expected valuation of the event in terms of future profits. Given that stocks immediately incorporate publicly available information into their prices and reflect investor's future expectations of profitability, they may be used as an indicator of how global financial actors value different kinds of politically unstable events---for example, pro-vs-anti business coups."

2. Building on the previous point, readers may be interested to understand what are the underlying macroeconomic links—theoretically speaking—that drive the effect of political events on stock market returns and volatility? Or how financial market actors may rationally respond to political events in ways that influence returns and volatility of daily stock prices. Some substantive interpretation or theoretical intuition underlying the key empirical results is perhaps worth discussing even briefly.

We thank the reviewer for their suggestion to expand our discussion of the macroeconomic links that drive changes in the stock market. As the reviewer notes, expected future macroeconomic performance is the theoretical link through which these events are expected to influence immediate stock market returns. We therefore now discuss these macroeconomic links together with the additional discussion of the theoretical mechanisms expected to influence market returns noted above. Specifically, we now state on pp. 5-6:

"The theoretical linkage through which political events are expected to have an impact on daily stock prices is changes in the broader macroeconomic environment that influence the expected stream of future profits of existing publicly-held firms. Examples of expected changes in the macroeconomic environment that can be influenced by sudden political events include: shifts in monetary policy, changes in trade or FDI openness, shifts in regulatory policy, changes in the share of output going to capital, transfer and convertibility restrictions, degree of resource nationalism, and heightened risk of capital expropriation or breach of contracts. Not all shocks to the political system will result in similar shifts in these

macroeconomic variables depending on the actors involved, their ideological preferences, and their expected ability to enact policy changes.”

3. I am not an expert on Argentina's economic history. But I suspect that scholars are generally aware that from the late-1970s to at least the late-1990s and beyond, Argentina experienced currency crises, inflationary crises, and (if one recalls correctly) banking crises as well. Given that political events such as different kinds of coups, resignations, and regime-change can cause deep—and likely persistent—economic shocks that influence financial markets (including the stock market), I am somewhat surprised that the authors did not consider employing Exponential GARCH models. Such models tend to carefully capture sudden or rapid (but sustained) "dips" and "jumps" in the daily stock price data that likely occurred during the political events that occurred. The authors could have also possibly estimate some Threshold ARCH models. These are not necessary per se but perhaps worth estimating for robustness checks.

> **We thank the reviewer for this observation and suggestion. We have now conducted robustness checks using both exponential GARCH and threshold GARCH models. Figures showing the volatility estimates using eGARCH and tGARCH models have therefore been added to the Appendix as Figures A.10 and A.11. These results corroborate the results using the GARCH(1, 1) specification, showing some increases in volatility preceding the regime change, but with a much larger spike in volatility occurring on the event day.**

4. The temporal range of the authors' daily stock price data across several stock market indices covers a wide range of years and is highly disaggregated as well. While this helps to capture stock price volatility and its persistence, daily stock price data (especially over long periods of time) tends to exhibit fractional integration. I suggest implementing asymptotic tests for fractional integration, such as the Geweke-Porter-Hudak test, the modified rescaled range test and Lagrange multiplier type tests. These tests are well suited for assessing high frequency time-series data that span several decades. If fractional integration is an issue, then the authors may consider using FIGARCH or FEIGARCH models as well to avoid spurious results.

> **We thank the reviewer for this suggestion. We have added mean volatility estimates from FIGARCH models to the robustness section of the appendix in order to account for potential fractional integration in the daily stock price time series data (see Appendix Figure A.12). These results also comport with the original analysis and the other robustness checks.**

5. It would also be worthwhile for the authors to demonstrate, illustrate and assess potential structural breaks in the data via standard diagnostic tests. Assessing this is useful because volatility persistence in GARCH models and spurious long memory in autoregressive models tends to arise if there are multiple structural breaks and thus structural changes in the data that are not incorporated in the estimated model. If there are multiple structural breaks in their data on daily stock prices (which is plausible given the temporal range of their data and the number of political shocks captured by their data), then conducting some structural break tests may be necessary.

We thank the reviewer for this suggestion. We now implement the Bai and Perron (2003) test for multiple structural breaks on all 36 indices in our sample, and find evidence of structural breaks in 8 of 36 time series. We therefore now run a robustness check of our volatility estimates by re-running our GARCH(1,1) model including dummy variables indicating the periods in which a structural break is present. Please see the new Appendix Figure A.13 for the results of this analysis, which comports with the other robustness checks.

We refer to the tests associated with Reviewer 2's comments 3-5 on the bottom of p.13, and again in footnote 10.

6. Finally, the conclusion could draw out the main implications and potential policy lessons learnt from the main findings presented in the paper. The current version of the conclusion is generally well written and does a good job of discussing the theoretical contributions. But it is worth writing a paragraph or so that delve into the policy lessons reader can obtain from the paper's main empirical analysis.

We thank the reviewer for this suggestion. We have added a paragraph to the conclusion with policy lessons and implications for practitioners on p. 32. This reads as follows:

"Our findings have a number of potential implications for policymakers. First, our finding that even democratically subversive irregular regime changes can have a positive impact on investment when the replacement government is clearly pro-business implies that investor goals and actions may be at odds with regime stability. Second, the immediate nature and large magnitude of the financial outflows we observe in the wake of the majority of coups and assassinations suggests that a weakened financial system and economy are likely to accompany these kinds of regime changes. More optimistically, however, we also find suggestive evidence that in the event of a democratic regime change or revolution, clearly signaling a commitment to democracy may result in increased investment in a country's firms."

Reviewer: 3

This paper aims to estimate the effect of political instability on economic performance. It does so by estimating average stock returns after episodes of regime change- namely coups, assassinations and resignations- using a pooled event-study approach.

The paper touches a relevant topic and is very well written and organized. I really enjoyed reading it. However, I have some concerns about the research design and the interpretation of results. As I explain in some detail below, I don't think that this research design can meaningfully answer the research question posed by the paper. There are three main reasons: first, the effect of a regime change on political instability is ambiguous; second, stock returns are a poor measure of overall economic performance; third, several of the events in the sample are likely to be endogenous and anticipated, as signaled also by the pre-trends that are visible in the data. I discuss each of these points below.

