

Democracy, Dictatorship, and Disease:

Political regimes and the fight against HIV/AIDS

Mogens K. Justesen

Department of Political Science

University of Southern Denmark

E-mail: mju@sam.sdu.dk

February 2010

This is a preliminary draft

PLEASE DO NOT QUOTE

Abstract

Despite an extensive empirical literature, there is little agreement on how and why political regimes affect social and economic development. Rather than examining the effect of political regimes on broad social outcomes, this paper examines how political regimes affect specific health policies relating to the treatment of HIV/AIDS – one of the most widespread and deadly epidemics of modern time. Drawing on arguments from political economy, the paper assesses whether political regimes – democracy and dictatorship – matter for access to treatment of HIV/AIDS. Using regression and matching methods on data for a broad cross-section of countries, the results show that democracy increases access to treatment of HIV/AIDS. For developing countries alone, the effect of democracy remains positive, but the significance and magnitude of the effect diminishes somewhat. This suggests that we should be careful about attributing too much importance to the role of political regimes in the battle against HIV/AIDS.

Introduction

Explanations of the wealth and poverty of nations have focused extensively on whether democracy promotes or hinders economic development. A host of studies have tried to examine the direct effect of political regimes on economic growth (Barro 1996; Persson and Tabellini 2008; Przeworski and Limongi 1993; Przeworski et al. 2000; Rodrik and Wacziarg 2005). The empirical results are mixed, however, and there is no general agreement on how democracy affects economic growth. Indeed, sceptics argue that it is doubtful whether political regimes matter at all. In their influential study, Przeworski et al. (2000: 178) have even gone so far as to claim that “the entire controversy seems to have been much ado about nothing”. In recent years, a related line of research has turned attention away from the direct economic effects of political regimes and towards possible indirect effects through policies, institutions, and social outcomes (Baum and Lake 2003; Lake and Baum 2001; Mulligan et al. 2004; Pinto and Timmons 2005; Tavares and Wacziarg 2001). Part of the motivation behind this turn of attention is that, to some extent at least, studies of democracy’s direct effect on growth are misguided, because political regimes and governments do not affect economic growth directly. Indeed, there is no theory to suggest any such direct and unmediated effect (Tavares and Wacziarg 2001: 1342-1343). Rather, what governments do is to select and enforce a set of policies and institutions which, in turn, may affect economic growth and development. If anything, the effect of political regimes on growth should therefore be indirect and mediated through policy choices and the provision of public goods and government services.

Following this logic, a growing body of literature has begun to examine the effect of political regimes on public policies and development outcomes such as public health and education, which are widely accepted as important determinants of economic growth (Barro 1996; Lorentzen et al. 2008; Pinto and Timmons 2005; Tavares and Wacziarg 2001). In two related articles, Lake and Baum (2001) and Baum and Lake (2003) find that democracy affects growth indirectly by increasing human capital accumulation and life expectancy. Besley and Kadumatsu (2006) too find that life expectancy is higher in democracies compared to dictatorships. While the results of Tavares and Wacziarg (2001) and Pinto and Timmons (2005) suggest democracy has a negative effect on investment rates, their findings also indicate that that democratic regimes accumulate human capital to a larger extent than dictatorships. In a study of education spending in Africa, Stasavage (2005) also finds that democratic governments allocate more economic resources to education than non-democracies. This finding is corroborated in studies of Latin American

government spending on health and education by Kaufman and Segura-Ubiergo (2001) and Avelino et al. (2005). While the consensus from these studies seems to be that democracy improves public health and human capital accumulation, work by Ross (2006) has challenged this claim and finds no difference between democratic and non-democratic regimes in terms of infant mortality rates as an indicator of development and poverty. Similarly, the results of Mulligan et al. (2004) suggest that taxes and government spending on education and social security is not affected by political regimes. These results question whether democracy matters for development – even in an indirect way.

