Information, Elections, and Political Change

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While liberal democracy has become more common since the end of the Cold War, non-democratic regimes remain the modal form of government around the world.[1] Consequently, much scholarly and political energy has continued to be devoted to understanding the ways in which authoritarian regimes work and how they might open up or liberalize politically.

Perhaps the most contested issue in this literature—both theoretically and empirically—has been the role and impact of elections in authoritarian regimes. Most scholars regard elections as stabilizing to authoritarian regimes because they facilitate the rotation of cadres and the sharing of access to rents and policy,[2] demonstrate incumbent strength and thus intimidate would-be challengers,[3] and provide information on the strengths and weaknesses of specific elites, thereby facilitating the selection of loyal and effective officials.[4] However, others have viewed authoritarian elections as opportunities for democratic development. Instead of showing incumbent strength, elections might create incentives for previously divided oppositions to unite.[5] Alternatively, repeated experience of elections might promote democracy by gradually inculcating democratic norms.[6]

Empirically, there is little cross-national evidence that elections help to make authoritarian regimes more durable. In fact, some argue that authoritarian regimes with at least some genuine competition in elections are more vulnerable to democratization than those without,[7] though others suggest that any such democratizing effects are short-lived.[8]

In this article we consider the relationship between elections and the political dynamics of contemporary authoritarianism. While we do not deny that elections may be useful for the reasons that have been put forward, we contribute to the growing evidence that the decision to hold elections in an authoritarian regime carries costs as well as benefits.[9] The reason, we argue, is that the electoral process in an authoritarian environment has the potential to generate information that changes perceptions of incumbents and opposition, challenges existing alliances and coalitions, and disrupts the status quo. In the otherwise information-poor environment of authoritarian regimes, elections represent a unique opportunity for incumbents, opposition, key elites, and mass publics to gather, communicate, and share existing information and to learn

new—and perhaps surprising—political information. These actors use what they learn about the candidates, the electorate, and each other to decide whether to continue bandwagoning with the incumbent leadership, or to break with it. If the incumbent coalition holds, neither liberalization nor deliberalization is likely. However, if the process of elections leads to changes in political coalitions, for example by inducing key politicians or other actors like the courts or security forces to change sides or by encouraging previously passive social groups to get involved, then change is likely. Moreover, political liberalization is not the only possible result of the dynamics unleashed by elections. Upsetting the ruling authoritarian coalition might instead lead to deliberalization if hardliners assert themselves and win the resulting struggle. In our view, as the early work on transitions understood well,[10] political liberalization is a knife-edge process, and elections can easily cut both ways.

We test our theory and a range of alternatives against a new global dataset of political dynamics from 1992 to 2008. Existing studies of political dynamics have tended to neglect the possibility that liberalization and deliberalization can both have the same causes and have used methods that are either not designed to catch changes in an illiberal direction[11] or that average out changes in opposite directions.[12] To address this issue we constructed a new dataset of political liberalizations and deliberalizations that treats these outcomes separately. We then combined this dataset with a number of measures of election and information environments to allow us to look systematically at the electoral dynamics of political change in the post-Cold War era.

The article makes three principal contributions to debates about the relationship between elections, democratization, and authoritarian retrenchment. First, we analyze liberalization and deliberalization in the same framework and demonstrate how both outcomes can be the result of quite similar processes. Thus, while others before us have argued that elections are not necessarily democratizing,[13] we make the stronger causal claim that the causes of deliberalization and liberalization are often very similar. This is in sharp contrast with most of the literature on democratization that sees the causes of these two phenomena as being distinct.[14]

Second, we are amongst the first to take a deeper empirical look at how different kinds of elections are likely to affect the probabilities of political liberalization or deliberalization. We consider not just whether an election takes place, but also look at the effects of cancelled elections, regularly scheduled elections, and unscheduled elections to illustrate the robustness of the election effect on the chances of both political opening and authoritarian retrenchment. We also consider the effect of election quality and demonstrate interesting non-linear effects in the relationship between election quality and both liberalization and deliberalization. This perspective complements and complicates existing work that has tended to see liberalization in a linear fashion as being more likely in a more liberal environment.[15]

Third, we not only demonstrate empirically the ambiguous effect of authoritarian elections but also deepen our understanding of the mechanisms through which elections have an impact. We show that variation in the availability of information about the relative strength of incumbents and opposition plays a major role in shaping political

dynamics and that the degree of media freedom, the presence or absence of international election observers, and the presence and nature of public opinion survey shape the susceptibility of a country to change by elections.

Authoritarians and Elections

Almost all countries hold elections. Moreover, while elections without competition of any kind, typified by those held in single-party communist states, still take place, elections with some form of genuine competition are common even in authoritarian regimes. While scholars focused on the Middle East and other long-standing authoritarian regimes tend to stress the usefulness of elections to authoritarians,[16] those looking at the former communist states or at sub-Saharan Africa tend to see elections with competition as having the potential to undermine authoritarianism. This may happen either through the long-term effects of repeated elections on democratic norms and political competition[17] or through the short-term opportunities elections provide for opposition groups to challenge and overthrow incumbent authoritarians.[18] In this article, we focus on these short-run effects, leaving aside the question of how durable these effects might be.

Scholars have proposed two different short-term modes through which elections can have a liberalizing effect. Looking specifically at the experience of post-communist states, a number of scholars have analyzed the dynamics of "electoral revolutions" (or "electoral breakthroughs") in which dramatic popular mobilizations after elections have overthrown incumbents accused of electoral fraud.[19] Alternatively, authoritarians may be defeated at the ballot box in "electoral turnovers" (or "liberalizing electoral outcomes") brought about by the opposition uniting in a single coalition before the election.[20]

While we agree with many of these arguments, we argue that elections do not just provide opportunities for liberalization but also for deliberalization, i.e. negative changes in the quality of political rights, institutions, or the fairness of elections. For a combination of theoretical and methodological reasons, however, the existing literature has typically not tried to incorporate these two outcomes within the same framework. In this article we develop a measure of short-run political change that puts both liberalization and deliberalization together and allows us to test the possibility that very similar political contexts could promote diametrically opposed political changes.[21]

Authoritarian Elections and Information

Recent years have seen considerable growth in the study of authoritarian regimes, with much of the attention focused on how authoritarians attempt to overcome the significant problems they have in acquiring and evaluating reliable information. For all the secret police and other coercive instruments authoritarians have at their disposal, it is by now well understood that there are few incentives for sincere preference revelation in non-democratic systems and that this can create serious problems in maintaining stability.[22]

Much of the recent scholarship has focused on how authoritarians might use institutions to try to overcome or mitigate these problems. In particular, much has been made of the role that periodic elections can play in information gathering.[23] The focus in this literature has been primarily on what incumbents can learn about their own support and about the qualities of subordinates.[24] However, as we argue here, the process of elections can help not just incumbents but also other political actors to learn more about the nature, positions, strengths, and weaknesses of incumbents and opposition figures and to create new coalitions based on this information that can radically change politics relative to the situation before the election. Thinking about elections as a period of communication and learning in this broader sense leads to quite distinctive expectations about the relationship between elections and change in authoritarian regimes.

In thinking about elections, we draw on previous work on rumor[25] and on political protest[26] that shows how vital signals about regime strengths and weaknesses can be conveyed in repressive environments. We extend this kind of argument to the case of elections. While elections and protest have some common features, such as bringing together disparately held pieces of information, elections are particularly interesting because they provide a structured set of challenges for an authoritarian regime that are rich with the potential to change the strategies of pivotal actors. The challenges inherent in contesting elections test the mettle of the incumbents and the opposition and allow for communication at multiple levels. Moreover, elections do not just bring together existing information but have the potential to create new "facts on the ground." Events at one stage in the process change expectations in subsequent stages in ways analogous to Beissinger's "thickened history,"[27] when the focus of political attention on a clearly specified period with a set of pre-determined routines to fulfill creates enormous potential for unintended consequences and sequences of events. Consequently, elections are a particularly vulnerable time for authoritarian regimes.

To illustrate how this works, we think of elections as consisting of a series of challenges that take place sequentially. The first stage of the election process, the registration of candidates, is of central importance. Here a key task is to control the registration of parties and candidates and to structure the overall campaign environment.[28] While structuring elections in this way is a common practice in "authoritarianism in an age of democratization,"[29] getting it right is difficult. Excluding known oppositionists may be relatively easy but predicting which hitherto loyal candidates or parties might be tempted to run hard against the incumbents, or might attract a large following among the public, is much harder, as Iran's rulers discovered in 2009.

The next stage is the campaigning. During campaigns both the incumbents' and challengers' behavior sends crucial signals to elites and the public. As DeNardo[30] rightly argued, the way in which state repression is exercised is likely to be crucial. Repression conveys two very distinct messages: one about the coercive capacity of the incumbents and another about the incumbents' confidence in their popular support. Vigorous harassment and arrests of opposition candidates might indicate regime strength and determination, intimidating the opposition and potential defectors alike. However, repression can strengthen the opposition by making the incumbents look afraid of an electoral

contest or by facilitating contacts between opposition leaders and the security apparatus.[31] Repression can also make dangerous martyrs out of formerly trusted regime insiders. For example, in the Orange Revolution in Ukraine, the poisoning of former Prime Minister Viktor Yushchenko helped to establish his credentials with the opposition, despite Yushchenko's complicity in repressing previous opposition protests.

The campaign period also represents an important opportunity for challengers both to learn and to send messages. Authoritarian elections often present elites and voters with opposition candidates about whom little is known or whose reputations have been largely shaped by state-run media. Consequently, a key task for challengers is, on the one hand, to convince certain elites (such as security forces) that they are not too threatening and, on the other, to persuade voters that they are worth voting for. In part, this means demonstrating that they have enough support to mount a credible challenge to the regime. Voters are more likely to take a chance on supporting the opposition if they have evidence that the opposition has some possibility of winning. But this is not the only consideration. As Bunce and Wolchik[32] point out, oppositions in authoritarian regimes can be at least as unpopular as incumbents and often have a major task on their hands in convincing voters that change is not only possible but worthwhile. The campaign can also help the opposition to coordinate on a single candidate or a joint slate, as it reveals to members of the opposition their relative strengths and political potential.

The election results themselves can also carry very important signals. The task for the incumbents is to get out the vote for regime-supported candidates and parties. Success may also depend on having an effective network of officials who can effectively falsify results if necessary. Authoritarian incumbents are also likely to try to actively depress turnout in areas known to favor the opposition: the incumbents' repertoires range from relatively benign administrative tactics (such as short polling hours or insufficient ballots) to systematic and large-scale violent campaigns against groups known to side with the opposition, as in the eviction and killing of Kikuyu voters in the Rift Valley prior to the 1992 Kenyan elections.