1. This paper aims to assess the impact of political instability on economic performance. My first concern is that what the paper estimates is only weakly and indirectly related to both political instability and economic performance.

Political instability here is identified with 'one-shot' regime changes. But political instability refers to the volatility of political institutions and political power over some period. For example, the frequency of regime changes, or the variability in the government's approach in some key policy areas, over some period.

> **We thank the reviewer for this comment. We have clarified that we are adopting a short-term definition of political instability that is akin to a sudden and unexpected "instability shock" event, as the Reviewer notes. We recognize that this definition may not comport with standard (long-term) definitions in political science, but argue that this short-term approach is more applicable to our outcome measure of daily financial returns. In that vein, we now discuss the theoretical underpinnings of how and why sudden changes in daily returns reflect investor expectations of future financial performance at the beginning of the "Estimation strategy" section on p.12 in order to better justify this approach. Specifically, we now state that:**

> **"The focus on *immediate* fluctuations in daily financial data implies that we are examining the effect of "one shot" regime changes akin to a sudden day-of shock. Our estimates therefore capture the effect of these sudden, unexpected shocks on stock prices on the same day of the shock. Importantly, this implies that we are not estimating the effect of any larger underlying political conditions surrounding the event, but rather only the immediate event itself independent of other surrounding conditions. Further, as this approach provides an estimate at the exact time of the event, confounding events would need to occur on the same day, and do so for a large portion of all of our independently tested events in order to influence our estimates. We recognize that broader definitions of political instability may encompass broader political volatility over a wider range of time. In an attempt to address**

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this concern, we also examine the impact of periods of protest on financial returns in the appendix.”

The paper implicitly assumes that a regime change always leads to higher political instability—that is, an increase in the expected frequency of government changes or policy changes. But this is not the case: some regime changes can lead to reduced instability, if the new regime is seen as more stable than the previous one, for example because it has a stronger power base, or more unitary political preferences, relative to the previous government. One could actually argue that generally regime changes tend to resolve uncertainty, and therefore should be identified with decreased instability (at least on average).

> **We thank the review for demonstrating the need to further clarify that it is *not* our intention to suggest that a regime change always leads to higher political instability. In fact, our theoretical argument is predicated on the idea that a coup or resignation may lead to less uncertainty for investors. For example, investors may expect increased stability after a resignation following a period of public protest. The theoretical discussion on p.5 therefore now begins with “it is not necessarily the case that even an unexpected regime change will result in higher levels of instability” and presents a larger discussion of how sudden regime changes may suggest increased stability to investors.**

For these reasons, I think that the author’s conclusion that “While conventional wisdom suggests investors dislike political instability, we show that this is not necessarily always the case” is not actually supported by the empirical analysis. It seems perfectly possible that instability always reduces stock market valuations, but some of the events in the sample reduced (rather than increased) instability.

> **Similar to our response above, we agree with the reviewer that some events in our sample, particularly resignations, may be perceived as increasing stability. We have therefore altered the first paragraph of the conclusion on p.31 to read: “While conventional wisdom suggests investors dislike *unexpected regime changes*, we show that this is not necessarily always the case. Unexpected changes in ruler virtually always increase market volatility, but the directionality is not always negative as markets can be given a boost when a new regime is expected to offer a more *stable*, democratic, or pro- business environment than the previous one.”**

2. With regard to economic performance, the paper assumes that stock returns are a measure of overall economic performance (“growth and investment” in particular). But stock prices only reflect the present value of the expected stream of future profits of existing publicly-held firms. Higher stock prices need not reflect an increase in expected long-run output. They might just reflect a greater expected share of output going to capital, or an expected redistribution from (usually smaller) non-publicly held firms towards publicly-held firms, or an expected increase in barriers to entry that favors existing firms over potential new-entrants. Or a decrease in expected interest rates.

> **We thank the reviewer for their suggestion to clarify that stock returns are not akin to a measure of overall macroeconomic performance. In response to this suggestion—as well as Reviewer 2’s suggestion to clarify the theoretical macroeconomic links that drive the effect**

of political events on stock market returns and volatility—we now clarify that changes in stock market returns/expected future profits are theoretically driven by changes in the broader macroeconomic environment, which may differ from expected (or actual) long-term economic output and growth. We therefore now state on pp. 5-6 that "the theoretical link through which political events are expected to have an impact on daily stock prices is changes in the broader macroeconomic environment that influence the expected stream of future profits of existing publicly-held firms. Examples of expected changes in the macroeconomic environment that can be influenced by sudden political events include: shifts in monetary policy, changes in trade or FDI openness, shifts in regulatory policy, changes in the share of output going to capital, transfer and convertibility restrictions, degree of resource nationalism, and heightened risk of capital expropriation or breach of contracts. Not all shocks to the political system will result in similar shifts in these macroeconomic variables depending on the actors involved, their ideological preferences, and their expected ability to enact policy changes."

We also now recognize that the title of the paper may suggest greater implications for general macroeconomic performance than the empirical results imply. We are therefore open to changing the title of the paper to "Are regime changes always bad for markets: Evidence from daily financial data" in order to alleviate this potential misunderstanding.

It should also be mentioned that stock returns after an event measure the expectation that investors hold about the effect of the event, which need not coincide with the actual effect that the event will have (expectations can be wrong). The case of the failed 2002 coup against Chavez, which the paper examines, is instructive in this respect: most likely, stock returns during that event reflected the fear that Chavez would redistribute income away from capital, rather than (or in addition to) changed expectations about long-run output. In sum, stock returns are not a good measure of general economic performance.

