

## **Vulnerability, Path Dependence, and Donor Leverage in African Democratic Experiments**

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**Abstract:** In this paper, we try to explain variations in African democratization trajectories by looking at the determinants of changes in civil liberties and human rights between the five-year period preceding 1990 and the five-year period ending in 2008. We find that the civil liberties and human rights performances of African governments respond to different factors. With regard to civil liberties, the initial transition following the Cold War largely determines subsequent trajectories. This initial transition is probably a function of regime vulnerability, as suggested by the effects of some other variables which capture vulnerability. There appears, however, to be some room for policy leverage by donors, as the organization of elections and democracy promotion aid may have some positive effects. However, elections no longer seem to matter after 1994. The degree of improvement in human rights is also largely determined in the first five years of democratization. Yet, the human rights performance of governments seems to be more immune to outside pressures—whether measured through commodity dependence, aid, or elections—and more a function of structural domestic variables such as ethnic and religious polarization. We conclude by discussing the policy implications of our findings.

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## Introduction

Twenty years after the launch of Africa's first post-Cold War democratic transitions, the continent's democratic achievements remain disparate. A few countries, like Benin or Ghana, appear to have acquired enduring democratic qualities, in contrast to their authoritarian and unstable pasts. More, like Chad or Sudan, have remained largely immune to domestic and international pressures to democratize. Others yet, like the Republic of Congo, Madagascar or Mauritania have witnessed rapidly faltering transitions and returns to authoritarianism or non-constitutional politics.

While observers of African democracy have identified an *average* continental trend characterized as hybridity—the spread of regimes with some democratic trappings but continued authoritarian and personal-rule features—, there is, in contrast, still relatively little understanding of what accounts for variations in individual democratic trajectories across the continent. Some qualitative studies have suggested the importance of certain factors such as leadership, institutional quality and resource endowment (Magnusson and Clark 2005), or institutional arrangements, rules of representation and elite interests (Villalon and Von Doepp \_\_\_) but their narrow focus limits the scope of their findings. Quantitative studies, relying in part on hypotheses derived from the broader democratization literature, have been contradictory, and have generally managed to explain only a very small proportion of the continent's democratic variance (Bratton and van de Walle 1997; Bratton and \_\_\_; Lindberg 2006). In other words, African democratization fits poorly with established theories of democracy and there is still much that we do not know about it. Of particular empirical and policy importance is the question of whether African democratic transitions are still ongoing. Aggregate trends suggest that the initial improvements of the first half of the 1990s may have stalled, bringing an end to the “transition paradigm (Carothers \_\_\_). Yet, it is unclear whether such averages capture or blur specific country experiences.

In this paper, we try to bring some light on variations in democratic performance among African countries and their causes. We do so through cross-national quantitative analysis over the 1985-2008 period. Our paper differs from previous similar empirical studies by its methodology, the scope of its time-frame, the number of variables it considers and by its findings.

## Problems of Measurement

The study of democratic trajectories always first raises the question of how democracy should be measured. There is much disagreement in this respect (see McHenry 2000; Munck and Verkuylen 2002; Bogaards 2007). Setting aside the question of whether democratization can be studied quantitatively at all, the main disagreements regard the validity of specific indicators (do they actually measure democracy?) and their potential for measurement errors (how often are they wrong?). To minimize these risks, we collected and compared data from several existing indicators of democracy. We looked at indicators of political rights and civil liberties from the Freedom House project which range from 1 to 7 (Freedom House 2009).<sup>1</sup> Political rights scores are based on the country’s electoral process, political pluralism, participation and the functioning of the government, while civil liberties scores are based on freedom of expression and belief, associational and organizational rights, rule of law, personal autonomy and individual rights. The two are highly correlated. We also looked at the Polity2 variable from the Polity IV Data set, which is simply a 21 point continuum from autocracy (-10) to democracy (10). Finally, we consulted Cingranelli and Richards’ (CIRI) Human Rights Dataset which measures “government respect for a wide range of internationally-recognized human rights” (<http://ciri.binghamton.edu/faq.asp#1>). From the CIRI dataset we included a measure of citizens’ democratic empowerment (measured on a scale from 0 to 10) and of physical integrity, which captures, on a scale of 0-8, “the rights not to be tortured, summarily executed, disappeared, or imprisoned for political beliefs.”

With respect to variable validity, we observed the extent to which these different indicators correlated with each other or not. Since they are measured by different researchers using different sources and estimation methods, a high correlation can be interpreted as a presumption of validity.<sup>2</sup>

Table 1—Pair-wise correlations of democracy indicators (within Africa)

	c12008	pr2008	pol~2007	emp2006	phys2007
c12008	1.0000				
pr2008	0.9002	1.0000			
polity2007	0.7142	0.8503	1.0000		
emp2006	0.8306	0.7707	0.6424	1.0000	
phys2007	0.6772	0.6077	0.3638	0.6772	1.0000

<sup>1</sup> We inverted Freedom Houses’ scale for ease of exposition. A score of 1 is non-democratic; 7 is democratic.

<sup>2</sup> Of course, it could also indicate collective delusion.

As Table 1 suggests, the Freedom House indicators, the polity variable and the CIRI empowerment index are highly correlated with each other within Africa, suggesting that they all seem to capture some intrinsic features of African regimes.<sup>3</sup> Yet, the CIRI physical integrity variable is more loosely correlated. This suggests that the other indicators, by focusing on more formal dimensions of the political system like free media and rights of association (for civil liberties) or competitive elections (for political right and polity) might be partly missing a human-rights dimension of democracy. In other words, some regimes might authorize opposition parties and multi-party elections, and yet still occasionally disappear their opponents. To reduce the risk of biasing our results and capture the plenitude of the meaning of democracy, we decided to perform our analysis on both civil liberties (an indicator used in many other studies, which allows for better comparisons) and physical integrity.<sup>4</sup>

Even if an indicator is broadly valid in its design, some of its specific measurements might still be “wrong.” McHenry (2000) pointed out, for example, Freedom House’s dubious ranking of apartheid South Africa as more democratic than Nyerere’s Tanzania. It is also possible that coders get unduly influenced by the holding of elections in specific years, even if the democratic qualities of these elections leave much to be desired. Another crucial issue is that of vantage point, that is, *when* democracy is measured and *over what period* it is measured. Looking at democratic trajectories produces a different picture as a function of the time period what looks at and, particularly, of the end point one looks from. While true everywhere, this is a particularly important problem in Africa where there are few consolidated regimes. In the last few years alone, some apparently democratic governments like Kenya, Mauritania and Nigeria, have experienced significant setbacks through fraudulent elections or coups. In the last few months, an elected president was assassinated in Guinea-Bissau and another deposed in Madagascar. In contrast, army takeovers in several countries, including Guinea and Mauritania, have been accompanied by promises of rapid returns to elections, suggesting a certain degree of

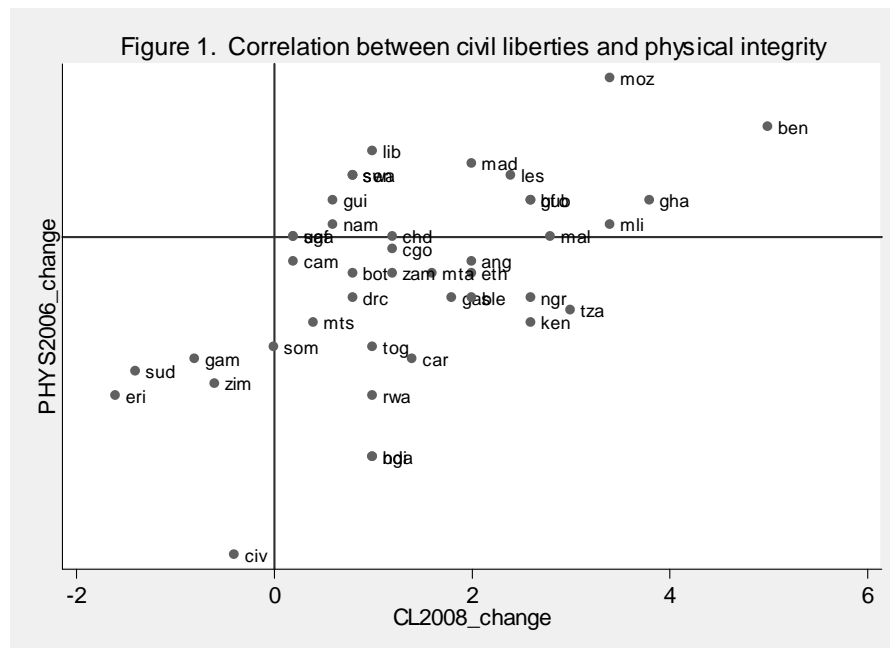
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<sup>3</sup> This is also true of the “voice” indicator by Kaufman et al (2008), which we did not include because it is only available over a much more reduced time frame (1996-2007), but whose 2007 value correlates with Freedom House’s civil liberties at  $r = .96$ .