The leadership's relative success in mobilizing supporters, demobilizing opponents, and falsifying returns not only affects the electoral outcome but also reveals crucial information about regime strength to pivotal elites, the opposition, the populace, and even to core regime members themselves. In some cases, weaknesses will be revealed that can lead to post-electoral concessions to the opposition, as in Mexico in 1988. In other cases, the prospects for liberalization might improve if incumbents do well and gain confidence in their ability to compete, as in the case of the incumbent-led liberalization in Ghana following the 1992 elections. Hence, even the results of seriously flawed elections can communicate information that may affect regime dynamics. Seriously flawed elections that nevertheless end up being close (Zimbabwe 2008) may contribute to the scale and impact of post-electoral opposition mobilizations and protest. By contrast, flawed polls that confirm incumbent dominance (Russia 2007 and 2012) are less likely to be subject to serious challenge.

International actors also matter in the information game. Elections give both international critics and supporters of the incumbent regime an opportunity to signal the

strength or weakness of their commitment. For example, former allies abandoning an incumbent can send a particularly powerful message. In the aftermath of the protests surrounding the Georgian parliamentary elections in December 2003, meetings between the incumbent Shevardnadze and his former ally, the Russian Foreign Minister Igor Ivanov, appear to have played a crucial role in Shevardnadze's decision to stand down.

Finally, the process of ratifying the results requires coordination between the incumbent leadership and other organs such as the Central Election Commission and the judiciary. The extent to which this cooperation will be forthcoming is contingent on players' assessment of the signals in the elections so far. The security forces often play a key role in this process, as they are responsible for controlling potential opposition mobilization in the streets. Maintaining the loyalty of these forces is crucial, especially if the elections turn out to be unexpectedly close. The "Orange Revolution" in Ukraine in 2004 is one recent example of incumbents who "won" the campaign and the vote count but lost the ratification because pivotal elites in the courts and in the security services changed their minds on whom to back in the light of dramatic post-electoral protests. Similarly, in Serbia in 2000, key elites only began to defect from the regime after the opposition presented its parallel vote tabulation, exposing regime fraud to the public and international community.

Thus, elections have enormous potential to change the perceptions and strategies of key players. Authoritarian incumbents might learn that they can compete and win in elections and may liberalize to reap the benefits of being seen as more open. The opposite may also happen if authoritarians find relatively free elections too risky and clamp down to secure control. Security forces might learn of incumbent weakness and decide that the costs of repressing the opposition are just not worth it if the regime is unstable anyway. Alternatively, they may be impressed with the strength and resolve of the incumbent rulers and decide it is worthwhile to take risks to defend the regime. Either way, these decisions are crucially shaped by what people learn during the electoral process.

Elections, Information, and (De)Liberalization: Hypotheses

In this section, we develop specific testable hypotheses about the relationship between elections and (de)liberalization based on the theory outlined above. We develop two sets of hypotheses that describe how the existence of elections and the institutional and informational context in which they take place should affect the prospects of political (de)liberalization in a non-democratic state.

We have argued above that elections provide crucial tests of strength and popularity for incumbent authoritarian regimes. Consequently, we expect that political change should be strongly associated with the holding of elections.

> H1: Political liberalizations and deliberalizations should be more frequent following elections.

However, not all elections should have the same effect. If elections do indeed work through the mechanism of information revelation, then it is reasonable to suppose that elections that reveal more information are more likely to be associated with either liberalization or deliberalization than less informative elections. This leads to a number of different hypotheses. Most obviously, elections without any competition whatsoever are unlikely to provide any new information.

> H2: Uncontested elections should not be associated with either liberalization or deliberalization.

The effects for contested elections, however, are more subtle. Since both the quantity and the quality of information revealed should be positively associated with the quality of elections, we might expect that the cleaner the elections the greater the chance of change. However, in developing our theory, we have argued that what matters is not just what elections communicate about the strength or weakness of the regime, but also the extent to which that information has not already been incorporated into political expectations. Thus, although fully free and fair elections should generate the most accurate political information, fully free and fair elections tend to occur in places with greater press freedom and well-respected political and civil rights and so more continuous political contestation. In this environment, the election period is less unusual than it is in more closed contexts. Therefore, we predict that elections that lie somewhere between completely fake and completely fair are the most likely to have bigger effects.[33] This hypothesis contrasts sharply with the existing literature that has argued that the likelihood of liberalization increases linearly with the quality of elections.[34]

> H3: Contested but flawed elections should have a stronger effect than fully free and fair elections on the probability of liberalization and deliberalization.

In the second set of hypotheses we consider the interaction between elections and the information environment in which they take place. Identifying and measuring the vulnerability of different systems to "information shocks" is difficult since "shocks" are always relative to some starting set of expectations that are extremely difficult to capture systematically in a cross-national context. Recognizing these empirical challenges in capturing initial expectations, we adopt a different strategy. Rather than measuring expectations directly, we consider a range of typical conditions that are likely to make different authoritarian regimes more vulnerable to information shocks. While not an exhaustive list, we outline some of the most typical conditions under which elections are more likely to lead to political change.

First, we should expect elections to be more consequential when either information on the relative strength of different political players was harder to find before the elections—that is, when the information environment is worse—or when the elections themselves lead to specific moments of information revelation. If this is true, then, quite counter-intuitively, we should expect a negative interaction effect between elections

and measures of the quality of the pre-electoral information environment in driving political change. In other words, given a relatively competitive election, incumbents should be more vulnerable to crisis in places where information was more restricted before. Hence, in contrast to Schedler,[35] who argues that greater media freedom is bad for incumbents, our theory predicts that within a certain range it is limitations on media freedom that can actually hurt incumbents. However, given that at least the minimal amount of information freedom may be necessary for election-related information to reach the relevant political actors, we would expect this relationship to be non-monotonic: as in the case of election quality, we predict intermediate levels of information freedom to be the more conducive to informational surprises, and hence to episodes of rapid political change, than either completely closed or completely free information environments.

> H4: Political liberalizations and deliberalizations should be more frequent following elections in restrictive but not completely closed information environments.

Second, authoritarian leaders in the contemporary world are increasingly under pressure to allow international observers to monitor their elections.[36] This often pushes dictators into a series of cat-and-mouse games with observers. Nevertheless, if it is true, as Hyde and Marinov argue,[37] that monitors improve the quality of information available in elections and that incumbents who are criticized by international observers are more likely to face either domestic protest or international punishment, then, on average, elections with monitors present should be more likely to lead to liberalization (if incumbents cheat and are successfully punished) or deliberalization (if incumbents cheat and crack down in the face of punishment). We would expect this vulnerability to be particularly high in the first internationally observed elections, where incumbents may be less practiced at hiding potential abuses from observers and where the likelihood of surprising information should be higher than in subsequent elections. Hence:

> H5: Political liberalizations and deliberalizations should be more frequent following elections that have international observers present and particularly in the first internationally observed elections.

Third, just as election observation has become more common, so has the presence of widely available public opinion surveys that can shape expectations around elections. The existence of such surveys varies considerably across regimes (in our sample, 55.7 percent of elections had surveys that were openly published). Even where surveys do exist, however, not all surveys are likely to be associated with new information and challenges to incumbents. Indeed, surveys that show a strong incumbent lead are likely to suggest that nothing will change. On the other hand, surveys that indicate support for the opposition to be high are more likely to be associated with change, in either direction (depending on how the incumbents decide to respond to such an opposition challenge). Hence:

> H6: Political liberalizations and deliberalizations should be more frequent following elections that have publicly available surveys showing support for the opposition.

It is important to note the parallelism of our expectations with regard to (de)liberalization. If the logic of our argument is correct, and elections can generate or reveal information that leads to a decision point for the regime and for pivotal actors in the country, the outcome could be liberalization, but it could also be retrenchment. Incumbents may accept defeat and stand aside (Romania 1996, Slovakia 1998), or they may falsify the results and clamp down on opponents (Iran 2009), or perhaps cancel elections and impose authoritarian rule (Algeria 1992). Consequently, despite the diametrically opposed outcomes, the logic of information revelation should be similar in the cases of liberalization and deliberalization. Elections that reveal more information are more likely to lead to a crisis or decision point and so will have a greater impact on the odds of deliberalization, just as they do for liberalization. Therefore, different types of elections and elections in different information environments will affect the probability of deliberalization and liberalization in the same way.

Testing the Theory: The Dependent Variable

We take "political liberalization" to mean a significant improvement in the quality or quantity of political rights exercised by citizens, improvements in the institutional framework that shapes political competition to make it more open or fair, and/or improvements in political practice that have the effect of making competition for the most important political posts in the state more transparent and impartial. In using this definition, our goal is to include a range of ways in which polities can experience a "political opening." By deliberalization we mean significant deterioration in these same dimensions.

Two caveats are in order. First, liberalization and democratization are emphatically not, as Linz and Stepan remind us,[38] the same thing, and it is possible, indeed common, to have liberalization without democratization (though the reverse is not possible). Closed authoritarian regimes can witness improvements in the extent of political competition in the system or in the permitted range of political action without coming close to democratization. The limited opening of a one-party state to real but constrained political competition, as in Tanzania in 2000, is an example. Second, neither liberalization nor deliberalization need be durable but may be followed by further changes in either direction. While the medium- to long-term effects of such changes are an interesting empirical question, they are beyond the scope of this article.

Operationalization and Patterns

In operationalizing the dependent variable, we follow Howard and Roessler,[39] who use a combination of an improvement in Freedom House (FH) political rights scores and a simultaneous improvement in the Polity IV regime score. We observe each country annually and define a (de)liberalizing moment as a simultaneous (or one-year-lagged)

improvement/decline of 1 point on the Freedom House political rights scale and a 2 point improvement/decline in the 21-point Polity scale (see Appendix A for details).[40]

This measure has a number of advantages. First, our dependent variable has three potential values: liberalization, deliberalization, and neither. We treat these outcomes as categorical rather than ordered in order to test whether the same variables can cause both liberalization and deliberalization. Second, it offers a clear and transparent coding rule to decide the frequently contentious question of whether a particular moment is liberalizing or not. Third, basing our coding on a combination of the Freedom House political rights scale and the Polity IV regime score rather than one of the many sub-categories offered by these and other rating agencies means that our sample is both inclusive, in the sense of being open to counting political change that arises from the broadest range of sources, and selective in that only changes that are significant are counted.

Using this broad definition of a liberalizing moment, we identified ninety-five cases of rapid liberalization taking place in countries with a population greater than 500,000 in the period from 1992–2008. Looking at simple cross-tabs, there seems to be considerable support for the view that liberalizing moments are associated with elections; seventy-six (80 percent) of these liberalizations occurred in election years, and forty-five (59 percent) of these were non-finishing touch liberalizations, i.e. instances where qualitative readings of the cases suggest elections played a significant causal role in driving liberalization. There were sixty-seven cases of deliberalization, twenty-eight (or 42 percent) of which took place after elections. Equally clearly, whatever the relationship between elections and (de)liberalization, most elections are not associated with either type of change. Even if all cases of simultaneous (de)liberalization and elections were causal, this leaves most elections (684 of 788, or 87 percent) without liberalizing or deliberalizing consequences.