We thank the review for this important point. We have also clarified in the introduction that we "examine the impact of irregular regime changes and public protests on investment in a country's firms," and that this may differ from broader economic performance. Specifically, we now state on p.2 in the introduction that: "Stock market returns are used as an indicator of whether investors view different types of sudden political shocks as "good" or "bad" for future profits. Importantly, expected future profits may differ from general economic performance, as stock prices only reflect the expected future profits of existing publicly-held firms." In addition, on p. 6 we now note that "changes in stock prices reflect investor's expected changes in the macroeconomic environment, and these expectations may differ from realized future economic performance."

3. A third major concern regards the fact that, in order to provide correct estimates of their effects, these events must be assumed to be exogenous and unexpected, and the related issue of pre-trends in the data. From Table 3, mean returns in the 6 days before coup events are actually larger than mean returns in the six days after the event (2.010 versus 1.545), and basically as large in absolute value as average one-day post-event returns (-2.137). For some of the single coup events, it is quite clear that returns are above or below normal already before the event. For example, the pattern for Thailand 1977 strongly suggests that

the change did not constitute a surprise, given the remarkably large positive returns in the days before the event and the small and not significant post-event return. Also Table 5 (estimating the effect of resignations) displays some pretty large pre-trends for several of the individual events (see for example Turkey 1997, Argentina 2001, Tunisia 2011, Thailand 1992, ...). This fuels concerns about endogeneity and anticipation effects. Regime changes seem likely to be triggered, or at least correlated with, poor economic performance, which would also show up in average returns, biasing estimates. Moreover, many regime changes will be (at least partially) anticipated, and thus already discounted in stock valuations, which makes it incorrect to interpret post-event returns as average effects.

We thank the reviewer for this comment. While we agree that at the individual event level not all events are necessarily unexpected, we stress that: (1) the pre-event returns are not significantly different from zero when aggregated across all events, and (2) the directionality of all individual significant pre-event trends is positive while the event day effect is negative. If investors anticipated the coup (and reacted negatively upon its occurrence), we would expect the pre-event trends to also be negative.

Nevertheless, we now provide an additional robustness check removing all events with significant pre-event trends (in any direction) in the appendix (see Tables A.3-A.5). The directionality, magnitude, and significance of the estimated effects remain substantively the same using this subsample of events.

The "Robustness" section tries to address concerns about endogeneity and anticipations. The results, however, seem to be consistent with the idea that the events studied are endogenous and at least partly anticipated. In Figure 4 there are pretty strong pre-trends in the cases of coups and resignations. Moreover, this kind of aggregate analysis might mask pre-trends, because it takes averages over events which can have pre-trends in opposite directions, due to the ambiguous expected effect of these events.

Similar to our note above, we stress that if the events were expected, pre-trends would in theory be expected in the same direction as the effects of the events themselves. However, the pre-trend that exists for resignations is in the opposite direction of the event-day returns, and there is a clear break between the pre and post event trend. This suggests that investors were on average expecting continued political turmoil and were relieved by sudden resignations on the event day. To aid clarity on this point, we have added the following note to p. 29: "For resignations, CARs trend downward in the 10 days prior to the event; however, if investors were aware that a resignation were about to occur one would expect the pre-event CARs to be positive given the positive CARs observed in the post-event window. By contrast, a clear break exists between the negative pre-event trend and positive event day jump and post-event trend."

Pre-trends and endogeneity are to be expected, because the paper uses all available events, without an identification strategy to identify a subset of exogenous and unanticipated events and focus on them.
I think that the section "Exploring possible mechanisms" is promising, and it constitutes in my view the most interesting part of the paper. Unfortunately, it is also rather brief and mostly descriptive. I think that a more extended and refined version of this section could constitute an interesting paper, if it found a way

to identify a subset of these events that are plausibly exogenous and unexpected, and if it appropriately took into account that the outcome variable reflects firms' profits, not overall social welfare.

We thank the reviewer for the suggestion to identify a subset of events that are most plausibly exogenous. As noted above, we therefore now subset all events in each category of instability to only those with 7-day pre-trends that are not significantly different from zero at the 5% level, and rerun the analysis as a robustness check (the results can be found in Appendix Tables A.3-A5). This analysis results in roughly the same estimates for coups and assassinations. For resignations, the magnitude of the estimate is reduced by roughly half (i.e., from +4 to +2), but the estimate remains statistically significant at the 1% level and the sign/direction remains positive.

This paper examines empirically whether successful coups and assassinations as well as leader resignations produce abnormal stock market returns. The paper does not really offer a theory for why or how these political events influence market returns and the many of the results presented in Tables 3-5 do not show a discernible, statistically significant pattern. Thus I am unclear what we can learn about how political events influence stock returns.

We thank the reviewer for their suggestion to expand our discussion of theoretical mechanisms, which is in line with comments from Reviewer 2. As noted in our response to Reviewer 2, we now clarify the macroeconomic linkages through which daily stock returns would be theoretically expected to be influenced by sudden political events in a new "Estimation strategy" section beginning on p.12. In the "Estimation strategy" section, we have added additional discussion of the theoretical mechanisms underlying event studies that justify the method's use and our use of daily financial data.

We also stress that event studies commonly aggregate estimates across groups, rather than only estimate effects for individual firms/markets/etc. (we include the event-level estimates in order to maximize transparency). The fact that "many of the results presented in Tables 3-5 do not show a discernible, statistically significant pattern" therefore does not preclude the aggregate results being both statistically and substantively significant. We therefore stress that the mean, aggregate results presented on the bottom of Tables 3-5 all show a discernible, statistically significant pattern on day 0 (i.e., the event day) for each type of event presented, and that these effects persists following a multitude of robustness checks, including when comparing against a synthetic "control" portfolio of national financial indices not exposed to political stability.

As importantly, the design does not address selection into the event. Put simply, underlying political conditions that cause these kinds of political events are likely influencing both the event and the abnormal returns, in which case the events themselves are simply epiphenomenal. There are designs that can help alleviate these concerns; implementing them would entail new and updated data collection and analysis.