<sup>4</sup> The CIRI data only deals with government practices. Thus, in a situation like Somalia, where summary execution, disappearances or torture might not be the results of any formally identifiable government, CIRI tends to report the data as missing. We re-coded it as 0 (and 1 for countries like DRC, Liberia, and Sierra Leone at the worst of their civil wars). This increased our data set from 38 to 42 and slightly improved model fit but did not affect the findings in a substantial way.

ingraining of electoral democracy (despite a tendency to intervene forcefully to reshape its unsatisfactory outcomes).

As a result, one's perception of democratic progress in Africa has been affected by one's vantage point. Writing in the mid-1990s, Michael Bratton and Nicolas van de Walle (1997) expressed concern at the erosion of Africa's recent democratic gains (see also Bratton 1998). Looking at data up to 2003, Staffan Lindberg (2006) saw greater cause for optimism. Given recent setbacks in countries like Ethiopia, Guinea, Madagascar, Mauritania, Kenya, Nigeria and Zimbabwe, one might conceivably end up with a more morose assessment in 2009. To mitigate this problem, we decided not to use the latest score of a country as our dependent variable, but instead its change over time. To further minimize the impact of any single value, we first average indicators over a five year period *before* Africa's democratization wave (1985-1989) and then subtract it from the average value of the indicator over the last five available years (2004-2008 for civil liberties, and 2003-2007 for physical integrity).<sup>5</sup> Not only does this method allow us to minimize the impact of measurement errors and reduce the problem of vantage point, but it also lets us more specifically focus on democratization—that is change in democratic performance—rather than on levels of democracy per se. Looking at the data in this manner also further separates the two variables from each other, with their correlation now reduced to  $r = 0.54$  (see Figure 1).

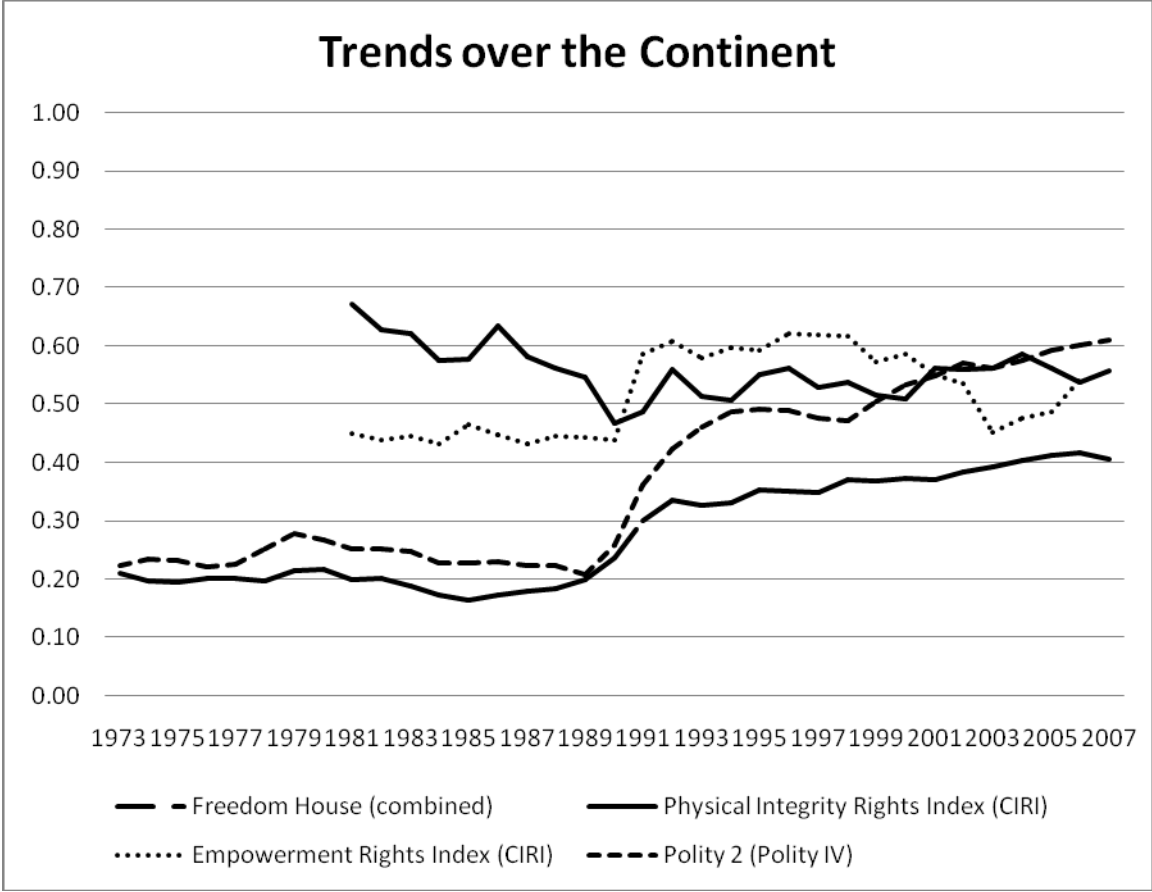


<sup>5</sup> Eritrea's first period is 1993-97; South Africa's first period is 1994-98; Namibia's first period is 1989-93

## **The Distribution of Democratization in Africa**

We begin in Figure 2 by looking at average democratization trends across the continent. For this figure, all variables have been rescaled 0-1 in percentage of their maximum possible variations. Some well-known trends about African democratization jump out. Particularly, the extent to which African regimes are “hybrid” ((Joseph \_\_; Lewis \_\_; van de Walle \_\_) is quite visible with the stagnation of all indicators around mid-values since the mid-1990s. Similarly, the democratic push of the early 1990s comes through clearly. However, we are comforted in our decision to use physical integrity by its different behavior over time: all indicators show improvement except for physical integrity. While civil liberties improves an average of 1.5 points (or 25% of its range), physical integrity decreases an average of 0.7 points (or about 8% of its range).

How are we to make sense of the average negative trend in physical integrity in Africa? Possibly, many non-democratic African countries were somewhat benign in terms of human rights before the 1990s. They may not have been democratic, but their citizens were not usually in immediate danger at the hands of the state beyond the banal arbitrariness of public authorities. In many “transitioning” countries, however, democratization has correlated with increased violence and endangerment of citizens. Some governments have granted formal freedoms while resisting their actual implementation by opponents. Others have seen the rise of militia violence together with political competition. Burundi, for example, saw a 1-point improvement in civil liberties together with a 2-point drop in physical integrity while struggling with civil war. Similarly the Central African Republic moved from 2 to 3 on the civil-liberty scale but coups, attempted coups, conflict and marauding soldiers reduced its human rights situation from 6 to 3. Even in Kenya, where civil liberties jumped from 2 to 5, physical integrity fell from 4 to 3 as the government engineered inter-communal and militia violence.



**Figure 2. Average Democratic Trajectories**

A comparison of the distribution of the two variables highlights the extent to which they capture different realities. Figures 3 and 4 show what proportion of countries fall in different categories of change. A value of 0 means that a country has neither improved nor deteriorated over the whole period. While the majority of countries are squarely on the positive side when it comes to civil liberties, the picture is inverted for physical integrity (see Appendix tables 1 and 2 for details on which countries fall in what category of performance).