Causal Inference Challenges

Our choice of (de)liberalization measures raises an important and difficult question about how to analyze the relationship between political regimes and elections. Specifically, one drawback of our broad measure is that both FH political rights and Polity regime scores include an assessment of the extent to which a country's government is chosen by free and fair elections. Consequently, holding free and fair elections (where there were none before) will trigger an improvement in both indices and lead to a classification of the case as a liberalizing moment, which introduces a potential bias in favor of finding that elections—and especially clean elections—matter. Similarly, we may risk introducing an opposite bias for deliberalizations. From this perspective, which we refer to as the accounting explanation, elections are simply a time when analysts update their evaluations of the extent of democracy in a given country. In other words, elections provide information not to political actors on the ground but to analysts at Freedom House, Polity, etc.

There are a number of potential responses to this problem. The first is to note that the predictions of the accounting view differ in important ways from our information-based

explanation. From an accounting perspective, we should find that the better the quality of an election, the greater the probability of triggering a political upgrade (i.e., liberalization). Conversely, the likelihood of deliberalizations should decrease as election quality improves. By contrast, our informational approach predicts that intermediate quality elections should matter most and that the effect should be the same on both liberalizations and deliberalizations. Thus, any biases in the coding process will work against our argument.

Second, our coding of the cases uses two separate sources: Freedom House and Polity IV. Since the accounting view essentially requires that regime coding specialists only "wake up" to the liberalizing/deliberalizing trends in a given country when these trends are reflected in an election, both of these organizations would have to simultaneously err in upgrading or downgrading a country based simply on the election for such observer bias to influence our data. Since Freedom House and Policy IV use different coding criteria (and often differ in their assessments), this is quite unlikely, and it is certainly lower than for most other cross-national analyses of political change, which tend to rely on a single source of regime scores.

One radical solution would to be to identify liberalizing moments based only on non-electoral measures such as FH civil liberties scores and Polity executive constraints. As we show in Table C4 in the appendix, our results hold when using this narrower measure. However, this alternative operationalization artificially narrows the scope of the cases we analyze, reducing their number and, worse, systematically missing cases in which the source of progress or decline is something other than changes in civil rights or executive constraints (such as the liberalization in South Africa in 1993–94 and the royal coup in Nepal in 2002). Therefore, we use the broader measure presented above for our main analysis.

However, we also took additional steps to insulate the results from electoral bias. First, while we used the quantitative rules to identify a baseline set of cases of liberalization and deliberalization, we then conducted a qualitative analysis to determine on a case-by-case basis whether the relationship between elections and change was clearly an artifact of some fundamentally non-electoral process. For liberalizations, this would be cases in which liberalization results in, rather than results from, elections. An example of such a "finishing touch" election is Mozambique in 1994 when elections resulted from a peace deal ending the civil war. For deliberalizations, these are cases in which a coup or some other deliberalizing event precedes new and less fair elections. The elections in Azerbaijan in 1993 are a good example. In this case, the elected President Elchibey was stripped of power and replaced by Heydar Aliyev, who then held new presidential elections and won 98 percent of the vote. We excluded such cases from our statistical analysis, making it harder for us to find an electoral effect.[41]

A second and distinct causal inference concern is about the potential endogeneity of several of the election and information environment indicators highlighted by our hypotheses. To state the problem succinctly: one can easily argue that the timing and quality of elections, as well as key features of the electoral information environment—such as press freedom, election surveys, or the presence of international observers—are endogenous to some of the same factors that drive political

liberalizations and deliberalizations. While future work could focus on experiments designed to manipulate the information environment in authoritarian elections, for the purpose of our current analysis, the more practical solution would be an instrumental variable (IV) approach. The credibility of these tests hinges on finding instruments that simultaneously strongly predict the endogenous variable(s) and satisfy the exclusion restriction (of not affecting the DV through any other channels). While we have identified a credible instrument for election timing, for other variables it is much more difficult to identify similarly powerful instruments. Given these limitations and space constraints, we focus our present discussion on non-IV results and present both IV regression results and Nearest Neighbor Matching Estimation in the appendix (Table C3 and Figure C1).[42]

While these endogeneity concerns obviously mean that we need to be cautious about interpreting our statistical results causally, the nature of our findings provides some reassuring evidence. First, to the extent that our endogenous variables simply capture other features of the political system that drive political (de)liberalization, we should not expect these variables to have the symmetric effects on both liberalizations and deliberalizations that we find. For example, if we worry that the publication of a public opinion survey that reveals incumbent weakness would not be allowed by strong authoritarian leaders, we might expect such surveys to trigger liberalizations, but we should not expect a similar effect on deliberalizations. Second, it is possible that the effects of elections might be primarily driven by unscheduled elections that are a symptom of a deeper political crisis that could drive both liberalization and deliberalization. However, below we differentiate between regularly scheduled and unscheduled elections and show that scheduled elections also have the effects we predict.

Key Independent Variables Two key variables in testing our explanation are the presence and quality of elections. The presence of elections is a simple dichotomous indicator that captures whether a country had a national-level parliamentary or presidential election in a particular year. Furthermore, as noted above, we created a dichotomous indicator of whether an election was constitutionally scheduled in a given country-year.[43] Based on these two variables we then created three dummy variables indicating whether a given country-year featured (1) a regularly held scheduled election, (2) an unscheduled election, or (3) a scheduled but canceled election.

To code election type, we used election observer reports, newspaper reports, and the coding in Lindberg[44] to code all national-level parliamentary and presidential elections in our dataset. We coded elections in four categories: (1) elections with no competition, (2) elections with limited competition and/or heavily falsified results, (3) elections with competition and significant irregularities but with results that were generally seen as acceptable, and (4) elections that were essentially free, fair, and clean.

To capture the nature of the information environment, we first created a press freedom index, which combines data from Freedom House Freedom of the Press ratings and the CIRI Freedom of Speech and Press indicator.[45] We then created three dummy variables (to capture severely restricted, partially restricted, and reasonably

free mass media environments)[46] and interacted them with the election-year indicator to test the impact of elections in different information environments.

For the international election observer variable, we collected data from three sources: the NELDA dataset,[47] Kelley and Kolev,[48] and our own coding of international observer missions. Given that the three sources did not always agree in their assessment, we coded an election as having been internationally observed if at least two of the three sources classified it as such. Based on this indicator we then created a set of three dummy variables to indicate whether an election was (1) unobserved, (2) observed for the first time, or (3) repeatedly observed. To test Hypothesis 6, we used data from the NELDA dataset to code whether any given election had (1) no pre-electoral survey, (2) an anti-incumbent pre-electoral survey, or (3) a pro-incumbent pre-electoral survey.

Alternative Explanations

In addition to the role of elections, there are a number of other elements that are likely to affect the probability of (de)liberalization. Prior literature stresses the importance of economic development in driving both democratization and democratic survival[49] and the impact of economic crisis on the stability of authoritarianism.[50] Therefore, our baseline statistical models include lagged measures of GDP/capita and urbanization, as well as economic growth.

Our regressions also control for a number of other well-established correlates of democracy and democratization, which are also lagged to avoid the risk of reverse causality. Given earlier findings about the impact of inequality on democratization,[51] we included the Gini coefficient for income as an indicator of economic inequality. We use a measure of resource rents to test the potential impact of a "natural resource curse" on the likelihood of democratization.[52] To test the effect of an extensive and well-resourced coercive apparatus on the stability of incumbent regimes, we control for the size of the military per capita. Finally, given that several studies have linked ethnic heterogeneity to political instability,[53] we control for the degree of ethnic fragmentation.

Another set of competing explanations that needs to be taken into account is neighborhood effects in which the political environment of neighboring states has an independent effect on the likelihood of (de)liberalization in a given state.[54] Consequently, we control for the average regional Freedom House political rights score in the preceding year for a given country. Since larger countries are less sensitive to outside leverage,[55] we also control for population size.

Finally, we control for several different regime type indicators in the preceding year. At the most basic level, the regressions include previous levels of democracy, to test whether the likelihood of political change is affected by prior political openness and to capture potential ceiling/floor effects. Moreover, given that earlier studies have found that the stability of non-democratic regimes depends to a significant extent on their institutional and political make-up,[56] we included indicators for military regimes,

monarchies, and party-authoritarian regimes based on Hadenius and Teorell's classification scheme.[57]

Results

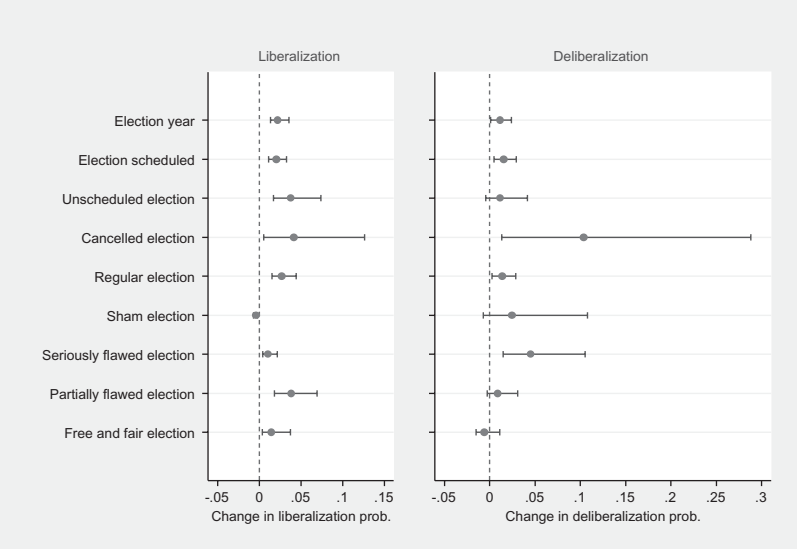
Since our dependent variable is trichotomous (liberalization, deliberalization, or no change), we use multinomial logistic regressions with robust standard errors clustered by country.[58] Given that the regression coefficients are not readily interpretable in substantive terms (especially in the case of interaction effects), we present our key results in graphical terms and refer the reader to the appendix (Table C1) for the full regression results. In the figures below we illustrate the impact of different types of elections on the probability of political liberalizations and deliberalizations and the role played by information in these processes.[59]

Elections and Political Change As a first step, we test the political impact of simply holding an election. The results are strongly supportive of the importance of elections. In line with Hypothesis 1, elections have a highly significant positive effect on both political liberalizations and deliberalizations. To put the substantive importance of these (and subsequent) effects in perspective, we should note that in the absence of elections, the probability of a liberalization or a deliberalization in any given country-year is .9 percent and 2 percent respectively. This means that the predicted 2.2 percent increase in the probability of liberalization in elections years corresponds to a 237 percent increase in the risk of political change, while in the case of deliberalizations the 1.1 percent increase in the effect amounts to a 57 percent rise compared to the baseline.

To address concerns about the endogeneity of election timing, we tested the effect of having an election scheduled in a given year (irrespective of whether an election was actually held). The effects are positive for both liberalizations and deliberalizations and very similar in terms of size and statistical significance to the effects of actual elections. This suggests that our election effects are not simply driven by irregular elections for which endogeneity concerns are much greater.