We thank the reviewer for highlighting the need to strengthen our description of the identification assumptions underlying our event study approach. We have added a new section to the paper entitled "Estimation strategy" which on p.12 which now notes that:

"The focus on *immediate* fluctuations in daily financial data implies that we are examining the effect of ``one shot'' regime changes akin to a sudden day-of shock. Our estimates therefore capture the effect of these sudden, unexpected shocks on stock prices on the same day of the shock. We recognize that broader definitions of political instability may encompass political volatility over a wider range of time. In an attempt to address this concern, we also examine the impact of periods of protest on financial returns in the appendix. The key identifying assumption behind our interrupted time-series approach is

that the events are unpredicted in an immediate temporal sense. In other words, for an event to be plausibly exogenous, the exact timing of the event must be unknown to investors. If information about the event has leaked prior to occurrence of the event, a portion of the change in investment will already be priced into the stock during the estimation window.

From an inferential perspective, the paper should say something about how the outcome – abnormal returns – for the sample of countries that experience these events compares to the outcome for countries that do not experience these tumultuous political events. In short, is there something different about returns in places with political instability than places that are more stable? This comparison would be a nice first cut in understanding the selection issues involved in answering the main research question.

> **We thank the author for this suggestion, which is in fact the rationale underlying the synthetic control approach taken in the robustness section. We have therefore borrowed the reviewer's language in describing this analysis and have expanded our discussion of the synthetic control method. We now note that the synthetic control portfolio represents a basket of all countries which did not experience tumultuous political events, and compares returns in countries that *did* against this basket of countries that *did not*. In this sense, the analysis shows that there is indeed something different about returns in places with political instability than places that are more stable**.

> **Specifically, we now note on p. 29 that: "This approach creates a "synthetic" time series of daily financial returns based on the returns in a basket of countries which did not experience tumultuous political events on the day of the event. We are then able to compare the returns in the country which experienced the unexpected regime change with the actual returns of a reference market—in this case all countries that did not experience an unexpected regime change… In contrast with the large and significant abnormal returns in the actual countries of study, the returns in the synthetic control portfolio are never significantly different from zero for coups, assassinations, or resignations (or when aggregated together)."**

Second, assassination and coup attempts are not random events, even within the sample of countries with political instability. The occurrence of these events often coincide with – and, indeed, are preceded by – some type of social unrest or external event that may cause both the event attempt and that may influence market returns. For example, coup attempts are often preceded by protests, which themselves may be a response to economic instability.

> **We thank the reviewer for the recommendation to examine social unrest that precedes many of our events. Many of the resignations in our sample are indeed preceded by large-scale protests. We therefore point the reviewer to the expanded Public Protests section of the Appendix (see Figures A2-A9, reproduced below), which now directly models the crises/protests that precede all of the applicable resignations in our sample.**

If we assume that observed assassination or coup success is as if random conditional on attempt, then we can leverage the universe of attempts to examine how success influences the outcome relative to the attempt. The structural and time-varying determinants of attempts to change leadership irregularly are vast, but the theorized factors that contribute to success, conditional on attempt are few. It is much more plausible to assume success is quasi-random conditional on attempt than to assume success – relative to non-success – is quasi-random.

We thank the reviewer for this suggestion, which we agree represents a promising estimation strategy. However, as our current focus of study and estimand is the effect of the occurrence of an irregular regime change vs. no regime change, we believe that the suggested estimation strategy changes our estimand in a way that is outside of the scope of the current paper. We nevertheless think that this is a promising area for future research, and have added the following note to the conclusion. "It remains unclear how markets react to successful irregular regime change conditional on an attempt. A future study could therefore examine how markets react to successful assassinations or coups when compared to failed attempts."

Third, observed confounders such as protest (e.g. Indonesia 1998, Egypt 2011) or currency crises (e.g. Indonesia 1998) should be modeled directly rather than posthoc by subsetting a couple of cases where the successful event was preceded by, for example, a protest.

We thank the reviewer for this important point. Please see our response to the second point above, noting that we now directly model protests as distinct events. This analysis also shows that volatility and the absolute value of returns increase during times of protest, but also reveals that the resignations themselves constitute distinct events that are typically clearly distinguishable from the general period of protest.

Finally, the Polity data on coups is inconsistent and not well-documented. I would recommend using either the Powell and Thyne data, which are cross-checked with other extant coup data sets, or, better-yet, the Colpus data that contains detailed cross-checks and narrative descriptions of the events, including all coup attempts (and even failed coup plots). It is not enough to dismiss assessing failed coup attempts simply because Monty Marshall – who coded the Polity data – thinks it is hard to collect data on failed instability events.

We thank the reviewer for this recommendation. However, while we agree that assessing the impact of failed coup attempts is an interesting research question, our choice to not assess failed coups is theoretical in nature. As previous researchers (including Powell and Thyne 2011) have noted, it is "difficult to identify ambiguous forms of coup activity, such as coup failures, plots, and rumors." We therefore believe that attempting to ascertain which failed coup attempts belong in the present analysis is outside of the scope of the current paper, and that focusing on the effect of successful irregular regime changes allows us to examine a more cleanly estimated and generalizable effect. We acknowledge that we include one failed coup in a separate analysis (the 2001 Venezuelan coup attempt). However, our decision to

examine this event is due to its seemingly successful nature, which allows for a more direct comparison with the other successful coups in our sample. Nevertheless, we have noted in the conclusion that assessing the impact of failed regime changes is a promising area of future research.

To ease interpretation of the results presented in the main tables, it would be helpful to mark if any of the differences between pre- and post-event are statistically significant across similar windows: e.g. compare the Argentina 06/08/1970 [-1,-7] window to the [0,6] window. Are these estimates statistically different from each other? The means for coups and assassinations appear not to be statistically different from each other in Tables 3 and 4. And less than half of the resignation events in Table 5 appear to have statistically different pre- and post returns.