This paper expands on this literature by examining how political regimes affect health policies across countries, particularly policies relating to the treatment of HIV/AIDS – one of the most widespread and deadly epidemics of modern time, with disastrous consequences for human life and economic development. The paper contributes to the existing wisdom in three ways. Firstly, in spite of its harmful impact on human and economic development, no study has yet provided any general and systematic, let alone quantitative, analysis of how political regimes shape policies aimed at treating victims of HIV/AIDS. The aim of this paper is to fill this gap. Secondly, the paper departs from the common practice of many studies of democracy's effect on public health and human capital, which is to examine broad social outcomes such as life expectancy and infant mortality (Baum and Lake 2003; Besley and Kadumatsu 2006; Lake and Baum 2001; Ross 2006). However, just as governments cannot directly select economic growth rates, they cannot directly control social outcomes either. In so far as political regimes have effects on development indicators such as life expectancy, it is mainly through the selection of health policies and public services provided to citizens. Rather than examining social outcomes, this paper considers the effect of political regimes on specific policy choices relating to the medical treatment of HIV/AIDS victims. Distinguishing social outcomes from public policies is particularly relevant in the case of HIV/AIDS. Actual rates of HIV/AIDS prevalence is affected by a host of behavioural, cultural, social, and economic factors (Epstein 2007; Gizelis 2009). While Gizelis (2009) and Iqbal and Zorn (2010) examines the effect of political regimes on HIV/AIDS infection rates across countries, this is a questionable choice of dependent variable –at least in relation to political regimes – since it is very difficult for governments to control exposure to risk of infection at the individual level. However, once infected, governments can control – or at least have some leverage over – the availability and coverage of treatment for the victims of HIV/AIDS. In this way, governments directly influence the availability and scope of public health services, but not necessarily the prevalence of disease in society. By distinguishing outcomes from policies we thereby avoid

assigning ‘bad policies’ to countries – such as Botswana – that have high rates of HIV prevalence but also progressive policies and broad-based treatment programs. Thirdly, while practically all the studies quoted above rely on some form of linear regression technique, this does not necessarily imply that the results can be interpreted in causal terms. The usual approach to deal with this issue relies on instrumental variables to identify causal effects. However, it is often very difficult to find valid instruments for political institutions that satisfy the exclusion restriction, which essentially implies that the entire effect of a valid instrument must be transmitted solely through the endogenous variable, in this case democracy (Przeworski 2007). This assumption is rarely satisfied, even in the best applications. As an alternative, this paper uses so-called matching methods, which have become increasingly popular means of trying to uncover causal relationships when valid instruments are in short supply. The key idea of matching is to try to approximate the ideal of a controlled experiment in samples with observational data. If successful, the point is that we can create two groups that are (fairly) similar on all observed and relevant characteristics, except that one group receives a ‘treatment’ (in this case democracy) whereas the other one – the control group – does not. Deriving the counterfactual of the treated cases from comparisons with similar controls therefore enables us to estimate the effect of political regimes on the outcome of interest.

To develop the analysis of the impact of political regimes on HIV/AIDS policies, the paper draws upon recent contributions to the political economy of democracy and dictatorship (Acemoglu and Robinson 2001, 2006; Bueno de Mesquita et al. 2003; Besley 2006; Olson 1993, Wintrobe 1998). The basic feature that distinguishes democracies from non-democracies is that in the former, voters can use elections to hold governments accountable for their performance in office. In order to maximize chances of re-election, democratically elected governments therefore need to be minimally responsive to the demands of voters. By contrast, non-democracies possess no such mechanism for selecting political leaders. A decision by non-democratic governments to accommodate to the demands of citizens is therefore a matter of choice rather than a matter of electoral and institutional constraint. Electoral incentives also imply that democratically elected governments become accountable to a large (majority) coalition of citizens. Non-democratic governments on the other hand typically rely on the support of small, powerful elite coalitions to stay in office. This means that democratically elected governments face incentives to provide public service – including health care – on a broad scale that is available to large groups in society, while non-democratic governments have incentives to target government spending at elites as selectively provided goods. In this scenario, health care and medical coverage, including treatment of

HIV/AIDS, may therefore be expected to be available for larger groups of citizens in democracies relative to non-democracies.

However, some arguments emphasize that it is precisely the fact that non-democratic governments do not face electoral and institutional constraints that provide them with the means to respond swiftly and effectively to a major crisis like an HIV/AIDS epidemic. Relative to democratic governments, the lack of electoral and institutional constraints provide dictators with a comparative advantage in rapid action, and this may be precisely what is needed to respond effectively to major crises (Wintrobe 1998: 247-49). Likewise, the fact that HIV/AIDS often affects relatively small voter groups in society may reduce the incentives of democratic governments to respond effectively to their demands. Dictatorial governments, on the other hand, need not accommodate to broad voter preferences, and can therefore effectively pursue policies aimed at relieving major crises, even though such policies may lack support among a majority of the electorate.