To probe this issue more deeply, we then analyzed the effects of different configurations of scheduled vs. held elections. The results in Figure 1 suggest that while deviations from normal election schedules trigger political changes that are consistent with a selection model—unscheduled elections are more likely to trigger liberalizations and cancelled elections are more strongly associated with deliberalizations—we find that cancelled elections are also significant drivers of liberalizations, which is at odds with a simple selection-based story. Even more importantly for our purposes, Figure 1 shows that even regularly held elections were associated with statistically significant increases in the probability of both political liberalizations and deliberalizations. In other words, even when the election timing is not surprising, elections provide opportunities for significant political change in either direction.

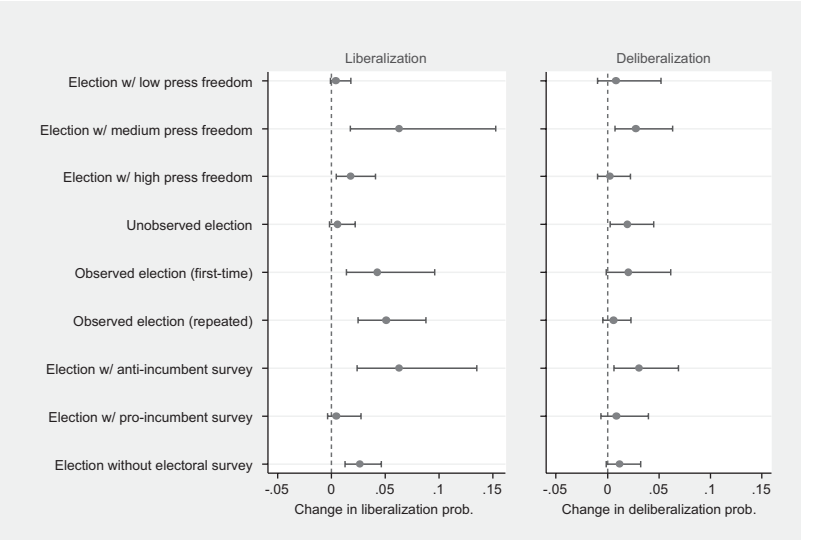
Figure 1 Elections and Political Liberalization/Deliberalization



The final set of results in Figure 1 confirms that the political effects of elections are a function of the quality of elections. In line with Hypothesis 2, we find that sham elections are not associated with statistically significant political changes in either direction. While this result is not particularly surprising in the case of liberalizations, the null effect vis-à-vis deliberalizations is at odds with the predictions of the accounting explanation, but it is consistent with an information-based explanation that focuses on the low probability of information revelation in completely sham elections. We also found support for Hypothesis 3: fully free elections had no effect on deliberalizations, whereas seriously flawed elections, and to a somewhat lesser extent partially flawed elections, significantly increased the probability of deliberalizations.[60] Even though free and fair elections had a significant positive effect on liberalizations, these effects were smaller than for partially flawed elections[61] and similar to those of seriously flawed elections. Overall, we find that the most consistent drivers of political change are the two intermediate types of elections, which are likely to reveal information that is, on the one hand, more accurate than in the carefully choreographed sham elections but, on the other hand, more likely to provide politically destabilizing informational surprises than in the case of fully free and fair elections.

Elections and Information Environments We have also argued that other aspects of the information environment should shape the capacity of pivotal elites and regular citizens to gather accurate information about the relative strength of different political actors, and thus to shape their beliefs about the feasibility of electoral challenges against

Figure 2 Information and Political Liberalization/Deliberalization



the status quo. The first set of findings in Figure 2 confirms Hypothesis 4 that the likelihood of liberalizations and deliberalizations is both substantively and statistically higher during elections with partially restricted information environments. By contrast, elections in countries with the most restrictive media environments have small and statistically insignificant effects for either type of political change, while elections in reasonably free media settings have no effect on deliberalizations and a statistically significant but substantively modest effect on liberalization.[62]

Turning to the role of election observers, the results in Figure 2 are broadly in line with the predictions of Hypothesis 5. As expected, the most consistent political changes occurred during elections that were observed for the first time by international observers, which were associated with significantly higher risks of both liberalization and deliberalization. While the liberalization effect is also compatible with a selection-based mechanism, the greater risk of deliberalizations can only be explained in the context of our information-based argument and highlights the double-edged nature of informational surprises that can be triggered by the first-time presence of international observers. Meanwhile, the effects of unobserved and continuously observed elections, where we would expect fewer informational surprises, are more inconsistent and are also compatible with a selection-based account.

Turning to the comparison of elections with and without pre-electoral surveys, we find that in line with Hypothesis 6, both liberalization and deliberalizations are more likely in elections where surveys show support for the opposition. Where a survey shows support for the opposition, the likelihood of liberalization is more than twice

as high as where there are no surveys (6.3 percent versus 2.7 percent), and the likelihood of deliberalization is almost three times as high (3.0 percent versus 1.2 percent). Where surveys show support for the incumbents, the probabilities of either change are substantively tiny and statistically no different from zero. Once again the parallelism of the results is fascinating. Clearly, elections are not merely ratifying underlying distributions of opinion. Moreover, the fact that we see no effect for either liberalization or deliberalization in elections where surveys favor the incumbent suggests we are not simply seeing an accounting effect where popular incumbents are rewarded by analysts for running more open elections. Instead it is clear that pro-opposition information that emerges during the campaign is associated with destabilization, while pro-regime information makes change less likely.

Conclusion

In this article we have developed a theoretical framework to explain why elections often play a key role in both political liberalizations and authoritarian retrenchment. In doing so, we have made both theoretical and empirical contributions to the study of political dynamics in contemporary authoritarian regimes.

First, our analysis demonstrates the importance of looking at both political deliberalizations and liberalizations in a common framework. Doing so allows us to identify those factors that make both liberalization and deliberalization more (or less) likely and those that promote one and not the other. This common framework approach has been used successfully by others in the context of the development/democracy link,[63] but it is not the approach generally used in the literature on regime change, which still tends to discuss the causes of deliberalizing events like coups and liberalizing events such as Colored Revolutions separately. Our study suggests that a common framework is a useful way to look at the effects of a range of variables, including but not limited to those we focus on here.

In bringing together both liberalizations and deliberalizations, we have advanced the substantive debate on the role of elections in political change by showing that elections in authoritarian regimes do not only promote democratization, but can have a knife-edge quality. We show that while elections usually do not upset the political apple-cart, when they do, the results can be positive from the perspective of liberalization, as in Ukraine in 2004, or negative, as in Iran in 2009.

We also presented evidence that a key mechanism linking the elections causally to political liberalization and deliberalization is the information that is generated in the course of elections. Separating causal effects in cross-national studies is extremely difficult, and, as we discussed, issues of endogeneity are largely unavoidable in this context. Nevertheless, we have demonstrated a number of relationships that cannot be easily explained by a conventional story in which good things (elections, high quality elections, free information, election observation effort, and pro-opposition surveys) are all related positively to liberalization and negatively to deliberalization. Instead, we have

shown that elections, even if they are just regularly scheduled, increase the probability of a country experiencing either liberalization or deliberalization, and that election quality effects and the effect of the quality of the information environment are not linear but curvilinear. We have also shown that election observers monitoring elections for the first time increase not only the probability of liberalization but also of deliberalization and that pre-electoral surveys showing strong opposition support also increase the probability of liberalization and deliberalization symmetrically. All of these findings only make sense if we understand the relationship between authoritarian elections and political change as operating in large part through the generation and dissemination of information about regime strength and weakness in the course of elections.

NOTES

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30. DeNardo.

31. Valerie Bunce and Sharon Wolchik, "Defeating Dictators: Electoral Change and Stability in Competitive Authoritarian Regimes," in Lindberg, ed.

32. Ibid.

33. Based on the election typology we describe below, roughly half (50.4 percent) of the elections from 1992–2004 fall into the two intermediate categories, which we would expect to be the most likely opportunities for electorally induced political regime change.

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40. Due to space constraints, the Appendix is not in the print version of this article. It can be viewed in the online version, at www.ingentaconnect.com/cuny/cp.

41. In doing this we consulted a range of sources including the African Elections Database, Economist Intelligence Unit Country Profiles, and newspaper reports from Lexis-Nexis.

42. See also Table C3 in the appendix for results excluding long-standing democracies and countries without elections and Table C4 in the appendix for similar results using different (de)liberalization thresholds.

43. In doing so we only counted elections as constitutionally scheduled if they had been set at least a year prior to the actual election. This fairly conservative approach allows us to exclude short-term scheduled elections where we have the greatest reasons to be concerned about reverse causality.

44. Lindberg, 2006.

45. The standardized index had an alpha reliability coefficient of .69.

46. See Table B1 in the appendix for additional details about coding decisions.

47. Hyde and Marinov, 2012.

48. Judith Kelley and Kiril Kolev, "Election Quality and International Observation 1975–2004: Two New Datasets," 2010, available at SSRN: http://ssrn.com/abstract=1694654.

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52. Michael Moss, "The political Economy of the Resource Curse," *World Politics*, 51 (January 1999), 297–322. Schedler, 2009.

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56. Barbara Geddes, "Authoritarian Breakdown: Empirical Test of a Game Theoretic Argument," Annual Meeting of the American Political Science Association, Atlanta, Georgia, 1999; Axel Hadenius and Jan Teorell, "Pathways from Authoritarianism," *Journal of Democracy*, 18(January 2007), 143–57; Gandhi, 2008.

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58. The role of elections is strongly confirmed when using fixed effects (see Table C2 in the appendix).

59. The coefficients and standard errors were calculated using Clarify in Stata 13.

60. The latter effect was only marginally significant at .1 one-tailed.

61. The difference in coefficients was marginally significant at .07 two-tailed.

62. The difference between the election impact in moderate vs. high media freedom environments is marginally significant at .08 one-tailed.

63. Boix and Stokes; Adam Przeworski, Michael Alvarez, Jose Cheibub, and Fernando Limongi. *Democracy and Development: Political Institutions and Well-Being in the World, 1950–1990* (Cambridge Studies in the Theory of Democracy, 2000).

Appendix

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Appendix A Operationalizing Liberalizing and Deliberalizing Moments

Howard and Roessler's (2006) definition of liberalization as a simultaneous three-point improvement on the 21-point Polity scale and a 1-point improvement in Freedom House political rights represents a useful starting point for a systematic analysis of liberalizing moments. Nonetheless, a closer look at the results produced by these coding criteria suggests two types of limitations of this approach to identifying (de)liberalizing moments. For the sake of simplicity we discuss the relevant issues here in terms of liberalization, but since we use symmetrical measures, all the same arguments apply to deliberalizations as well.

The first limitation is of a primarily technical nature and can be illustrated by discussing a few of the more prominent cases, which would arguably be misclassified by the Howard-Roessler coding scheme. One such case is the Georgian Rose Revolution of 2004, which would not qualify as a liberalizing moment under the "Polity3&FH1" rule because its two-point Polity score improvement fell short of the three-point threshold. Howard and Roessler (2006, 369) argue that a 1-point change on the 7-point FH scale is mathematically equivalent to a 3-point change on the 21-point Polity scale. However, these changes are not necessarily theoretical equivalents, both because of the extensive disagreements between the two sources and because 1-point FH improvements are almost three times more frequent than 3-point Polity improvements for the countries in our sample. Since there is no particular theoretical reason for using a 3 rather than a 2-point cutoff, we have decided to use a "Polity2&FH1" version of the variable, which slightly expands the universe of "liberalizing moments."