We thank the reviewer for this suggestion. However, the causal effect that can be identified with the most confidence (and which captures our theoretical quantity of interest) is the comparison between the abnormal return on the event day itself vs. the whole time window (we acknowledge that we also include larger post event windows as a secondary effect and pre-event effects for diagnostic purposes). We therefore prefer to focus reader attention on the event day returns as the identification assumptions are well-established in previous literature, and the reliability of the estimates diminish beyond this point and comparison (there are limitations and concerns regarding reliability of longer horizons of estimation for event studies. See for example Kothari and Warner, 1997; Lyon, Barber, and Tsai, 1999; Brown and Warner, 1980; Khotari and Warner 2006).
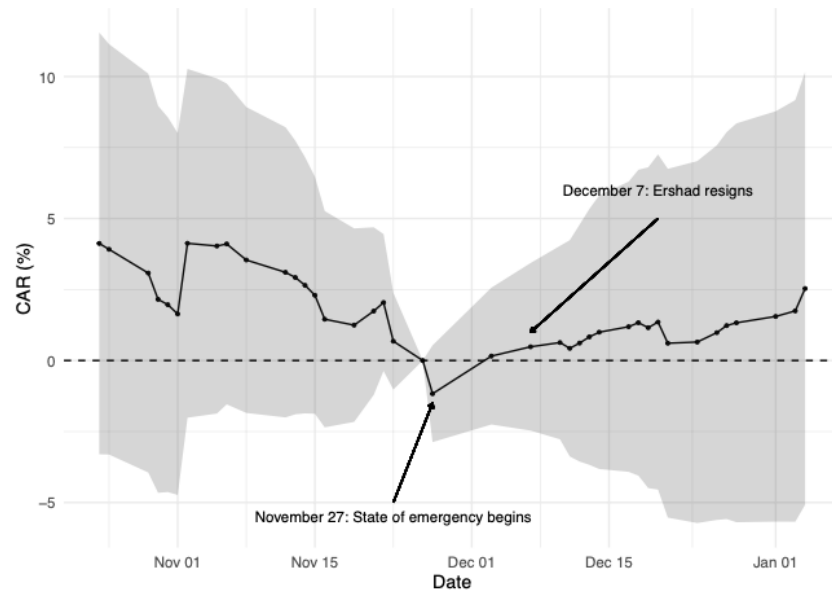
Newly added figures tables
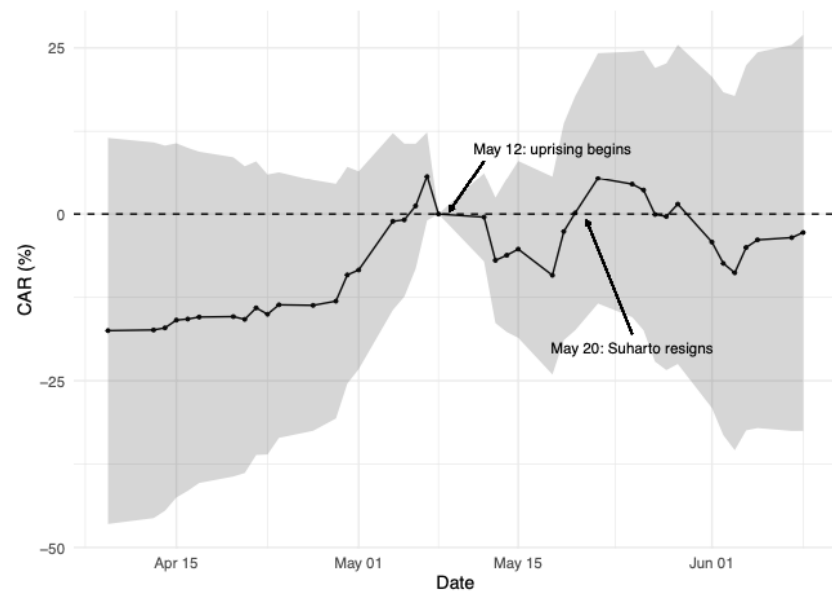

Figure A.2: CARs during the 1990 Bangladesh uprising

Figure A.3: CARs during the 1998 Indonesian revolution

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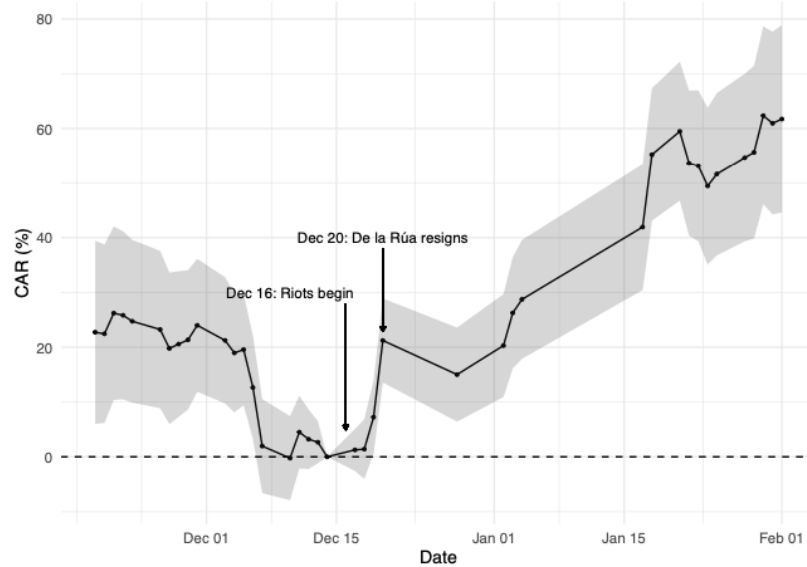**Figure A.4: CARs during the Philippines Second People Power Revolution**