On this background and using data for a broad cross-section of countries, the empirical part of the paper tests whether political regimes matter for access to treatment for HIV/AIDS victims. Results obtained from regression and matching estimators suggest that procedural democracy does have a significantly positive effect on the treatment of HIV/AIDS by anti-retroviral medicine. While this also applies when the sample is reduced to developing countries alone, the magnitude and significance of the democratic effect diminishes somewhat for countries in the developing world. This suggests that democracy is not a panacea for addressing the HIV/AIDS issue, but that democracy may nonetheless have *some* effect on policies aimed at treating victims of HIV/AIDS.

The remainder of the paper proceeds as follows. The next section briefly considers why the HIV/AIDS issue is important from a social science perspective by describing its human and economic consequences. The section after that outlines the theoretical argument relating political regimes to public policy and health care services. Next, the data and methods used in the empirical analyses are described. The following section presents results obtained from cross-sectional regressions and matching methods of the effect of political regimes on HIV/AIDS treatment. The final section concludes on the main findings.

Why HIV/AIDS Matters: Human and Economic Costs

Why study the effect of political regimes on health care policies relating to treatment of HIV/AIDS? First of all, we know very little about whether democratic and non-democratic governments respond differently to the HIV/AIDS epidemics. Secondly, in the last couple of decades, HIV/AIDS has evolved into a global pandemic with disastrous human and economic consequences. Despite the human and economic costs, economists and political scientists have paid only scant attention to the issue. To get a sense of the scale and impact of HIV/AIDS, a few stylized, albeit rather harsh, statistics will do. The most recent numbers show that as of 2007, around 33 million people were infected with HIV/AIDS worldwide, of which two million were children under the age of 15, and there continues to be around three million new infections annually, more than 10 percent of which are children (UNAIDS 2008: 32-33). In terms of mortality, around two million people died of AIDS in 2007 alone, while the total death toll of the disease amounts to 25 million people since the first outbreak was discovered less than 30 year ago (UNAIDS 2008: 31-32). In comparison, deaths caused by civil war – a widely studied subject in social science (Fearon and Laitin 2003) – amounts to somewhere between 3.5 and 5.2 million for the entire period from 1946 to 2005.¹

While the numbers are staggering, the impact of both infections and mortality hits disproportionately across continents, with sub-Saharan Africa being most severely affected: Around two thirds of all recorded HIV/AIDS cases are in sub-Saharan Africa, amounting to more than 22 million people in that part of the world alone (Barnett and Whiteside 2006: 10; UNAIDS 2008: 39). Indeed, in the most severely affected countries such as Botswana and Lesotho, more than one third of the adult population is infected with HIV/AIDS (Patterson 2006: 5). The tragedy of HIV/AIDS is not confined to Africa however. Both Latin American and Asia are severely affected by the disease, and by 2020 Asia is expected to be the epicentre of HIV/AIDS (Barnett and Whiteside 2006: 9). HIV infections and deaths due to AIDS means that life expectancy has declined and mortality rates increased rapidly in many countries in the past couple of decades (Jamison et al. 2001; Barnett and Whiteside 2006: 297-299). Apart from the obvious human suffering this has caused, the economic

¹ These are estimates of so-called battle deaths (soldiers plus civilians) and are based on the Uppsala/PRIO *Battle Deaths Dataset 1946-2005, version 2.0* (Lacina and Gleditsch 2005). The numbers are calculated based on the ‘best estimate’ of the variables ‘Internal armed conflict’ (3.5 million) and ‘Internal armed conflict’ plus ‘Internationalized internal armed conflict’ (5.2 million).