However, even with this lower threshold we would miss several prominent liberalization episodes, including the end of Apartheid in South Africa in 1993–94 and the political opening in Slovakia following the electoral defeat of Vladimir Meciar's HZDS in the September 1998 elections. In both cases the problem stems from the fact that the 2-point Polity score improvement occurs in the year preceding the FH-improvement. Since this discrepancy is due to different approaches to coding changes, not to disagreements about the nature of the liberalizing event, our variable version also captures such episodes with artificial coding lags. Thus, for example, Polity indicates a specific date for a regime score change (10/30/1998 for Slovakia) while Freedom House updates scores once a year and attempts to capture civil and political freedoms in a given year, which means that political changes occurring late in the year (as in Slovakia) are only reflected in the scores for the following year. We decided to assign such lagged liberalizing moments to the first year in which one of the two sources (usually Polity) recorded a change. Moreover, we made sure that this approach did not result in the artificial proliferation of consecutive liberalizing moments (e.g. in situations with two consecutive one-year FH improvements and a Polity improvement in only one of those years).

In a few cases where both sources noted a 1-point change or where one of the sources noted a large change (at least 2 points on FH or 3 points on Polity) while the other source did not, we used changes in two additional sources (Coppedge and Alvarez, 2008 and Cingranelli and Richards, 2009) to decide whether the case constitutes a (de)liberalizing moment.

Appendix B

Table B1 Statistical Measures and Sources

	Measure	Source
Liberalizing/ Deliberalizing Moment	Change of 2 points on Polity 2 and 1 point on Freedom House Political Rights associated with the same political process. Where both sources noted a 1-point improvement or where one of the sources noted a large improvement (at least 2 points on FH or 3 points on Polity) while the other source did not, we used changes in two additional sources (Coppedge, 2008 and Cingranelli and Richards, 2009) to decide whether the case constitutes a liberalizing moment.	Polity IV Project, Freedom House, Coppedge (2008), Cingranelli and Richards (2009)
Election Year and Election Schedule	Dummy for National Executive and Legislative Election	Lindberg (2006) Interparliamentary Union www.ipu.org; www.electionguide.org; Binghamton Election Results Archive http://www.binghamton.edu/cdp/era/index.html; OSCE www.osce.org; African Elections Database www.africanelections.tripod.com Economist Intelligence Unit Country Profiles www.eiu.org Lexis-Nexis Academic
Election Quality	Sham election: Elections without any competition, including single party or uncontested elections. Seriously flawed elections: Limited or highly unfair competition rendering results meaningless. Somewhat flawed elections: Elections with significant violations of international electoral norms but that nevertheless produce a competitive result. Free and fair elections: Elections that conform to international democratic standards	Using the four category scheme, codings were created consulting executive summaries of election monitoring reports of the OSCE, European Union, Council of Europe, Carter Center, National Democratic Institute, Organization of American States and African Union, Lindberg (2006). Where none of these sources were available codings were made on the basis of searching: Interparliamentary Union www.ipu,.org; www.electionguide.org; African Elections Database www.africanelections.tripod.com Economist Intelligence Unit Country Profiles www.eiu.org Lexis-Nexis Academic
Finishing Touch Elections	Case excluded if elections complete (de)liberalization but play no causal role or elections follow rather than precede (de)liberalization.	Lexis-Nexis Academic Economist Intelligence Unit Country Profiles www.eiu.org
Inflation	Log of Consumer Price Inflation	World Development Indicators
GDP Change	Change in GDP (%)	World Development Indicators
GDP/capita	Gross Domestic Product in constant (1995) US dollars per capita	World Development Indicators
Population	Log of population	World Development Indicators
Urbanization	Urban population as % of total population	World Development Indicators

Continued on next page

Table B1 Continued

	Measure	Source
Natural resource rents	Log of combined dependence on energy, metals and minerals per capita	World Development Indicators
Income inequality	Gini coefficient of income inequality	Babones (2008)
Ethnic fractionalization	Index of ethno-linguistic fractionalization	Fearon (2003)
Regional democracy	Regional Average of Freedom House Democracy Scores.	Freedom House
Urbanization	Percentage of Population living in urban areas	World Development Indicators
Armed forces p.c.	Size of armed forces as a percentage of the population.	SIPRI
Regime Type	Military regimes, monarchies, party authoritarian and democratic regimes.	Hadenius and Teorell (2007)
Regime Duration	Log of duration of current regime	Hadenius and Teorell (2007)
Press Freedom	Index of Freedom House Freedom of the Press ratings and the CIRI Freedom of Speech and Press indicator.	Freedom House, Cingranelli and Richards (2009)
Election observers	Three dummy variables to indicate whether an election was (1) unobserved, (2) observed for the first time or (3) repeatedly observed	Hyde and Marinov (2012), Kelley and Kolev(2010) and authors coding of election observer reports
Pre-election surveys	Three dummy variables to indicate whether an election had (1) no pre-electoral survey (2) an anti-incumbent pre-electoral survey or (3) a pro-incumbent pre-electoral survey	Hyde and Marinov (2012)

Appendix C Regression Results and Robustness Tests

In this section we first present the full regression results for the models underlying the simulations in Figures 1 and 2 and then present a number of robustness tests.

As discussed in the main article, the main regression results in Table C1 are multinomial logistic regression models without country fixed effects. We also ran the same set of models using multinomial probit regressions and obtained very similar results but chose to present the multinomial logit models because the Clarify package, which we used to create the graphs, does not support the mprobit command in Stata.

Next, in Table C2 we reran all the models from Table C1 using country fixed effects to address the possibility that our findings could be driven by unobservable factors at the country level. Doing so results in significantly smaller sample sizes (because all the countries that do not experience any deliberalizations/liberalizations are automatically dropped from the models), but our key findings hold up quite well. The most important differences are with respect to election quality (which is not surprising given that this is one of the main areas where we would expect cross-country variation to be more important than cross temporal variation). Thus, unlike for the random effects models, we now find a negative and significant effect of sham elections on liberalizations (which is, however, in line with our theoretical expectations), and the effect of somewhat flawed elections on deliberalizations is positive but no longer even marginally statistically significant. However, it should be noted that the other results, including the significant positive effects of seriously flawed elections on both liberalizations and deliberalizations and the positive significant effect of somewhat flawed elections on liberalizations, still hold.

In Tables C3 and C4 we present a number of additional robustness tests discussed in the main article. In each table, model 1 reproduces the findings of model 1 in Table C1 as a baseline against which to compare the robustness tests. As a first step, in model 2 of Table C3 we exclude advanced industrial democracies, which have no further liberalization opportunities (due to ceiling effects) and which are much less prone to deliberalizations (Svolik, 2008). This resulted in the loss of about 370 observations but barely affected the results. Similarly, in model 3 we excluded countries (such as Saudi Arabia or Libya), which during the time period of our analysis did not have provisions for elections, in order to test whether the election-liberalization link is due to the fact that the world's worst regimes do not even bother to hold elections. The results were once again unchanged. In model 4 we included the finishing touch liberalizations and deliberalizations, which are excluded in our main models, to make sure that our results are not influenced by potentially arbitrary coding choices. As expected, putting finishing touch elections in strengthens the results—the coefficients for the election indicator are slightly larger (since we include previously excluded instances where liberalizations and deliberalizations happened in election years)—but the differences are not particularly large in substantive terms.

In models 5 and 6 we address the potential endogeneity of election timing through an instrumental variable regression approach. To do so, we used the scheduled election

Table C1 Main Regression Results

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6		Model 7	
	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib
Election year	.758*	2.234**							.138	2.047**				
	(.317)	(.303)							(.552)	(.655)				
Election scheduled			.944**	1.693**										
			(.312)	(.274)										
Unscheduled election					.648	2.337**								
					(.556)	(.372)								
Cancelled election					2.238**	2.226**								
					(.763)	(.751)								
Regular election					.886*	2.325**								
					(.345)	(.354)								
Sham election							.884	−12.644**						
							(.820)	(.498)						
Seriously flawed election							1.681**	1.931**						
							(.453)	(.336)						
Somewhat flawed election							.579#	3.231**						
							(.420)	(.437)						
Free & fair election							−.843	2.301**						
							(.806)	(.636)						
Election* Low press freedom									.104	−1.293				
									(1.014)	(.886)				
Election* Medium press freedom									1.188#	1.265				
									(.697)	(.876)				
Low press freedom									.442	−.111				
									(.707)	(.813)				
Medium press freedom									.328	−1.112				
									(.503)	(.913)				
Unobserved election											1.044*	.785		
											(.419)	(.580)		
Observed election (first time)											.989#	2.394**		
											(.525)	(.430)		

Continued on next page

Table C1 Continued

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6		Model 7	
	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib
Observed election (repeated)											.376	2.753**		
											(.437)	(.332)		
Election w/ pro-incumbent survey													.457	.375
													(.542)	(.753)
Election w/ anti-incumbent survey													1.388**	2.797**
													(.466)	(.512)
Election w/o pre-elect survey													.753#	1.997**
													(.412)	(.352)
Inflation (t-1)	.015	−.132	.011	−.182	−.020	−.137	.047	−.149	.043	−.149	−.005	−.072	.013	−.156
	(.209)	(.197)	(.219)	(.217)	(.213)	(.194)	(.206)	(.205)	(.202)	(.191)	(.206)	(.203)	(.206)	(.205)
GDP chg (t-1)	−.031	−.041*	−.033	−.040#	−.032	−.041*	−.037#	−.041*	−.035#	−.039*	−.031	−.039*	−.030	−.035*
	(.020)	(.018)	(.021)	(.021)	(.021)	(.018)	(.020)	(.020)	(.021)	(.020)	(.020)	(.019)	(.020)	(.017)
GDP/capita (t-1)	−.859**	−.398	−.815**	−.349	−.848**	−.377	−.884**	−.486	−.870**	−.345	−.902**	−.304	−.884**	−.492
	(.272)	(.370)	(.276)	(.351)	(.279)	(.368)	(.273)	(.387)	(.281)	(.346)	(.272)	(.377)	(.265)	(.362)
Population	.015	.084	.003	.020	.012	.080	.007	.095	−.004	.086	.013	.086	.005	.053
	(.105)	(.152)	(.106)	(.153)	(.106)	(.152)	(.109)	(.163)	(.104)	(.153)	(.103)	(.158)	(.110)	(.150)
Urbanization	.006	.014	.004	.014	.006	.014	.007	.014	.010	.013	.006	.014	.006	.017
	(.011)	(.013)	(.012)	(.012)	(.012)	(.013)	(.011)	(.013)	(.011)	(.013)	(.011)	(.013)	(.011)	(.013)
Ethnic fractionalization	−1.221#	−.187	−1.202#	−.066	−1.183#	−.148	−1.172#	−.536	−1.354*	.043	−1.241*	−.227	−1.257*	−.303
	(.624)	(.921)	(.634)	(.924)	(.630)	(.948)	(.605)	(.880)	(.658)	(.855)	(.620)	(.940)	(.615)	(.889)
Income inequality	−.003	.010	−.005	.013	−.005	.008	−.005	.012	−.004	.005	−.002	.008	−.002	.015
	(.017)	(.028)	(.017)	(.028)	(.018)	(.028)	(.017)	(.027)	(.018)	(.029)	(.017)	(.028)	(.017)	(.028)
Armed forces per cap	.175*	−.034	.165*	−.060	.160#	−.038	.173*	−.040	.170*	.003	.174*	−.047	.174*	−.040
	(.076)	(.077)	(.078)	(.085)	(.085)	(.076)	(.073)	(.093)	(.076)	(.079)	(.077)	(.077)	(.077)	(.075)
Natural resource rents	.220**	.003	.216*	−.007	.216**	.002	.226**	.023	.214*	−.006	.224**	−.008	.228**	.013
	(.082)	(.100)	(.084)	(.101)	(.083)	(.103)	(.081)	(.097)	(.085)	(.104)	(.082)	(.098)	(.081)	(.103)
Regional democracy	.158	−.144	.145	−.176	.158	−.134	.177	−.087	.141	−.074	.145	−.065	.173	−.021
	(.148)	(.196)	(.151)	(.186)	(.147)	(.194)	(.144)	(.203)	(.149)	(.180)	(.153)	(.217)	(.144)	(.193)
Military regime	−.610	.243	−.569	.493	−.661	.235	−.573	.117	−.642	.072	−.644	.140	−.596	.330
	(.752)	(.444)	(.747)	(.438)	(.782)	(.427)	(.668)	(.484)	(.746)	(.468)	(.746)	(.465)	(.756)	(.454)