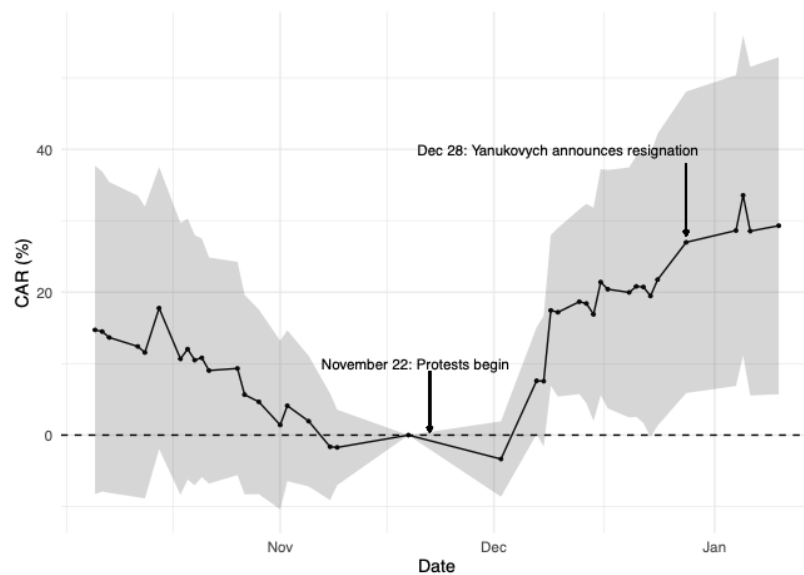**Figure A.5: CARs during the 2001 Argentine crisis**

Figure A.6: CARs during the 2004 Orange Revolution
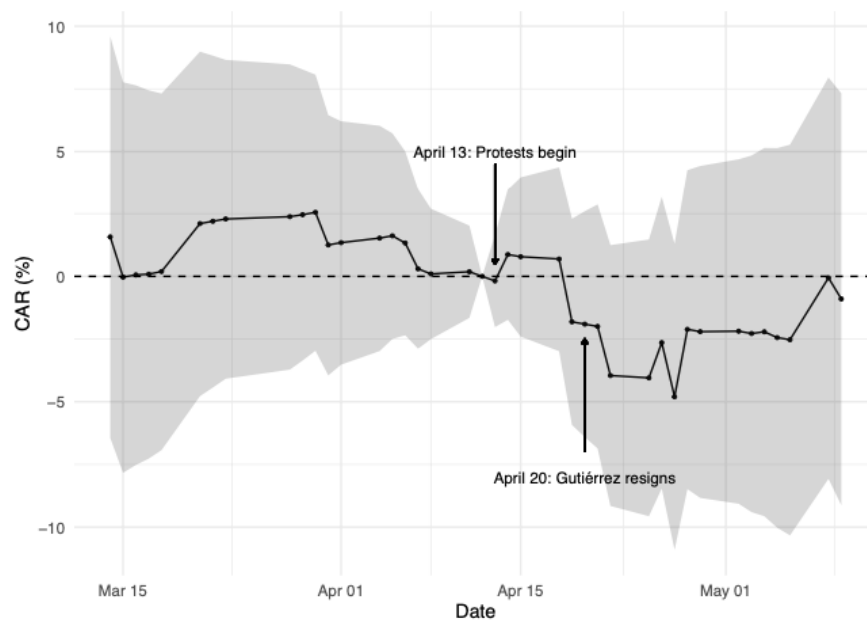

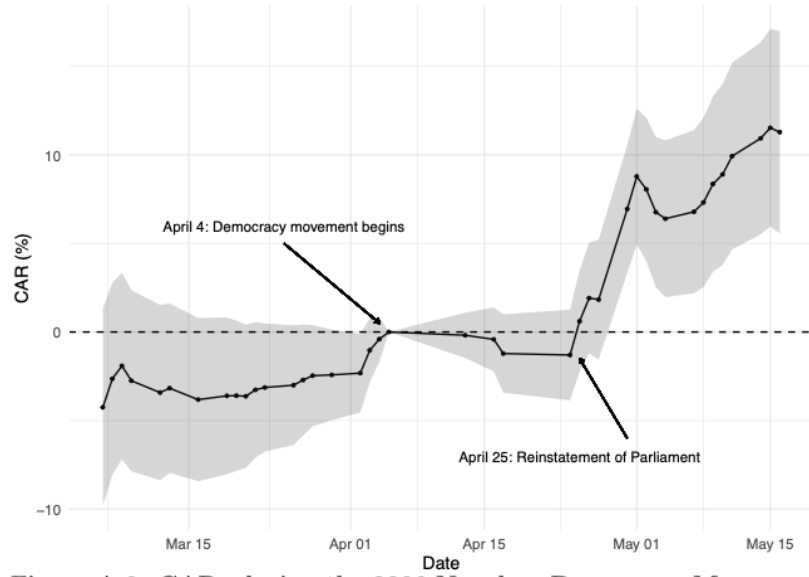**Figure A.7: CARs during the Ecuadorian Revolution of 2005**

Figure A.8: CARs during the 2006 Nepalese Democracy Movement
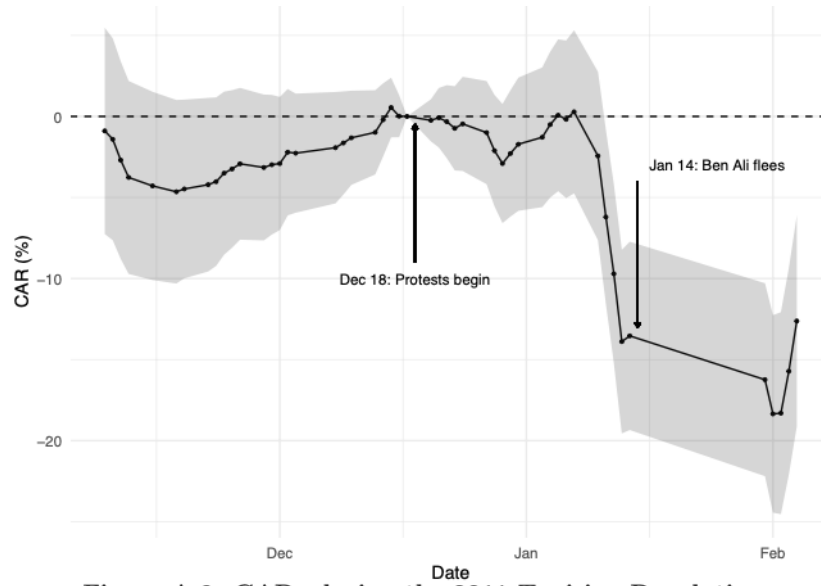