costs are substantial at both the micro and macro level (Mills and Shillcutt 2006: 28-29; Sachs 2005: 200-201). At the micro-level, the supply of labour decreases when people fall ill or die of the disease, and the productivity of infected individuals who do participate on labour markets decreases too. In addition, individuals who do not expect to live long because of high domestic mortality rates and a high risk of early death, develop short time-horizons, which means that they will be less inclined to engage in activities, such as savings, investments and education, that are costly in the short term but yield long-term payoffs (Lorentzen et al. 2008: 83). All these things in combination imply that HIV/AIDS has substantial negative effects on economic development and GDP growth at the macro level (Barnett and Whiteside 2006: 290; Dixon et al. 2001; Jamison et al. 2001; Mills and Shillcutt 2006; Sachs 2005: 200-201). Jamison et al. (2001), for instance, estimate that in Africa, the AIDS-related decrease in income growth amounts to around 1.7 percentage points annually on average, with even larger effects in heavily affected countries. Thus, in many parts of the developing world today, HIV/AIDS contributes significantly to keep countries trapped in poverty by increasing mortality and lowering life expectancy, investment, and human capital accumulation, with decreasing economic growth and prosperity as a consequence. What started mainly as a health issue has turned into a major issue of economic development.

While HIV/AIDS causes severe human and economic distress and while there is no known cure, the disease can be treated by use of so-called anti-retroviral (ARV) medicine. HIV (Human Immunodeficiency Virus) is a virus that causes the human immune system to weaken and eventually fail, ultimately leading to AIDS (Acquired Immunodeficiency Syndrome). HIV therefore destroys the body's immune system and its ability to combat even rather trivial diseases. Anti-retroviral drugs work by suppressing the development of HIV in the human organism and restoring the immune systems of patients (Epstein 2007: 266). Crucially, ARV treatment may also prevent the transmission of HIV from mother to child, and thereby reduce avoidable infections of children (Barnett and Whiteside 2006: 42; UNAIDS 2008: 121). The ARV drugs do not cure HIV/AIDS, but they do increase survival rates and life expectancy considerably for the treated individuals (Barnett and Whiteside 2006: 46-47; UNAIDS 2008: 131-134). By implication, ARV treatment may alleviate some of the social and economic consequences of HIV/AIDS. However, despite a dramatic decline in the price of ARV treatment in the last decade (Barnett and Whiteside 2006: 48), there is still enormous variation in the extent to which infected individuals are treated across countries, particularly for the group of developing countries. What this paper asks is basically: Why is there so much variation in the health services and medical treatment available to HIV/AIDS patients? And in

particular, how do political regimes shape public health policy and access to ARV treatment? At a more general level, we therefore need to ask how political regimes affect public policies and why we might, in theory, expect democracies to perform differently from non-democracies in terms of delivering public services to citizens.

Political Regimes and Public Policy

At the heart of debates about the economic effects of political regimes is the question of whether democratic and non-democratic governments behave differently in terms of providing access to public services such as education and health care (Baum and Lake 2003; Bueno de Mesquita et al. 2003; Lake and Baum 2001; Mulligan et al. 2004; Ross 2006; Stasavage 2005). On theoretical grounds, the issue concerns why we might expect a democratically elected government to perform differently from a non-democratic one.

Recent developments in political economy have increasingly turned towards studying how the constraints and incentives facing democratically elected governments may alter their performance relative to non-democratic governments. At its simplest, democracy is a game between two opposing political candidates who compete for votes in order to achieve office in an environment with extended (or universal) suffrage. There are obviously many contingencies – for instance, the number of parties, electoral systems, and systems of government – that affect how democratic politics operate and how any particular democratic regime performs (Persson and Tabellini 2003). Nevertheless, all democracies share one fundamental feature that distinguishes them from non-democratic regimes: In democracies, governments and legislatures are selected by voters in a process of competitive elections (Przeworski et al. 2000). Even such a minimalist and procedural account of democracy entails that voter participation in the selection of political leaders is broad-based and relatively equal. It also implies that political leaders, parties, and candidates operate in competitive political markets with low barriers to entry and low costs of exit (Acemoglu and Robinson 2001, 2006; Baum and Lake 2003; Lake and Baum 2001). Non-democracy – or dictatorship – is then simply a residual category in which voter participation is restricted and party competition is repressed.

It is precisely the mechanism of contested elections that is supposed to explain why public policies differ between democratic and non-democratic regimes. In contemporary models of

political economy, the key feature of democratic elections is that they make politicians and governments accountable to a broad coalition of voters (Adserá et al. 2003; Barro 1973; Besley 2006; Ferejohn 1986). For voters, elections are a means by which they can hold the government to account for its performance and oust it from office if it does not deliver policies that satisfy their demands. Given that democratic governments are at least partially motivated by re-election concerns, they therefore need to be responsive to voter demands (Besley 2006: 101). By implication, competitive elections create a distinct set of incentive for governments to accommodate to the demands of a broad coalition of voters.