Continued on next page

Table C1 Continued

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6		Model 7	
	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib
Monarchy	−.129	1.984**	−.120	1.821**	−.250	1.945**	−.382	2.263**	−.224	1.775*	−.091	1.983**	−.160	1.868*
	(.925)	(.733)	(.927)	(.695)	(.880)	(.711)	(.927)	(.783)	(.948)	(.773)	(.926)	(.714)	(.916)	(.746)
Party-authoritarian regime	.452	.226	.464	.226	.477	.262	.349	.049	.253	.152	.492	.073	.468	.238
	(.405)	(.381)	(.408)	(.392)	(.419)	(.377)	(.370)	(.403)	(.429)	(.390)	(.406)	(.371)	(.408)	(.396)
FH dem (t-1)	.152*	−.271**	.150*	−.252**	.156*	−.264**	.195**	−.372**	.189*	−.337**	.159*	−.300**	.148*	−.281**
	(.067)	(.074)	(.066)	(.068)	(.067)	(.070)	(.063)	(.079)	(.079)	(.112)	(.067)	(.073)	(.067)	(.075)
Regime duration	−.154	−.591**	−.191	−.638**	−.167	−.594**	−.129	−.496**	−.109	−.595**	−.158	−.556**	−.152	−.567**
	(.120)	(.143)	(.127)	(.139)	(.129)	(.144)	(.121)	(.141)	(.117)	(.142)	(.122)	(.151)	(.121)	(.140)
Year	.058	.124	.033	.027	.025	.117	.062	.160	.084	.124	.082	.072	.052	.078
	(.155)	(.174)	(.158)	(.171)	(.158)	(.178)	(.162)	(.201)	(.161)	(.175)	(.151)	(.189)	(.157)	(.174)
Year sq	−.007	−.007	−.006	−.003	−.005	−.007	−.007	−.010	−.009	−.008	−.008	−.005	−.007	−.005
	(.009)	(.009)	(.009)	(.009)	(.009)	(.009)	(.009)	(.010)	(.009)	(.008)	(.008)	(.009)	(.009)	(.009)
Reg_Africa	.631	.807	.628	.640	.499	.783	.436	.812	.713	.627	.720	.292	.506	.502
	(.885)	(.789)	(.899)	(.764)	(.904)	(.784)	(.883)	(.853)	(.915)	(.822)	(.901)	(.804)	(.862)	(.761)
Reg_Asia	.159	.430	.200	.612	.072	.381	.138	.369	.251	.110	.241	.134	.025	.209
	(.981)	(.797)	(.989)	(.811)	(.981)	(.784)	(.945)	(.862)	(.981)	(.878)	(.998)	(.816)	(.959)	(.785)
Reg_Eurasia	.518	.027	.511	.230	.459	−.008	.236	.431	.198	.170	.646	−.508	.432	.041
	(.915)	(.915)	(.925)	(.869)	(.918)	(.905)	(.938)	(.927)	(.913)	(.967)	(.944)	(.897)	(.913)	(.938)
Reg_Europe	−1.960	2.121	−1.891	2.316#	−2.035	1.986	−1.948	1.991	−1.819	1.693	−1.767	1.103	−2.193	1.429
	(1.453)	(1.360)	(1.466)	(1.302)	(1.455)	(1.336)	(1.422)	(1.372)	(1.460)	(1.304)	(1.501)	(1.463)	(1.435)	(1.373)
Reg_Americas	.178	1.850#	.203	1.688	.048	1.670	.092	1.685	.273	1.440	.356	.925	−.024	1.019
	(1.166)	(1.092)	(1.183)	(1.045)	(1.169)	(1.082)	(1.151)	(1.146)	(1.196)	(1.043)	(1.213)	(1.192)	(1.153)	(1.078)
Country f.e.	No		No		No		No		No		No		No	
N	2471		2472		2472		2472		2455		2471		2472	

Note: Logistic regression coefficients with standard errors in parentheses - # 10%; * 5%; ** 1% (one-tailed where appropriate).

Table C2 Fixed Effects Regression Results

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6		Model 7	
	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib
Election year	.739*	3.623**							.162	3.246**				
	(.324)	(.522)							(.618)	(.987)				
Election scheduled			1.065**	2.727**										
			(.334)	(.439)										
Unscheduled election					.545	3.795**								
					(.616)	(.737)								
Cancelled election					3.761*	3.491**								
					(1.480)	(1.097)								
Regular election					.928**	3.830**								
					(.355)	(.626)								
Sham election							1.215	−17.020**						
							(1.277)	(1.212)						
Seriously flawed election							1.659**	3.221**						
							(.623)	(.617)						
Somewhat flawed election							.268	4.495**						
							(.449)	(.780)						
Free & fair election							−.742	4.988**						
							(.869)	(1.081)						
Election* Low press freedom									−.155	−1.630				
									(1.142)	(1.397)				
Election* Medium press freedom									1.159	1.596				
									(.806)	(1.072)				
Low press freedom									.015	1.409				
									(1.093)	(1.163)				
Medium press freedom									.045	−.847				
									(.603)	(1.160)				
Unobserved election											1.037*	2.045*		
											(.486)	(.961)		

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Table C2 Continued

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6		Model 7	
	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib
Observed election (first time)											1.127#	3.964**		
											(.602)	(.843)		
Observed election (repeated)											.311	4.009**		
											(.481)	(.573)		
Election w/ pro-incumbent survey													.409	.288
													(.680)	(.996)
Election w/ anti-incumbent survey													1.229*	3.727**
													(.583)	(1.139)
Election w/o pre-elect survey													.704	3.479**
													(.461)	(.553)
Inflation (t-1)	.370	.765**	.370	.771**	.247	.775**	.402	.840**	.483	.667*	.332	.817**	.354	.837**
	(.341)	(.237)	(.367)	(.244)	(.358)	(.234)	(.326)	(.237)	(.359)	(.274)	(.343)	(.238)	(.350)	(.266)
GDP chg (t-1)	−.069*	−.053#	−.069*	−.041	−.077*	−.056#	−.076*	−.060#	−.065*	−.036	−.071*	−.059#	−.067*	−.024
	(.033)	(.032)	(.033)	(.030)	(.035)	(.032)	(.033)	(.034)	(.031)	(.038)	(.033)	(.034)	(.032)	(.028)
GDP/capita (t-1)	.552	−2.082	.486	−1.227	.304	−1.954	.749	−1.152	−.541	−1.104	.435	−1.854	.585	−1.485
	(1.341)	(2.207)	(1.323)	(1.941)	(1.379)	(2.289)	(1.473)	(2.057)	(1.807)	(2.293)	(1.338)	(2.061)	(1.316)	(2.282)
Population	−5.571	−13.315*	−5.743	−8.584#	−6.166	−14.344*	−5.334	−15.482**	−6.920	−1.641#	−6.138	−13.151*	−5.451	−8.864#
	(5.035)	(5.938)	(4.972)	(5.025)	(5.172)	(5.961)	(5.229)	(5.982)	(4.903)	(5.754)	(5.052)	(5.932)	(5.012)	(4.920)
Urbanization	−.073	−.071	−.051	−.054	−.065	−.105	−.091	−.083	−.085	−.099	−.059	−.082	−.071	−.099
	(.137)	(.171)	(.134)	(.156)	(.146)	(.165)	(.147)	(.154)	(.154)	(.176)	(.135)	(.175)	(.138)	(.156)
Ethnic fractionalization	−36.362	−111.1**	−37.857	−86.011**	−37.768	−114.75**	−33.806	−128.733**	−37.151	−105.23**	−39.868	−105.86**	−36.110	−87.801**
	(25.001)	(31.659)	(24.870)	(27.041)	(25.498)	(31.529)	(26.415)	(31.773)	(23.638)	(3.720)	(25.181)	(31.292)	(24.887)	(26.752)
Income inequality	−.006	.113	−.010	.127#	−.017	.092	−.004	.112	.002	.112	−.006	.138#	−.003	.131
	(.041)	(.079)	(.041)	(.071)	(.038)	(.064)	(.038)	(.087)	(.044)	(.080)	(.038)	(.081)	(.042)	(.082)
Armed_forces_per cap	.780#	1.162**	.811#	1.116**	.866*	1.192**	.669	1.317**	.684	1.324**	.785#	1.093*	.788#	1.320**
	(.470)	(.443)	(.471)	(.425)	(.435)	(.447)	(.493)	(.477)	(.454)	(.471)	(.468)	(.488)	(.471)	(.395)
Natural resource rents	.391	.694	.404	.618	.360	.674	.450	.538	.314	.798	.431	.671	.358	.719
	(.403)	(.609)	(.409)	(.656)	(.403)	(.594)	(.371)	(.564)	(.407)	(.618)	(.419)	(.589)	(.377)	(.596)
Regional democracy	.325	1.779*	.371	1.122#	.375	1.908*	.335	1.691*	.205	2.115**	.327	1.697*	.389	1.830*
	(.475)	(.752)	(.463)	(.641)	(.471)	(.787)	(.511)	(.790)	(.500)	(.804)	(.497)	(.795)	(.483)	(.733)