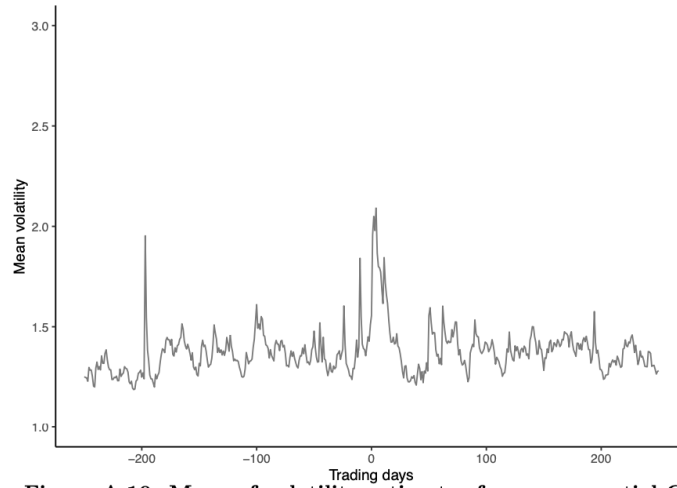**Figure A.9: CARs during the 2011 Tunisian Revolution**

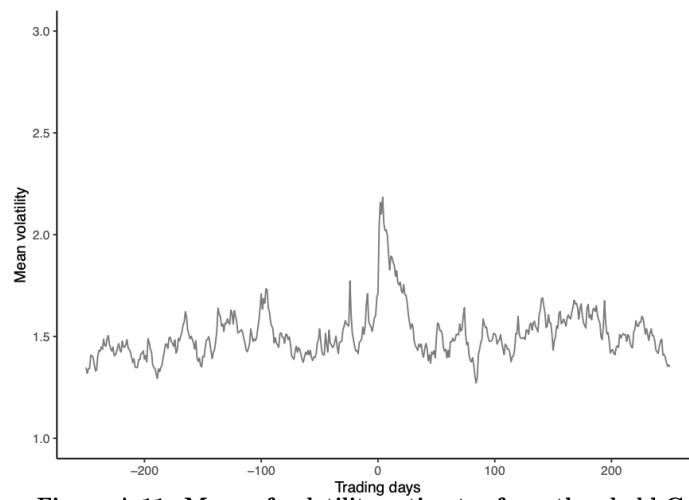Figure A.10: Mean of volatility estimates from exponential GARCH models

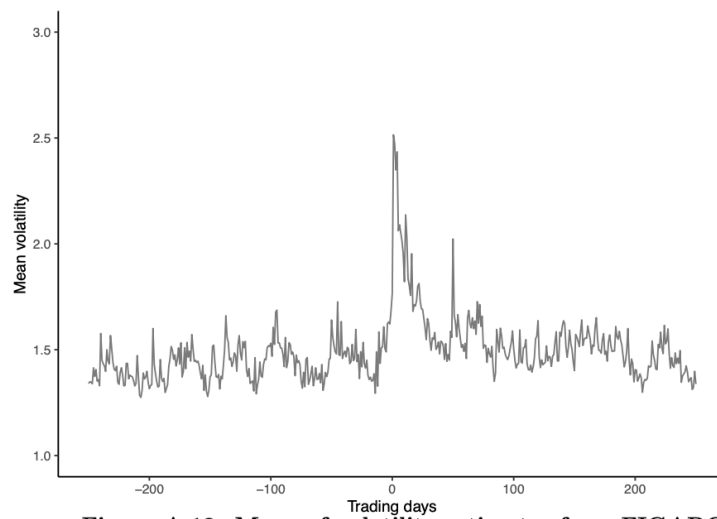Figure A.11: Mean of volatility estimates from threshold GARCH models

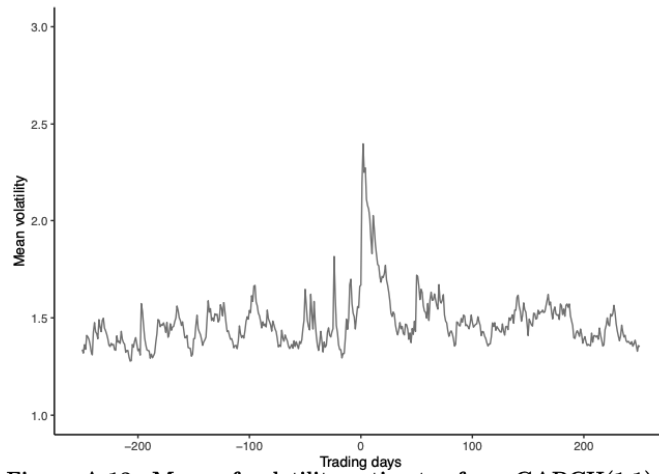Figure A.12: Mean of volatility estimates from FIGARCH models

Figure A.13: Mean of volatility estimates from GARCH(1,1) models with adjustment for structural breaks