By contrast, in a dictatorship, rulers face no electoral threat and failure to deliver policies and public goods that benefit the population at large are much less likely to lead to dethronement from power. Firstly, whereas voting a democratic government out of office is a low-cost activity for voters, removing an unpopular dictator may come at a high cost, because it often involves engaging in a violent revolution or *coup d'état*. To depose an unpopular dictator, citizens would secondly need to solve the collective actions problems inherent in any large-scale uprising. And thirdly, all dictators have strong personal incentives to use any means at their disposal – including repression of opposition parties and independent media – to cling on to power, because the cost of exit from office is much higher than in democracies, and often involves a life in exile, prosecution, or even execution (Lake and Baum 2001: 594; Tullock 1987; Wintrobe 1998). In this perspective, it is no wonder that dictators last in office for much longer than democratically elected governments (Bueno de Mesquita et al. 2003: 5).

While electoral competition is supposed to induce democratic governments to respond to voters demands, it may also affect the incentives of governments to adjust the supply of public and private goods as instruments for building and sustaining support among the electorate. In the simple Downsian model of one-dimensional two-party competition, the supporting coalition of any government constitutes a majority centred on the median as the pivotal voter. To stay in power, democratic governments therefore need to provide policies and services that benefit a majority coalition in the electorate (Acemoglu and Robinson 2001, 2006; Bueno de Mesquita et al. 2003; McGuire and Olson 1996; Olson 1993). In this sense, democratic governments differ from non-democratic rulers who typically do not rely on civilian majority coalitions, but instead need the support of powerful elites to stay in office. The mere fact that the size of the supporting coalition is larger in democratic regimes means that governments face incentives to redistribute more resources

to the provision of (quasi) public goods and broad-based services as a means to stay in power. On the other hand, the use of rents and selectively targeted private goods becomes a relatively less attractive instrument for attracting voter support, simply because such goods have to be divided among a much larger number of individuals, diminishing their *per capita* value (Bueno de Mesquita et al. 2003). This does not imply that non-democratic governments are not accountable or responsive to a supporting coalition. Indeed, no dictator can survive in office without the support of a significant and powerful group in society (Tullock 1987; Wintrobe 1998). However, in dictatorships, not only is the supporting coalition numerically smaller – that is, a minority comprising political and economic elites rather than a majority of middle-class and low-income groups – it is also often backed by powerful groups with a comparative advantage in violence, such as the military or militia groups. To survive in office and avoid suffering the costs of exit, any dictator needs first and foremost to retain the support of the pivotal political, economic, and military elite. The rational way of doing so is to supply government services on a selective basis to powerful elites. A non-democratic government is therefore more likely to target its policies and spending decisions towards establishing selective benefits and rents that has enough value for members of the ruling coalition to uphold their support to the incumbent.

Political Regimes and HIV/AIDS Treatment

If there is validity to the claim that elections make governments accountable to voters and establish incentives to supply larger quantities of public goods, this has specific implications for the provision of health care services in general. The effect of a move from a non-democratic polity to a system with competitive elections and universal suffrage should work in the same way as for most other types of redistributive policies. Specifically, the median voter in a democracy is likely to favour redistribution from high-income to middle-income groups, because the income of the median voter is practically always below the average income in the electorate (Acemoglu and Robinson 2001, 2006). Redistribution from high-income groups therefore yields net benefits to the median voter. By a similar logic, democracies are likely to redistribute more resources to health care than dictatorships. This does not necessarily imply that redistributive policies in democracies benefit low-income or poor groups in society (Ross 2006). Rather, the implication is that health care is made available to more encompassing groups than in comparable dictatorships, with the main

beneficiary of democratic redistribution being the middle class, represented by the stylized median voter.