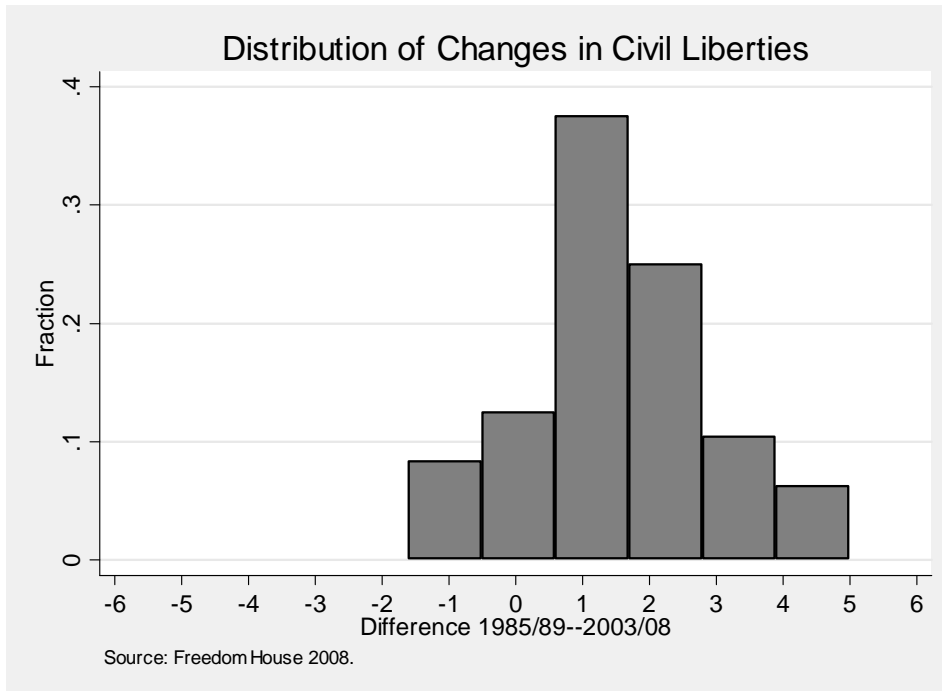


Figure 3.

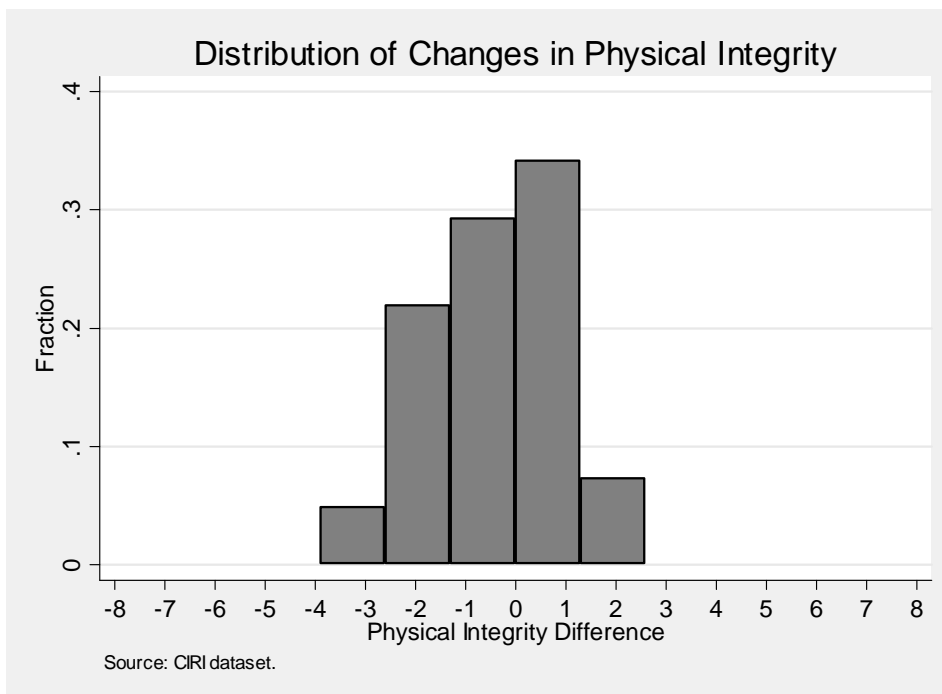


Figure 4



## The Determinants of Civil Liberties

Table 2 here

Table 2 summarizes our findings with respect to the determinants of civil liberties (Table 3 will look at physical integrity). The first model controls for the most conventional hypotheses about democratization, looking at modernization-type variables, social features, external linkages and democracy promotion. The second model focuses only on variables significantly associated with changes in civil liberties. That is the base model we use then to investigate the effects of initial change in the third model. Finally, the fourth model tries to replicate the second one looking only at the post-1994 period (past the initial transition).

The first finding to emerge from these regressions is that of the decisive effects of the initial transition. In all models, we control first for the level of civil liberties in 1989. This is equivalent to a convergence term in growth regressions because we measure our dependent variable as change over time, and we expect it thus to be negatively associated with initial values. While this effect shows up as expected, it evaporates in model (3) when we add a control for initial change in civil liberties between 1989 and 1994. This initial change variable has a strong positive impact on overall performance. Specifically, it can be interpreted to mean that 61% of entire democratic changes over the 1989-2008 period are realized by the end of the first 5-year period (1989-94). Note that including instead a similar variable for changes over the 1994-2008 period yields no significant effect. This does not appear therefore to be some statistical artifact of including an altered version of the dependent variable on the right-hand side. The disappearance of the convergence term further suggests that overall democratic performance is more a function of a country's initial transformation than of its starting point. Note also that, in the fourth model (post-1994 transformation), the magnitude of the convergence term is almost halved, suggesting an erosion of the dynamics of democratization after the initial impulse.

These findings support the idea of the "end of the transition paradigm" (Carothers \_\_\_) in Africa and of a certain degree of path dependence in African democratization. In other words, African democratic trajectories have tended to consolidate after 1994, irrespective of their level at that time. It is as if it was already written by 1994 what countries would be democratic and what countries would not, with only a few exceptions. Thus, a country's potential to

democratize over the total period studied is at least in part determined by whether or not it was successful in expanding civil liberties or maintaining a high civil liberties score during the first 5 years following the end of the cold war.

We dig deeper into this finding in Figure 5, in which we categorize countries by their patterns of change from 1989 to 1994 and observed each category's subsequent average trend in civil liberties. Taking out the "consistent democrats," those countries that initially and consistently scored high in civil liberties (and which in general had a zero to slightly negative change from 1989 to 1994), we find that countries with large changes in civil liberties (above 3 points) during the early 1990s have seen their democracy "consolidate," even at some points outperforming the "consistent democrats." The changes are much less dramatic for countries that increased civil liberties on average 1 to 2 points during the early 1990s. In general, these countries seemed to reach their collective peak in 1992 and have sustained partly free status since. Those with no change at all from 1989 to 1994 have tended to see no change either thereafter. Finally those with a negative change during this early period are the most volatile group showing a slight subsequent increase in civil liberties from 1994 to 2008. Taken as a whole, it seems that nations that democratized the fullest during the early 1990s have been able to consolidate such changes, while those that have partially democratized have either fallen back or have successfully avoided further democratization. Those that did not change or negatively changed during the early 1990s have had varying experiences of liberalization to stagnation. One cannot read too much in the post-1994 improvement of the last group, which ends up in 2008 at a similar level than it was in 1989. The original decline is due to the performance of countries that fell into conflict (Burundi, Liberia, Rwanda, Sierra Leone) and of initially somewhat better performing countries that collapsed or slipped (Gambia and Zimbabwe).