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Table C2 Continued

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6		Model 7	
	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib
Military regime	−.792	−.283	−.703	−.358	−.756	−.312	−.843	−.460	−1.010	−.259	−.824	−.077	−.712	−.346
	(1.103)	(1.354)	(1.028)	(1.146)	(1.026)	(1.431)	(1.081)	(1.302)	(1.011)	(1.265)	(1.023)	(1.295)	(1.073)	(1.242)
Monarchy	−16.19**	17.20**	−15.982**	15.06**	−18.340**	17.23**	−17.12**	17.85**	−16.0**	17.04**	−16.0**	16.24**	−16.218**	16.21**
	(1.547)	(1.444)	(1.515)	(1.509)	(1.838)	(1.508)	(1.473)	(1.561)	(1.594)	(1.411)	(1.523)	(1.536)	(1.559)	(1.530)
Party-authoritarian regime	.021	−.091	.071	−.370	.055	.061	.039	−.210	−.105	−.054	.026	.123	.047	−.706
	(.651)	(.698)	(.644)	(.680)	(.669)	(.735)	(.628)	(.741)	(.637)	(.679)	(.647)	(.769)	(.651)	(.783)
FH dem (t-1)	.485**	−1.003**	.483**	−.894**	.504**	−.991**	.498**	−1.105**	.420*	−.967**	.489**	−1.050**	.482**	−.959**
	(.181)	(.166)	(.178)	(.167)	(.189)	(.167)	(.177)	(.188)	(.168)	(.176)	(.181)	(.177)	(.180)	(.147)
Regime duration	.371	−.442	.327	−.699*	.320	−.492#	.391	−.508#	.477#	−.557#	.390#	−.466#	.372	−.478#
	(.226)	(.283)	(.227)	(.290)	(.227)	(.274)	(.239)	(.290)	(.266)	(.288)	(.229)	(.270)	(.230)	(.261)
Year	.439#	.422	.391#	.155	.381	.448	.473#	.464	.440	.157	.485*	.353	.405#	.183
	(.244)	(.296)	(.237)	(.251)	(.247)	(.298)	(.279)	(.293)	(.273)	(.323)	(.235)	(.324)	(.245)	(.269)
Year sq	−.019	−.008	−.016	.004	−.015	−.008	−.020	−.007	−.016	.002	−.020#	−.003	−.017	.001
	(.012)	(.014)	(.012)	(.013)	(.013)	(.014)	(.013)	(.014)	(.012)	(.015)	(.012)	(.014)	(.012)	(.014)
Country f.e.	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
N	1097		1098		1098		1098		1082		1097		1098	

Note: Logistic regression coefficients with standard errors in parentheses - # 10%; * 5%; ** 1% (one-tailed where appropriate).

Table C3 Robustness Tests (Sample Changes and Instrumental Variable Regressions)

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib
Election year	.758*	2.234**	.750*	2.224**	.743*	2.210**	.844**	2.777**	1.076**	1.929**	.453**	1.119**
	(.317)	(.303)	(.314)	(.302)	(.315)	(.305)	(.314)	(.286)	(.355)	(.312)	(.147)	(.167)
Inflation (t-1)	.015	−.132	.011	−.137	.005	−.138	.037	−.032	.026	−.154	.014	−.054
	(.209)	(.197)	(.213)	(.199)	(.207)	(.199)	(.205)	(.157)	(.220)	(.215)	(.095)	(.086)
GDP chg (t-1)	−.031	−.041*	−.031	−.041*	−.033#	−.041*	−.036#	−.025#	−.030	−.036#	−.015	−.020*
	(.020)	(.018)	(.020)	(.018)	(.020)	(.019)	(.022)	(.014)	(.021)	(.021)	(.009)	(.009)
GDP/capita (t-1)	−.859**	−.398	−.764**	−.354	−.840**	−.375	−.879**	−.601#	−.849**	−.409	−.379**	−.179
	(.272)	(.370)	(.273)	(.377)	(.265)	(.370)	(.270)	(.328)	(.274)	(.351)	(.115)	(.157)
Population	.015	.084	.041	.102	.029	.080	.021	.003	.008	.029	.006	.023
	(.105)	(.152)	(.105)	(.158)	(.107)	(.151)	(.104)	(.116)	(.106)	(.153)	(.049)	(.064)
Urbanization	.006	.014	.006	.014	.003	.013	.006	.011	.005	.016	.003	.006
	(.011)	(.013)	(.011)	(.013)	(.011)	(.013)	(.011)	(.011)	(.011)	(.012)	(.005)	(.006)
Ethnic fractionalization	−1.221#	−.187	−1.359*	−.224	−1.220*	−.141	−1.105#	.069	−1.201#	−.063	−.455#	.004
	(.624)	(.921)	(.639)	(.922)	(.617)	(.922)	(.620)	(.788)	(.634)	(.924)	(.273)	(.401)
Income inequality	−.003	.010	−.004	.010	−.003	.010	−.000	.014	−.004	.015	−.002	.004
	(.017)	(.028)	(.017)	(.028)	(.017)	(.028)	(.017)	(.023)	(.017)	(.028)	(.007)	(.013)
Armed_forces_per cap	.175*	−.034	.169*	−.034	.259**	−.018	.183*	−.024	.171*	−.049	.080*	−.015
	(.076)	(.077)	(.078)	(.077)	(.083)	(.083)	(.077)	(.072)	(.078)	(.085)	(.037)	(.034)
Natural resource rents	.220**	.003	.203*	−.009	.217**	.005	.216**	.087	.216*	−.006	.090*	.005
	(.082)	(.100)	(.083)	(.103)	(.081)	(.099)	(.081)	(.083)	(.084)	(.101)	(.036)	(.042)
Regional democracy	.158	−.144	.215	−.084	.171	−.145	.174	−.151	.163	−.143	.065	−.057
	(.148)	(.196)	(.147)	(.209)	(.151)	(.193)	(.149)	(.165)	(.152)	(.186)	(.064)	(.083)
Military regime	−.610	.243	−.554	.253	−.432	.258	−.680	.025	−.619	.404	−.291	.164
	(.752)	(.444)	(.741)	(.442)	(.684)	(.445)	(.766)	(.431)	(.743)	(.440)	(.286)	(.216)
Monarchy	−.129	1.984**	−.193	1.957**	−.128	2.006**	−.146	1.702*	−.087	1.879**	−.102	1.034**
	(.925)	(.733)	(.947)	(.742)	(.880)	(.740)	(.913)	(.738)	(.930)	(.696)	(.345)	(.338)
Party-authoritarian regime	.452	.226	.434	.201	.362	.190	.500	.183	.468	.233	.167	.094
	(.405)	(.381)	(.404)	(.378)	(.397)	(.385)	(.384)	(.319)	(.408)	(.393)	(.171)	(.179)
FH dem (t-1)	.152*	−.271**	.160*	−.266**	.155*	−.272**	.147*	−.227**	.151*	−.250**	.069*	−.131**
	(.067)	(.074)	(.068)	(.073)	(.066)	(.074)	(.064)	(.063)	(.066)	(.068)	(.028)	(.031)

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Table C3 Continued

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib
Regime duration	−.154	−.591**	−.110	−.578**	−.142	−.580**	−.143	−.582**	−.168	−.597**	−.074	−.286**
	(.120)	(.143)	(.119)	(.142)	(.121)	(.145)	(.118)	(.130)	(.124)	(.138)	(.051)	(.062)
Year	.058	.124	.051	.120	.065	.128	.054	−.042	.050	.057	.024	.041
	(.155)	(.174)	(.154)	(.173)	(.155)	(.172)	(.157)	(.143)	(.156)	(.172)	(.064)	(.077)
Year sq	−.007	−.007	−.007	−.007	−.007	−.008	−.007	.002	−.006	−.004	−.003	−.003
	(.009)	(.009)	(.008)	(.009)	(.008)	(.009)	(.009)	(.008)	(.009)	(.009)	(.003)	(.004)
Reg_Africa	.631	.807	.685	.766	.974	.838	.579	.705	.624	.632	.205	.392
	(.885)	(.789)	(.860)	(.776)	(.975)	(.795)	(.887)	(.706)	(.899)	(.764)	(.373)	(.367)
Reg_Asia	.159	.430	.050	.324	.364	.454	.077	.283	.144	.513	.049	.235
	(.981)	(.797)	(.974)	(.807)	(1.041)	(.792)	(.997)	(.757)	(.989)	(.814)	(.406)	(.370)
Reg_Eurasia	.518	.027	.520	.025	.885	.092	.376	−.467	.461	.139	.186	.086
	(.915)	(.915)	(.894)	(.908)	(1.018)	(.920)	(.928)	(.802)	(.927)	(.871)	(.386)	(.406)
Reg_Europe	−1.960	2.121	−2.173	1.807	−1.877	2.130	−2.037	1.629	−1.998	2.123	−.855	.954
	(1.453)	(1.360)	(1.397)	(1.354)	(1.498)	(1.339)	(1.439)	(1.111)	(1.470)	(1.304)	(.587)	(.589)
Reg_Americas	.178	1.850#	−.180	1.508	.441	1.880#	.178	2.181*	.156	1.605	.026	.858#
	(1.166)	(1.092)	(1.162)	(1.149)	(1.250)	(1.079)	(1.157)	(.935)	(1.185)	(1.046)	(.487)	(.487)
Cases	All (excl. FT)		Exclude consolidated democracies		Exclude countries without elections		All (incl. FT libs and delibs)		All (excl. FT)		All (excl. FT)	
IV regression	No		No		No		No		Yes (two-stage logit)		Yes (IV probit)	
N	2471		2098		2403		2502		2455		2410	2410

Note: Logistic regression coefficients with standard errors in parentheses - # 10%; * 5%; ** 1% (one-tailed where appropriate).

indicator (described in Table B1) as an instrument of elections. The justification for using scheduled elections as an instrumental variable is that by the 1990s most countries, regardless of their degree of democracy, had constitutionally set election intervals. This suggests that our instrument fulfills the exclusion restriction since simply scheduling elections does not signal that a particular regime is more or less likely to liberalize or deliberalize. (In our sample from 1992–2008 only a handful of countries had no provisions for holding regular elections, and excluding such countries does not affect our results). While countries sometimes either hold early elections or postpone elections, and such delays may be indicative of political crises, scheduled elections turned out to be an extremely powerful instrument for actual elections (correlated at .79). Since Stata does not offer a canned command for estimating multinomial IV regressions, we took two alternative approaches. In model 5 we ran a two-stage IV regression (with OLS in the first stage and multinomial logistic regressions in the second stage) and found that elections still result in statistically significant increases of both deliberalizations and liberalizations. While the size of the coefficients does not change significantly compared to the base model, it is worth noting that in the IV regression the election effects are slightly larger for deliberalizations and slightly smaller for liberalizations than in the non-IV baseline model. Since two-stage regressions that do not use OLS in the second stage models may produce biased estimates and standard errors, in model 6 we addressed this issue by running two separate maximum likelihood instrumental variable probit regressions for deliberalizations and liberalizations (using the ivprobit command in Stata 13). While the coefficients in model 6 are obviously not directly comparable to either the results in model 1 or model 5 (since we report probit rather than logit coefficients), the relative size of the coefficients for *election year* and their high statistical significance suggest that our results are robust even once we correct for endogeneity.