Abnormal returns for events with insignificant 7-day pre-trends only

Table A.3: Abnormal returns following coups

Country	Event Date	Post-Event CAR			Pre-Event CAR		Days to rebound
		(0,0)	(0,6)	(0,19)	(-1,-7)	(-1,-20)	
Argentina	06/08/1970	−1.919	−0.530	−2.011	0.247	4.728	204
		(0.949)	(2.510)	(4.243)	(2.510)	(4.243)	
Argentina	03/22/1971	0.925	14.294	24.218	0.131	0.274	
		(1.216)	(3.218)	(5.439)	(3.218)	(5.439)	
Bangladesh	01/11/2007	−0.320	10.351	14.883	−0.896	2.250	2
		(1.166)	(3.086)	(5.217)	(3.086)	(5.217)	
Egypt	07/03/2013	−0.346	5.169	7.144	6.776	−4.869	2
		(1.515)	(4.009)	(6.776)	(4.009)	(6.776)	
Nepal	10/04/2002	0.090	1.563	5.567	−1.014	−0.493	2
		(1.206)	(3.190)	(5.392)	(3.190)	(5.392)	
Pakistan	10/14/1999	−7.737	−9.431	−7.130	4.151	4.900	36
		(1.943)	(5.141)	(8.690)	(5.141)	(8.690)	
Peru	04/06/1992	−6.819	−5.814	−25.027	−2.075	−10.519	5
		(2.210)	(5.848)	(9.885)	(5.848)	(9.885)	
South Korea	12/12/1979	−1.784	−3.474	−24.465	−1.678	−6.187	418
		(1.152)	(3.047)	(5.150)	(3.047)	(5.150)	
Thailand	10/06/1976	−0.541	0.837	0.731	0.001	0.713	3
		(0.639)	(1.691)	(2.859)	(1.691)	(2.859)	
Thailand	02/25/1991	−7.326	2.860	14.162	6.326	26.262	7
		(2.884)	(7.631)	(12.899)	(7.631)	(12.899)	
Thailand	09/19/2006	−0.481	−2.640	0.111	1.848	0.131	17
		(1.094)	(2.894)	(4.892)	(2.894)	(4.892)	
Thailand	05/23/2014	−0.571	2.800	4.591	2.350	−0.424	5
		(1.201)	(3.177)	(5.370)	(3.177)	(5.370)	
Mean		−2.236	1.332	1.064	1.347	1.397	63
		(0.448)	(1.184)	(2.001)	(1.184)	(2.001)	

Notes: Standard errors are in parentheses. "Days to rebound" is the number of trading days following a negative stock return for the national stock index to return to pre-event level (it is calculated if the price decreases on the event day, not if the event day abnormal return is negative). Returns are inflation adjusted.

Table A.4: Abnormal returns following assassinations

Country	Event Date	Post-Event CAR			Pre-Event CAR		Days to rebound
		(0,0)	(0,6)	(0,19)	(-1,-7)	(-1,-20)	
Israel	11/05/1995	−3.460	−3.177	0.743	−0.857	−10.316	12
		(1.473)	(3.897)	(6.587)	(3.897)	(6.587)	
Nepal	06/12/2001	−0.513	2.965	15.516	5.956	1.791	20
		(3.513)	(9.295)	(15.711)	(9.295)	(15.711)	
South Korea	10/26/1979	−0.364	−9.376	1.186	0.690	−0.368	14
		(1.058)	(2.800)	(4.734)	(2.800)	(4.734)	
Sri Lanka	05/03/1993	−3.231	−0.983	3.515	−0.541	−1.360	7
		(0.767)	(2.030)	(3.432)	(2.030)	(3.432)	
Sweden	03/03/1986	0.698	5.038	10.908	−3.754	0.955	
		(0.927)	(2.452)	(4.145)	(2.452)	(4.145)	
United States	09/07/1901	−4.522	−3.055	−8.920	−0.733	3.456	963
		(1.283)	(3.394)	(5.738)	(3.394)	(5.738)	
Mean		−1.899	−1.431	3.825	0.127	−0.974	203
		(0.721)	(1.908)	(3.225)	(1.908)	(3.225)	

Notes: Standard errors are in parentheses. "Days to rebound" is the number of trading days following a negative stock return for the national stock index to return to pre-event level (it is calculated if the price decreases on the event day, not if the event day abnormal return is negative). Returns are inflation adjusted.

Table A.5: Abnormal returns following resignations

Country	Event Date	Post-Event CAR (0,0)	(0,6)	(0,19)	Pre-Event CAR (-1,-7)	(-1,-20)	Days to rebound
Bangladesh	12/07/1990	0.323	1.002	2.171	1.880	3.654	
		(0.871)	(2.305)	(3.896)	(2.305)	(3.896)	
Ecuador	04/20/2005	−0.084	−0.249	−0.595	−1.305	0.710	
		(0.945)	(2.499)	(4.225)	(2.499)	(4.225)	
Indonesia	05/20/1998	2.817	4.296	4.543	−2.695	−17.868	
		(3.392)	(8.974)	(15.168)	(8.974)	(15.168)	
Lithuania	04/06/2004	−0.575	−3.319	−11.704	2.182	5.426	159
		(1.137)	(3.007)	(5.083)	(3.007)	(5.083)	
Nepal	04/25/2006	1.915	8.132	9.937	−1.951	−4.205	
		(0.665)	(1.760)	(2.975)	(1.760)	(2.975)	
Pakistan	04/19/1993	−3.265	−0.432	2.771	−0.312	−0.485	15
		(1.108)	(2.930)	(4.953)	(2.930)	(4.953)	
Pakistan	11/06/1996	5.084	1.229	−0.441	4.182	7.597	
		(1.416)	(3.746)	(6.331)	(3.746)	(6.331)	
Philippines	02/26/1986	12.938	21.473	23.086	−1.847	−6.884	
		(0.477)	(1.263)	(2.134)	(1.263)	(2.134)	
Philippines	01/19/2001	1.150	16.837	18.469	−5.382	3.581	
		(1.591)	(4.209)	(7.115)	(4.209)	(7.115)	
Thailand	05/25/1992	3.248	−6.574	3.789	−5.085	−10.841	
		(1.433)	(3.793)	(6.411)	(3.793)	(6.411)	
Turkey	06/30/1997	2.010	−2.861	−7.629	12.876	4.532	
		(3.015)	(7.976)	(13.481)	(7.976)	(13.481)	
Ukraine	12/28/2004	5.118	12.837	18.445	4.170	32.085	
		(2.797)	(7.401)	(12.511)	(7.401)	(12.511)	
Mean		2.557	4.364	5.237	0.560	1.442	87
		(0.526)	(1.392)	(2.353)	(1.392)	(2.353)	

Notes: Standard errors are in parentheses. "Days to rebound" is the number of trading days following a negative stock return for the national stock index to return to pre-event level (it is calculated if the price decreases on the event day, not if the event day abnormal return is negative). Returns are inflation adjusted.