The extent to which each group's respective trajectory is flat after 1994 enlightens us about the path dependence of African democratization. Whatever slight average progress has been noted on the continent after 1994 (see Figure 2) is almost entirely due to the performance of those countries that initially moved *away* from democracy and might have subsequently bounced back a little, several of which are post-conflict countries that saw a slight boost in civil liberties in the wake of transition elections. As a group, however, these countries are hardly above the level they started from in 1989.

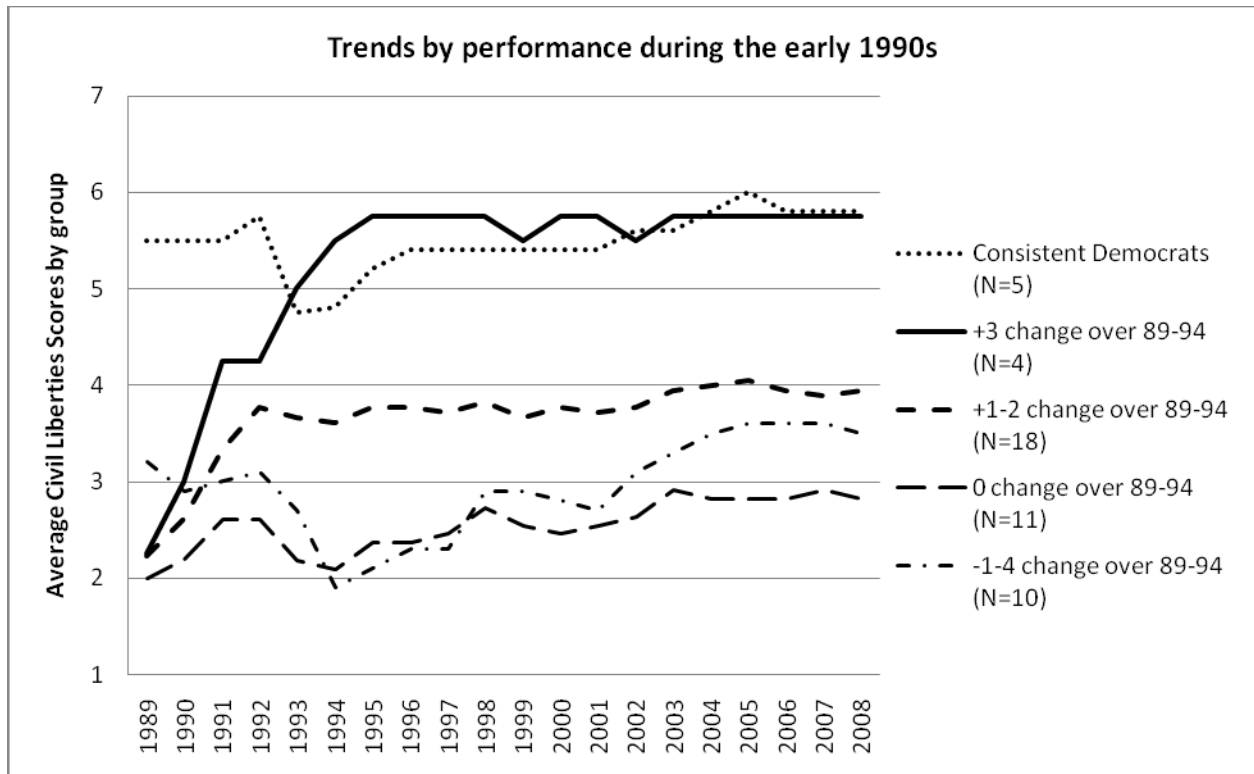


Figure 5

An analysis of variance confirms the statistically significant effects of initial change on both overall change and eventual level of civil liberties (Table 3, where the categories are those of Figure 5)

Table 3: Analysis of variance of 2008 civil liberties by initial change

Group	Obs	Mean	Std. Dev.	Min	Max
Consistent	5	5.80	0.45	5	6
+ 3 change	4	5.75	1.26	4	7
+ 1-2 change	18	3.94	1.16	2	6
0 change	11	2.82	1.60	1	5
< 0 change	10	3.50	0.85	2	5

ANOVA F Test for level of civil liberties in 2008:

F (4, 43) = 8.19\*\*\* (Prob > F = 0.0001)

ANOVA F Test for change in civil liberties, 1989-2008 (not shown):

F (4, 43) = 10.98\*\*\* (Prob > F = 0.0000)

There are two dimensions to this finding. The first is that something happened in the initial period which effectively triggered regime change. The second is that, when this something ended, countries appeared largely locked into their trajectories. Those that had democratized tended to consolidate; those that had reverted to authoritarian ways were subsequently unlikely to undo the damage; those that had been immune to the initial change remained so; etc. This suggests some lock-in mechanism which deserves further scrutiny.

Vulnerability might be part of the explanation for the first dimension. Our findings support the argument that there was a brief window of vulnerability of African regimes in the early phases of democratization which was subsequently closed but which nevertheless continues to have significant effects for countries affected by it. Countries embarked on significant democratization if the established foundations of their regime were seriously affected by the shock to the international system. The regimes of poor countries, dependent on aid for their recurring budgets and unable to rely on the resources from control of mineral commodities—like Benin, Cape Verde or Sao Tome—were incapable of resisting radical transformation. However brief the moment was in which their foreign patrons applied pressure on them, it might have been all they had at the time.

The vulnerability hypothesis is further supported by the effects of oil production and foreign aid which Table 2 also identifies. The oil dummy scores 1 for countries for which the average rents from oil exports (the value per capita of their export revenue over the cost per capita of their oil production) over the whole period represent at least 1% of their GDP (variable derived from Ross, forthcoming). It has a systematically significant negative effect on democratization (which was robust to many other model specifications). There is little doubt that regime reliance on significant rents from oil shields ruling elites from the kind of vulnerability that otherwise prompts democratic changes. Similarly, initial dependence on foreign aid is positively associated with reforms. The regression indicates an inverted-u curve relationship, but this merely captures the fact that small or basket-case countries like Guinea-Bissau, Somalia, or Equatorial Guinea received much of their minuscule GDP from aid in 1990. Apart from those, the trend is quite positive as illustrated by figure 6. Thus, the more fiscally vulnerable the country in 1990, the more likely to democratize.

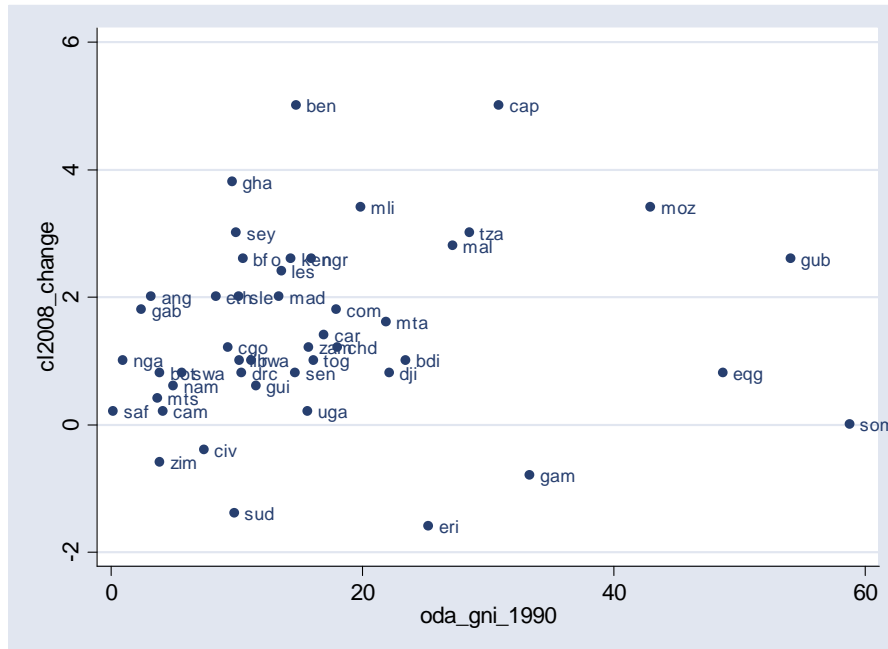


Figure 6

But what explains path dependence? It is possible that those countries that have a 3-point jump in democracy actually reach a level of civil liberties at which lock-in mechanisms take place. In other words, sufficient opening takes place for non-state or opposition actors to gain effective leverage and no longer depend on the good will and concessions of the rulers. Mechanisms of domestic accountability—such as Benin’s superior court, which played a crucial role in the early transition and might have set precedent (Magnusson \_\_)—get established and reproduced by those who benefit from them. For countries that had not democratized by 1994, the erosion of the democratic commitment of donors translated into a resumption of the previous authoritarian equilibrium. For those that had tolerated superficial changes in civil liberties (such as freedom of the media and multi-party politics), these changes endured without effectively challenging the control of incumbents over the political system (explaining their continued hybrid scores). Those that had not made any concessions by 1994, or where violent breakdown had undone such concession, no longer faced a period of sufficient vulnerability after 1994 to force them on any other path. Only countries where the breakdown was such that it called for UN or other foreign interventions saw themselves constrained to show signs of political liberalization later in the process (such as transition elections in the DRC in 2006), which resulted in small civil liberties improvements. For these countries, vulnerability continued past