To show that our statistical findings are not simply an artifact of the coding criteria for our dependent variable in Table C4, we ran a series of tests using different coding criteria for our dependent variable. As a first step, we reran our model using Howard and Roessler's (2006) 3-point threshold for the Polity regime scale combined (as before) with a 1-point change in Freedom House political rights (model 2). This measure is close to the one we used in the main article: a 1-point change FH political rights and a 2-point Polity regime. Not surprisingly, we found very similar election effects as in model 1 for both liberalizations and deliberalizations. In models 3 and 4 we show what happens if we use only one of the two democracy scores for coding our dependent variable, which is a less demanding standard for coding political change. In model 3, we use only a 2-point change in the Polity regime score and still find large and statistically significant effects for both liberalizations and deliberalizations. It is worth noting that the relative size of the coefficients is less unequal than in the base model. Meanwhile in model 4, where political change is coded on the basis of a much less demanding 1-point change in Freedom House political rights, we still find positive and statistically significant election effects for both liberalizations and deliberalizations. Finally, as discussed in the paper, in model 5 we use a 1-point change in FH civil liberties and a 1-point

Table C4 Alternative Measures of Political Change

	Model 1		Model 2		Model 3		Model 4		Model 5	
DV coding rule	PolityReg2& FH-PR1 (orig)		PolityReg3& FH-PR1		PolityReg2		FH-PR1		PolityXC2&FH-CL1	
	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib
Election year	.758*	2.234**	.747*	2.410**	1.209**	1.609**	.352**	.798**	.637#	2.288**
	(.317)	(.303)	(.397)	(.368)	(.365)	(.302)	(.143)	(.153)	(.408)	(.418)
Inflation (t-1)	.015	−.132	−.009	−.587#	.181	−.115	−.032	−.172#	.330#	−.314
	(.209)	(.197)	(.301)	(.333)	(.230)	(.169)	(.091)	(.090)	(.187)	(.313)
GDP chg (t-1)	−.031	−.041*	−.047#	−.051#	−.022	−.024	−.023	−.040**	−.042*	−.040#
	(.020)	(.018)	(.027)	(.028)	(.028)	(.016)	(.014)	(.012)	(.021)	(.024)
GDP/capita (t-1)	−.859**	−.398	−.960*	−.196	−.807**	−.301	−.438**	−.011	−.561	−1.353*
	(.272)	(.370)	(.411)	(.521)	(.303)	(.316)	(.139)	(.120)	(.371)	(.671)
Population	.015	.084	−.113	.306#	.006	.073	−.014	.005	.014	.333
	(.105)	(.152)	(.130)	(.182)	(.120)	(.110)	(.064)	(.052)	(.136)	(.211)
Urbanization	.006	.014	.007	.005	.011	.005	−.001	.000	−.011	.044
	(.011)	(.013)	(.016)	(.023)	(.015)	(.013)	(.006)	(.005)	(.019)	(.033)
Ethnic fractionalization	−1.221#	−.187	−1.537#	−.843	.215	−.765	.158	.670#	−1.682#	−.608
	(.624)	(.921)	(.929)	(1.095)	(.874)	(.842)	(.344)	(.361)	(.874)	(1.390)
Income inequality	−.003	.010	.006	.043	.027	.018	.009	−.017	.036	.039
	(.017)	(.028)	(.030)	(.030)	(.026)	(.022)	(.009)	(.014)	(.035)	(.038)
Armed forces per cap	.175*	−.034	.154	−.037	.228*	−.004	.077	−.072#	.317**	−.255*
	(.076)	(.077)	(.099)	(.120)	(.094)	(.068)	(.053)	(.044)	(.075)	(.122)
Natural resource rents	.220**	.003	.203#	−.099	.203#	−.031	.106*	−.070#	.334**	−.112
	(.082)	(.100)	(.110)	(.108)	(.108)	(.092)	(.046)	(.040)	(.124)	(.188)
Regional democracy	.158	−.144	.159	−.364	.045	−.123	.009	−.133#	.155	−.090
	(.148)	(.196)	(.159)	(.286)	(.120)	(.154)	(.063)	(.077)	(.132)	(.291)
Military regime	−.610	.243	−.871	.429	.460	1.022**	−.037	−.285	.822	.670
	(.752)	(.444)	(1.057)	(.484)	(.868)	(.386)	(.257)	(.271)	(.776)	(.600)
Monarchy	−.129	1.984**	−13.600**	.794	−11.439**	1.285#	1.350**	.871*	−13.841**	2.931#
	(.925)	(.733)	(.965)	(1.619)	(1.154)	(.686)	(.396)	(.388)	(1.301)	(1.634)
Party-authoritarian regime	.452	.226	.778	.145	1.616**	.923**	.577**	−.027	1.145*	.200
	(.405)	(.381)	(.540)	(.417)	(.601)	(.310)	(.173)	(.190)	(.536)	(.461)
FH dem (t-1)	.152*	−.271**	.120	−.441**	.259*	−.174*	.163**	−.143**	.342**	−.568**
	(.067)	(.074)	(.098)	(.097)	(.106)	(.068)	(.036)	(.035)	(.099)	(.117)
Regime duration	−.154	−.591**	−.305#	−.688**	−.089	−.243#	−.144*	−.372**	−.271	−.376*
	(.120)	(.143)	(.175)	(.191)	(.149)	(.137)	(.069)	(.059)	(.188)	(.183)
Year	.058	.124	.156	−.027	−.050	.073	−.352**	−.041	−.408#	.341
	(.155)	(.174)	(.185)	(.186)	(.220)	(.165)	(.077)	(.067)	(.236)	(.212)
Year sq	−.007	−.007	−.013	.001	.002	−.006	.014**	.002	.018	−.020#
	(.009)	(.009)	(.010)	(.010)	(.011)	(.008)	(.004)	(.004)	(.012)	(.011)
Reg_Africa	.631	.807	.490	1.582	.661	.600	.160	.134	1.539	.104
	(.885)	(.789)	(1.241)	(1.262)	(1.230)	(.675)	(.369)	(.415)	(.964)	(1.367)
Reg_Asia	.159	.430	.977	.946	1.656	1.049	−.159	.154	1.139	−.113
	(.981)	(.797)	(1.299)	(1.095)	(1.293)	(.709)	(.468)	(.389)	(1.171)	(1.437)

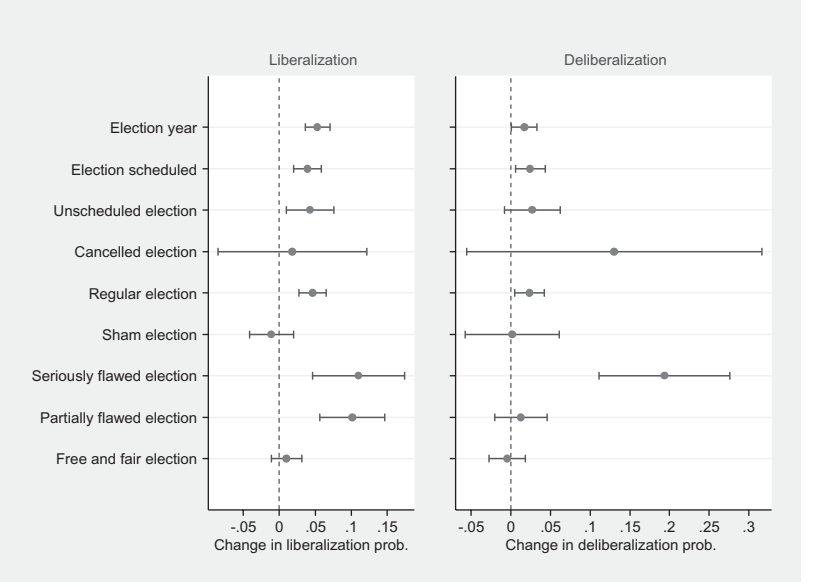
Continued on next page

Table C4 Continued

	Model 1		Model 2		Model 3		Model 4		Model 5	
DV coding rule	PolityReg2& FH-PR1 (orig)		PolityReg3& FH-PR1		PolityReg2		FH-PR1		PolityXC2&FH-CL1	
	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib	Delib	Lib
Reg_Eurasia	.518	.027	.142	−.334	1.585	.405	.207	−.630	.866	−.500
	(.915)	(.915)	(1.283)	(1.770)	(1.208)	(.747)	(.355)	(.515)	(1.155)	(1.429)
Reg_Europe	−1.960	2.121	−2.078	4.906*	.957	2.468*	−.568	1.095#	−15.794**	5.852**
	(1.453)	(1.360)	(1.626)	(2.090)	(1.418)	(1.233)	(.535)	(.658)	(1.282)	(1.971)
Reg_Americas	.178	1.850#	−.254	3.853*	.486	1.443	.189	1.143*	−1.095	3.207#
	(1.166)	(1.092)	(1.462)	(1.754)	(1.470)	(.990)	(.437)	(.518)	(1.449)	(1.706)
N	2471		2471		2471		2471		2471	

change in Polity executive constraints as the basis for coding regime change. While this approach has the advantage of using indicators, which are not affected by the nature of elections, it results in a much smaller set of liberalizations (45 vs. 95) and deliberalizations (29 vs. 67) and misses some fairly obvious instances of liberalizations (e.g. South Africa 1994) and deliberalizations (e.g. Albania 1996). Nonetheless, the results in model 2 confirm that even for this significantly modified version of our DV, elections drive both liberalizing and deliberalizing moments (though the effects were only marginally significant for the latter).

Figure C1 Elections and political liberalization/deliberalization (matching results)



Finally, as discussed in the article, we also re-estimated our tests for Figure 1 using a matching procedure instead of the multinomial logit regressions used to construct Figure 1 and presented in Table C1. We calculated average treatment effects (ATEs) using the Nearest Neighbor Matching Estimation in Stata 13 (based on the nnmatch package).

While the broad patterns are quite similar to the results reported in Figure 1 of the main article, a few differences are worth noting. First, for most indicators the substantive size of the effects is actually a fair bit higher using matching ATEs than for our simulations based on the multinomial logit regressions. Second, and relatedly, standard errors were also somewhat larger, particularly for variables with relatively rare occurrences (such as cancelled elections).

The theoretically most interesting difference is with respect to the effects of election quality. Thus, the effects of free and fair elections are now very small and statistically insignificant for both liberalizations and deliberalizations, which is even more in line with our theoretical expectations than the main results in the article. Also in line with our theoretical predictions, the effects of seriously flawed elections are even stronger for both liberalizations and deliberalizations than in Figure 1, while sham elections continue to be irrelevant. Finally, partially flawed elections are still important predictors of liberalizations, but their effects on deliberalizations are no longer even marginally significant (due to the larger standard errors, rather than to a decline in substantive effects).