1994 because their breakdown brought in international supervision. In the end, each group (except for the last one) stayed on its trajectory past 1994 and currently faces no further shock to dislodge it from such equilibrium.

Although insufficient data (n=40) prevented us from including this variable in our regressions, the above interpretation might find some support from the fact that there is a statistically significant positive association between overall changes in civil liberties and a dummy variable measuring whether or not a country experienced a reduction in its debt service ratio between 1990 and 1994. In other words, it is possible that regimes that saw immediate material benefits from their initial transition were reinforced in their virtuous behavior, which generated some form of lock-in mechanism.

And yet, despite these rather deterministic findings, Table 2 also seems to suggest that donors have not become entirely devoid of leverage. In every model we include two variables which approximate some additional levers that donors may pull to encourage change in civil liberties: the number of elections held in the country (frequently demanded, paid for and partly organized by donors) and democracy-promotion financial assistance (which goes beyond electoral support). While both variables are significantly and positively related to civil liberties in most models, we remain cautious of assuming that this association is necessarily causal.

Before we discuss the effect elections may have on civil liberties, it must be noted that there were significant measurement issues with this variable, which affect the results. We took our initial data on this variable from the work of Lindberg (2006) who argues that elections democratize a polity from the ground up. Lindberg identifies several phenomena that are byproducts of a nation holding elections, like the education of voters, training of domestic election officials, civil society organizations becoming hyper active, and added media criticism, all of which he argues contribute to a general increase in the likelihood that citizens will demand more civil liberties (2006:122-123). Lindberg finds support for these notions through analyses that positively link a nation's record of holding elections to its civil liberties score in 2003. However, he is not particularly explicit in his book on his measurement method. From the book and from corresponding with him by e-mail, we inferred that he counts either the total number of presidential or the total number of legislative elections a country has had since its last "breakdown" (coup or civil war), whichever is most.

Our count of elections diverges from Lindberg's in four ways. First, we extend coverage to the end of 2008. Second, because Lindberg (2006) provides no theoretical reasoning why we should ignore the possible democratizing effects of legislative elections if it is a presidential system or has had more presidential elections, we count both presidential and legislative elections. Third, we only count an election if it occurred after 6 months from the last election. Lindberg (2006) outlines many activities that are associated with an election which can have democratizing effects prior to voting day, as well as post voting day. Therefore one cannot count two elections held on two consecutive days as having an individual and distinct effect on democracy, because they are part of the same election period. Finally, our election count does not reset for breakdowns. We fear that taking breakdowns into consideration makes the measure of elections endogenous to democratic developments. In other words, starting the count at 0 after a breakdown makes the "number of elections" a function of whether democracy encountered a setback. Whenever it does, it is deemed that elections never took place. This strikes us as both methodologically and substantially unsound.

Using different measures of elections yielded different results (as did variations in control variables). Including all elections, presidential and legislative, even when occurring on the same day, yielded very significant positive results when estimating effects on the change of civil liberties over the entire period (1989 to 2008). On the other hand, including whichever-is-most of parliamentary or presidential elections since the last setback (which we think is the closest to Lindberg's method) fell well short of significance. Looking at parliamentary elections for parliamentary regimes and presidential elections for presidential regimes (resetting for setbacks) was also significant. It is hard to know how much to read into these results. The good news for supporters of elections in Africa is that the coefficients are always positive. Their lack of systematic significance, however, sheds doubt as to whether they are worth the effort they represent. Given their potentially negative consequences on conflict and development (Collier 2009), our results support a cautious assessment.

Outside of measurement issues, we still do not find a robust association of elections with civil liberties changes. When we change our vantage point and move from evaluating electoral effects over the entire period (1989 to 2007) to evaluating them after the initial transition period, they are no longer significantly associated with changes in civil liberties. That is, once we focus on changes after the initial spike of aggregate democratization (our "moment of vulnerability"), a

nation's electoral history does not significantly affect its democratic trajectory. These contradictory findings indicate that the power of elections have either lost their democratizing touch post 1994 or that the seemingly significant relationship of higher numbers of elections held and democratic improvements is not causal.

Our findings indicate a more robust relationship between US and UK democracy promotion assistance and changes in civil liberties (at this point we have only managed to obtain democracy-promotion data from the UK and US). Each are represented in the models as averages over the time frame for which we had available data (for the US this was 1990-2003; for the UK this was 1990-2007). Even as we narrow our focus on changes between 1994 and 2007 we find that democracy assistance continues its significant and positive relationship. The causality here is also quite ambiguous, however.

The flows of democracy promotion aid do seem to follow some rules associated with the level of democracy in a recipient country. However, it does not appear to be simply following the good performers. Instead, not including South Africa, none of the nations considered to be "consistent democrats" (those that had a high level of civil liberties prior to 1994 and have maintained high levels to present) as well as the nation's with the largest increases in civil liberties over 1989 to 1994 were of the top ten recipients in Africa of democracy promotion foreign aid from our two donors.<sup>6</sup> Furthermore, donors are generally not giving aid to the "lost causes" or the deeply entrenched authoritarian regimes like Equatorial Guinea, Gabon, Chad, Mauritania, Djibouti or Togo. Also in some cases, nations rebounding from civil war get surges of democratic promotion assistance, like Rwanda. These descriptive trends lead us to believe that donors are targeting those nations where they feel that their money can help a growing democratic movement or trend, while they avoid wasting their aid on nations which have consolidated as either democratic or authoritarian regimes. This is not to deny that this aid might have positive effects on democratization, but it does suggest that the results may be somewhat endogenous, since we focus on democratic changes.

Additionally there are some clear differences between these donors in terms of their targets. Two trends are noteworthy. First, the UK disproportionately funds its former colonies.

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<sup>6</sup> The US on average from 1990-2003 gave about 19 million dollars in democracy promotion aid to South Africa. This is nearly 4 times more than what the US gave on average to the Nigeria, a country that has more than twice as many people as South Africa and received the next highest amount in democracy promotion from the US. The decision to run the models without South Africa as an outlier seems to be justified on this basis alone.



The UK funds democracy promotion on an average of three times more in former colonies than it did in other African countries. Second, while the UK clearly favors former colonies, the US has tried to spread its money around. The UK has not given democracy promotion assistance to 12 African countries, while the US has only avoided 6.

To further explore the possibility that democracy promotion flows are impacted by the democratic nature of the recipient regime we also assessed the relationship using panel data. Keeping civil liberties as the dependent variable we analyzed different lags of democracy promotion assistance. With these models we find that democracy promotion assistance is significantly related to civil liberties when democracy promotion is measured as the same year as civil liberties, but is more significant when it is assumed to have a 1 to 2 year lag. Models estimating the effects of civil liberties on democracy promotion assistance indicated that civil liberties did not significantly affect democracy promotion with a 1 to 2 year lag. These anecdotal analyses give some reason to believe that it is democratic changes that are following democracy aid and not the other way around.<sup>7</sup>

The nature of the relationship still remains unclear. We have identified some clear patterns in how the donors allocate these funds, which highlight the real possibility that the selection criteria used to distribute such aid may be what is at the root of the significant association. In other words, it might be the case that donors have just bet on the right horses.

## **The Determinants of Human Rights**

Table 3 here

In general, variables that are conventionally associated with civil liberties and other formal dimensions of democracy are less accurate predictors of government practices with respect to human rights. The percentages of variance in physical integrity explained by our different models in Table 3 are indeed smaller than in Table 2. Moreover, there are some important missing observations in the models of Table 3. Although, with 41 observations the sample size remains reliable, the six missing observations are not random and their absence might affect the

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<sup>7</sup> Finkel et al. 2007 come to a similar conclusion through a hierarchical mixed model of USAID democracy promotion assistance and its effects on various indicators of democratization. Additionally they perform two-stage least squares and find no evidence that USAID democracy promotion assistance is driven by the recipient country's level of democratization.

results. There is indeed no CIRI data for Cape Verde, Comoros, Djibouti, Equatorial Guinea, Sao Tome and Seychelles. These are all small countries (which might affect our estimation of the effects of size). In addition, both Cape Verde and Sao Tome are among the top democratizers and are rather benevolent regimes. Their absence might therefore bias the data by reducing its upper bound. Finally, Equatorial Guinea is an oil producer and fast-growing dictatorship, which may also artificially reduce the effects of some economic variables.

We proceed with the changes in human rights as we did for civil liberties by estimating the effects of all of the variables we have identified to represent the conventional hypotheses for democratization first and then adding the initial (1989 to 1994) change to a smaller model narrowing in on those variables we find to be significantly related to changes in physical integrity. We find that, like with civil liberties, the changes over the entire period are at least partly determined by the changes in human rights in the early 1990s. This effect however is less significant and substantive when compared to the effect early changes had on overall civil liberties trajectories, suggesting that there is still a certain, albeit lesser, degree of path dependence when it comes to human rights. A nation experiencing a 4 point (half of the possible change if a nation was at the minimum of the physical integrity scale) positive change in the early period is associated with a total positive change of only 0.72, while the same early change in civil liberties (3 points or half of the possible change if a nation was at the minimum of the civil liberties scale) is associated with a 2.16 change in civil liberties over the entire period. The fact that the human rights trajectories may be less affected by early change may actually be of some comfort in terms of the aggregate continental trends. 20 nations experienced a decline in their physical integrity scores over 1989 to 1994, while only 12 nations experienced a decline in civil liberties over the same period. In other words, if this path dependence was more substantive we would predict that in general physical integrity scores would have deteriorated even further for almost half of our sample. Additionally, the physical integrity score in 1989 is not robustly significant in models 1 to 3, indicating that nations have not necessarily converged over the entire period. This is not too surprising given the lack of trend in the data in Figure 2.

In sharp contrast to our findings on the determinants of changes in civil liberties, we find that the variations in the changes of human rights are *not* related to the same variables that have seemed to impact the changes in civil liberties. We find that human rights trajectories are a function of more structural variables like religious and ethnic polarization. In other words,

countries that have large Muslim and Christian populations and countries with large populations of “competing” or different ethnic groups tend to have more negative changes in human rights over the entire period. Religious polarization has a slightly larger negative effect on the change in human rights than ethnic polarization. A 0.05 increase in religious polarization (or 20% of the range) results in a 0.5 decrease in human rights (6.25% of the range). Figure 6 illustrates that Côte d’Ivoire, Eritrea, and Nigeria contribute most of this effect. The same increase in ethnic polarization (0.05) results in a 0.44 decrease in the changes in human rights (5.52% of the range). Figure 7 illustrates that Côte d’Ivoire’s experience also helps characterize this correlation. Also, while it is obvious that Burundi and Rwanda’s ethnically charged civil wars would contribute to this trend it is also clear that a case like Zimbabwe, where an increasingly repressive Mugabe regime has coincided with an ethnically polarized polity, has helped shape this relationship.

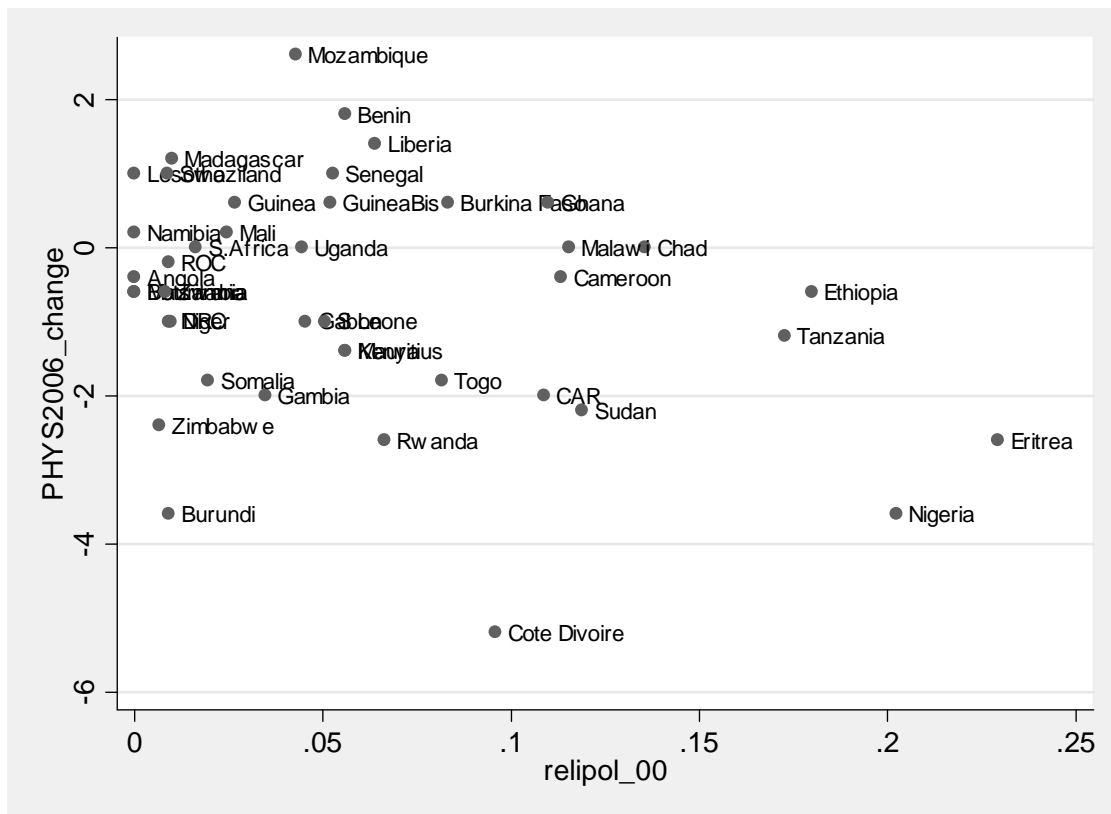


Figure 6

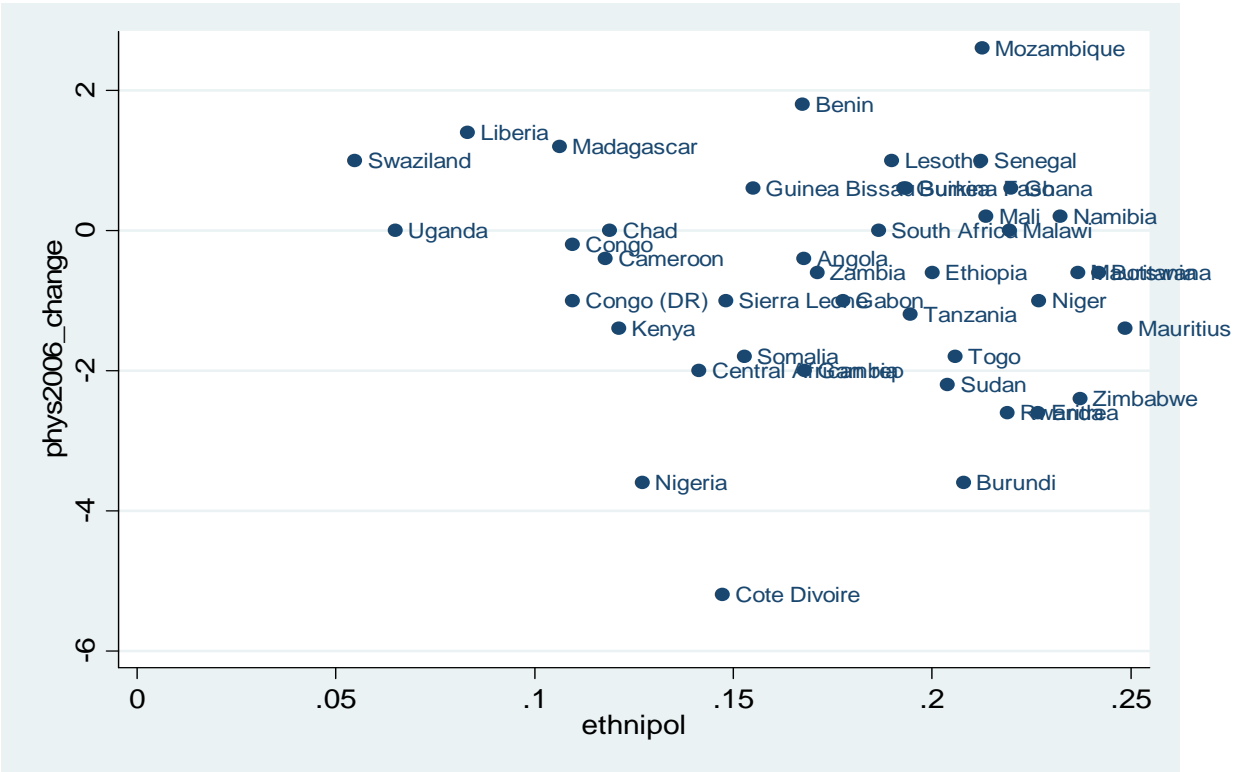


Figure 7

The average literacy rate has a significantly negative effect on the change in physical integrity, while a nation’s average economic growth rate has a positive effect. We are not aware of any theoretical explanation that would account for the simultaneity of these results.

There is a small but significant relation between holding elections and human rights performance. As with civil liberties, however, this relationship is not robust to control for the initial change in human rights. This might suggest that the relationship is probably not causal. Countries that had early improvements in human rights also were having elections in this first five year period, and have continued to have them. Subsequent elections, however, have not been associated with further improvements in human rights. Model 4, which focuses only on the 1994-2007 period indicates indeed that there is no relationship between the number of elections and changes in human rights after the initial transition. These results tend to shed some doubts on the argument that it is the repeated practice of elections per se, irrespective of their quality, which intrinsically improves democratic performance (Lindberg 2006). This finding is consistent with the lack of effect of elections on civil liberties which we identified for the same period in the previous section.

Similarly, there is no significant effect of US and UK democracy promotion assistance on the status of human rights in Africa, although the UK democracy assistance actually has a human rights component.

While civil liberties trajectories seemed to have been shaped by the extent of the nation's vulnerability to an external shock, the same cannot be said with the changes in human rights. Our results indicate that in some ways, the two measures of democracy could not respond to more different sets of variables. Oil rents have not necessarily buffered a regime from external pressure to liberalize. A nation's vulnerability to external pressures to democratize, as partially measured through its dependence on foreign aid in 1990 is not significantly related to the increases in the protection of human rights. The effects of holding elections and democracy promotion aid, two additional avenues donors have used to encourage political liberalization, do not have robust effects on the changes in physical integrity. While the extent to which a nation was vulnerable to the shock of the end of the Cold War has no discernable effect on the change in human rights, our models point to variables which are to a certain extent immune from outside manipulation to be the real drivers of the various human rights trajectories.

Even as we focus on the changes in the later period (1994 to 2007) the image of the significant impact of structural domestic variables on the changes in human rights continues. Outside of the convergence term (1994), religious polarization is the only significant variable. Its effect is negative and substantively close to the previous models. This paints a bleak picture for the leverage that the West may have in positively influencing human rights records in Africa. Short of encouraging territorial reconfigurations, our analyses suggest that the West will likely have little effect on the trajectories of human rights.<sup>8</sup> African nations may have liberalized formally to appease donors at a time of extreme vulnerability and further Western democracy promotion funds may have influenced continued positive changes in the more formal characteristics of democracy, but this era of vulnerability and democracy promotion funding has not produced the same impact on a regime's likelihood of increasing its protection of human rights.

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<sup>8</sup> On the potential benefits of territorial change in Africa, see Herbst (2000) and Englebert (2009).

## **Conclusion**

Our most important finding is that African democratization is path dependent: whatever transition to democracy is achieved by 1994 largely determines subsequent regime type and dynamics. We attribute this effect to a combination of initial vulnerability and subsequent lock-ins. African regimes are extraverted (Bayart 1989, Clapham 1996) and no single event has had greater destabilizing effect on them than the reshuffling of international patronage networks that followed the end of the Cold War in 1989. For countries deprived of secure resources like oil and under fiscal duress, the breakdown of Eastern European authoritarian models and donor reconsideration led to significant political changes. These subsequently endured because they might genuinely have empowered social and political groups outside of government and/or because they produced material returns that rewarded democratization and encouraged further democratic behavior.

Second, the organization of elections per se has little significant effect on continued democratization after 1994. Before 1994, it correlates by definition with transition. Subsequent elections, however, do not have democratizing effects per se. Although democracy promotion assistance by Western donors is significantly associated with positive changes in civil liberties, there are too many reasons for suspecting endogeneity and selection bias to impute too much leverage to donors.

Our third important finding is that the human rights performance of African regimes proceeds from a somewhat distinct causality from their civil liberties. While we also find a certain degree of path dependence with human rights, they tend to be more a function of structural variables than are civil liberties, including ethnic and religious polarization, and the extent of literacy. Otherwise, bearing in mind the caveat of incomplete and possibly biased data, very few of the variables conventionally associated with democratization seem to matter. More importantly, neither the organization of elections nor democracy promotion assistance seem to have any positive (or negative) effect on the human rights performance of regimes.

If we are right about the effects of vulnerability, and the subsequent horizontal trajectory of states, then it is quite possible that no further significant democratization can take place in Africa unless some equivalent circumstances are recreated in which incumbent regimes lose their balance and are genuinely constrained to make significant and binding concessions to their populations. Conditionality and other conventional tools of donor leverage do not clear this threshold. The current erosion of

impunity for criminal rulers might contribute to a similar effect (though it could also backfire). Providing sanction for behavior inimical to human rights and democratic accountability, removing the impunity of African rulers and challenging the economic abuses of office (as with the recent indictment in French courts of Omar Bongo, Denis Sassou-Nguesso and Obiang Nguema for ill-acquired wealth) might generate vulnerability among African regimes and open up avenues of sustainable changes. The focus on human rights might also promote change in this area, which has so far shown greater immunity to conventional donor pressure.

Appendix Table 1. Categories of Change in Civil Liberties

<u>Negative or no Change</u>	<u>Small Positive Change</u>	<u>Large Positive Change</u>
Eritrea (-1.6)	Cameroon (0.2)	Lesotho (2.4)
Sudan (-1.4)	Uganda (0.2)	Guinea-B (2.6)
<i>Gambia (-0.8)</i>	<i>South Africa (0.2)</i>	Burkina (2.6)
Zimbabwe (-0.6)	<i>Mauritius (0.4)</i>	Niger (2.6)
Côte d'Ivoire (-0.4)	<i>Namibia (0.6)</i>	Kenya (2.6)
Somalia (0.0)	Guinea (0.6)	Malawi (2.8)
	Swaziland (0.8)	Tanzania (3.0)
	<i>Botswana (0.8)</i>	Seychelles (3.0)
	<i>Senegal (0.8)</i>	Mali (3.4)
	Eq. Guinea (0.8)	Mozambique (3.4)
	DR Congo (0.8)	Ghana (3.8)
	Djibouti (0.8)	Sao Tome (4.6)
	Liberia (1.0)	Benin (5.0)
	Rwanda (1.0)	Cape Verde (5.0)
	Togo (1.0)	
	Nigeria (1.0)	
	Burundi (1.0)	
	Zambia (1.2)	
	Chad (1.2)	
	Rep. Congo (1.2)	
	Cent. Afr. Rep. (1.4)	
	Mauritania (1.6)	
	Comoros (1.8)	
	Gabon (1.8)	
	Ethiopia (2.0)	
	Angola (2.0)	
	S. Leone (2.0)	
	Madagascar (2.0)	

*Italics: Countries with scores of 5 or more as of 1989 (or at inception).*



Appendix Table 2. Categories of Change in Physical Integrity

<u>Negative or no Change</u>	<u>Small Positive Change</u>	<u>Large Positive Change</u>
Côte d'Ivoire (-5.2)	Namibia (0.2)	Mozambique (2.6)
Nigeria (-3.6)	Mali (0.2)	
Burundi (-3.6)	Guinea-B (0.6)	
Eritrea (-2.6)	Guinea (0.6)	
Rwanda (-2.6)	Ghana (0.6)	
Zimbabwe (-2.4)	Burkina (0.6)	
Sudan (-2.2)	Swaziland (1.0)	
Gambia (-2.0)	Senegal (1.0)	
Cent. Afr. Rep. (-2.0)	Lesotho (1.0)	
Togo (-1.8)	Madagascar (1.2)	
Somalia (-1.8)	Liberia (1.4)	
Mauritius (-1.4)	Benin (1.8)	
Kenya (-1.4)		
Tanzania (-1.2)		
Niger (-1.0)		
Sierra Leone (-1.0)		
DR Congo (-1.0)		
Mauritania (-0.6)		
Ethiopia (-0.6)		
Botswana (-0.6)		
Zambia (-0.6)		
Cameroon (-0.4)		
Angola (-0.4)		
Rep. Congo (-0.2)		
Uganda (0.0)		
Chad (0.0)		
Malawi (0.0)		
South Africa (0.0)		

Missing data for: Cape Verde, Comoros, Djibouti, Equatorial Guinea, Sao Tome and Seychelles.

### Appendix 3: Measurements and Sources

- The average GDP growth from 1990 to 2006/2007 (2006 for physical integrity models and 2007 for civil liberties models) is taken from the World Bank Development Indicators (WBDI).
- GDP per capita in 1989 was gathered for both models from WBDI.
- Oil rents per capita is from Michael Ross' forthcoming book (see Michael Ross' link at [www.polisci.ucla.edu](http://www.polisci.ucla.edu)).
- Literacy rates over the 1990 to 2006/2007 period are from WBDI.
- Overseas development assistance as a percent of GNI in 1990 was included in the analysis so as to approximate the country's level of dependence on foreign aid in 1990. During the 1990s Western donor countries rhetorically claimed that foreign aid would be suspended or given based on some basic democratic conditionalities. Including a country's dependence on foreign aid during 1990 approximates a control for how vulnerable nations would have been to these outside democratic demands. This data was gathered from the WBDI.
- US democracy assistance was an average of total USAID investment for all democracy and governance programs from 1990-2003, (Finkel et al. 2004). Data is in constant 2000 US millions of dollars.
- UK democracy assistance was an average of total DFID investment for all democracy and governance programs from 1990 to 2008. This data was given to the authors from the DFID. Data is in constant 2000 US millions of dollars.
- We consulted Alesina et al's 2003 data for ethnic fractionalization.
- Religious polarization multiplies the proportion of Christians with the proportion of Muslims (source: \_\_\_\_)
- Country size is from the CIA World Factbook.

Table 2	(1)	(2)	(3)	(4)
	CL Change 89-08	CL Change 89-08	CL Change 89-08	CL Change 94-08
<i>Civil Liberties 1989</i>	-0.69*** (0.15)	-0.66*** (0.15)	-0.12 (0.18)	<i>Civil Liberties 1994</i> -0.39*** (0.13)
<i>GDP per capita 1989</i>	0.69** (0.32)	0.78** (0.29)	0.58** (0.24)	<i>GDP per capita 1994</i> 0.58*** (0.18)
<i>Oil Rents</i>	-1.43*** (0.51)	-1.49*** (0.46)	-0.88** (0.34)	<i>Oil Rents</i> -1.08*** (0.30)
<i>ODA % of GNI 1990</i>	0.10 (0.06)	0.11* (0.06)	0.11** (0.04)	<i>ODA % of GNI 1994</i> 4.41* (2.49)
<i>(ODA % of GNI 1990)<sup>2</sup></i>	-0.00* (0.00)	-0.00* (0.00)	-0.00** (0.00)	<i>(ODA % of GNI 1994)<sup>2</sup></i> -4.41* (2.31)
<i>Number of Elections</i>	0.24** (0.09)	0.27*** (0.09)	0.19** (0.09)	<i>Elections since 1994</i> 0.07 (0.13)
<i>US and UK Democ. Aid 90-07</i>	0.34*** (0.11)	0.33*** (0.10)	0.40*** (0.11)	<i>US and UK Democ. Aid 94-07</i> 0.30*** (0.09)
<i>Ethnic Polarization</i>	1.07 (3.18)			
<i>Religious Polarization</i>	-2.66 (3.43)			
<i>Geographic Size</i>	-0.00 (0.00)			
<i>GDP growth 1990-2007</i>	0.05 (0.07)			
<i>Change in Civil Lib 89-94</i>			0.61*** (0.14)	
<i>Observations</i>	46	46	46	44
<i>R-squared</i>	0.46	0.43	0.64	0.45
<i>F test</i>	4.07	6.65	12.27	12.40
<i>Prob &gt;F</i>	0.00	0.00	0.00	0.00

Table 3	(1)	(2)	(3)	(4)
	PI change 89-07	PI change 89-07	PI change 89-07	PI change 94-07
<i>Phys. Int. 1989</i>	-0.31* (0.16)	-0.21 (0.13)	-0.07 (0.16)	<i>Phys Int. 1994</i> -0.77*** (0.12)
<i>GDP growth 90-07</i>	0.32* (0.19)	0.30** (0.13)	0.26* (0.13)	<i>GDP Growth 90-07</i> 0.24 (0.19)
<i>Ethnic Polarization</i>	-9.39 (6.03)	-7.87* (4.58)	-8.83** (4.24)	<i>Ethnic Polarization</i> -2.16 (5.91)
<i>Relig. Polarization</i>	-9.24** (3.79)	-10.27** (4.03)	-10.09*** (3.56)	<i>Relig. Polarization</i> -8.75** (3.51)
<i>Ave. Literacy 90-07</i>	-0.58 (0.46)	-0.92** (0.39)	-1.06** (0.41)	<i>Ave. Literacy 90-07</i> -0.19 (0.74)
<i>Number of Elections</i>	0.20* (0.10)	0.16* (0.09)	0.12 (0.09)	<i>Elections since 1994</i> 0.20 (0.20)
<i>Oil Rents</i>	-0.59 (0.78)			
<i>GDP per capita 1989</i>	0.06 (0.49)			
<i>ODA % of GNI</i>	0.07 (0.08)			
<i>(ODA % of GNI)<sup>2</sup></i>	-0.00 (0.00)			
<i>Geographic Size</i>	-0.00 (0.00)			
<i>US and UK Democ. Aid 90-07</i>	-0.03 (0.16)			
<i>Change in Phys Int. 89-94</i>			0.18** (0.08)	
Observations	41	41	41	41
R-squared	0.47	0.39	0.43	0.51
F test	2.65	3.59	5.59	8.87
Prob >F	0.0164	0.00731	0.000262	7.58e-06

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