In so far as governments respond to HIV/AIDS outbreaks in the same manner as they are expected to respond to demands for health care in general, democratic governments should provide ARV treatment as a quasi-public good, with relatively extensive coverage for those affected by the disease. In contrast, a non-democratic government can target access to ARV treatment to its supporters, party members, and high-ranking officials without fear of electoral repercussion. This becomes particularly relevant once we consider that in some countries, mainly in the developing world, the HIV/AIDS epidemic has reached a scale where it has quite severe effects on government operations, government staff and supporters (Barnett and Whiteside 2006: 319-326). Quite literally, HIV/AIDS may affect the dictators nearest allies and members of his supporting coalition. However, because an autocratic government does not face an electorate in competitive elections, it can respond to such events by targeting medical treatment, including ARV drugs, at elites, rather than making treatment part of a broad-based and general health care program to combat HIV/AIDS in society at large. On this background, one might hypothesize that democratic governments will provide more extensive coverage of ARV treatment for victims of HIV/AIDS.

However, government responses to HIV/AIDS are not necessarily similar to responses towards demands for general health care services. While the logic of democratic politics may work for health care services in general, it does not immediately follow that it applies to government responses to HIV/AIDS too. This is so for at least two reasons. Firstly, some arguments emphasise that 'developmental dictatorships' with a strong and autonomous political leadership insulated from electoral pressures is necessary to implement efficient policies which may lack popular support (Haggard 1990; Wade 1990, Wintrobe 1998: 247-49). Indeed, one of the key features of dictatorial government is its capability to respond rapidly to emerging crises – including health crises involving major diseases like HIV/AIDS – because non-democratic governments do not face the institutional and electoral constraints of a democratic government. Therefore, dictators may respond more effectively and rapidly to emerging HIV/AIDS crisis than their democratic counterparts. The developmental dictatorship argument does, however, rest on a relatively narrow set of assumptions, implying that autocrats have long-term planning horizons and do not face credible threats to their tenure in office (McGuire and Olson 1996; Olson 1993), or that autocrats are motivated by outright benevolence (Wade 1990). In so far as these assumptions are violated, there

is little reason to believe that dictatorships will outperform democracies in terms of their responses to humanitarian and health crises. Secondly, even if dictators fail to respond the HIV/AIDS crisis, it does not follow that a democratic government will do so. In many countries, HIV/AIDS has disproportionately affected small and sometimes marginalized groups, e.g. homosexuals (Iqbal and Zorn 2010: 153). Given that the numerical size of groups affected by HIV/AIDS is often small, this may limit the incentives of even a democratic government to respond their demands. However, particularly in countries in Africa and some in Asia, the victims of HIV/AIDS are not exclusively small and marginalized groups, but constitute relatively large (voter) groups (UNAIDS 2008). In addition, even if a democratic government is less responsive to the demands of small voter groups relative to large ones, it does not follow that a non-democratic government will be better at serving the interests of HIV/AIDS victims or will be any more responsive to their demands, e.g. by providing access to ARV treatment. Overall, while there is reason to believe that government health services *in general* differ across political regimes (Avelino et al. 2001; Baum and Lake 2003; Kaufman and Segura-Ubiergo 2001; Lake and Baum 2001), this does not necessarily imply that HIV/AIDS policies will vary across political regimes too. However, analyzing HIV/AIDS policies constitutes a strong test of potential regime effects. If democracies do provide more extensive access to treatment than non-democracies – even on an issue like HIV/AIDS that generally does not affect a majority in the electorate – this would constitute relatively strong evidence that political regimes do matter for public health policy.

Methods and Data

To test whether there is a positive effect of democracy on the degree to which HIV/AIDS patients have access to treatment by anti-retroviral (ARV) medicine, the empirical analysis rely on a cross-country dataset with up to 130 developed and developing countries. The analyses are conducted using regressions and matching methods. The data for ARV treatment are from the World Health Organisation's (WHO) *World Health Statistics* and covers the years from 2004 to 2007 (cf. WHO 2009). Since data on ARV treatment is not available before 2004 and since there is very little time-series variation in political regimes in that period, the use of panel data is essentially precluded.

Dependent Variable

As dependent variable I use a measure of the percentage of people (adults and children) with advanced HIV that are being treated with anti-retroviral therapy, involving a cocktail of three or more ARV drugs (WHO 2009: 73).² While the indicator does not say anything about the quality of the treatment, it does provide a measure of the proportion of HIV infected people who are receiving treatment, and as such it works as an indicator for the coverage and scope of medical treatment of HIV/AIDS. As the theory of the effect of democracy on the provision of government services speak directly to the scale and quantity of services, rather than their quality, using the coverage of ARV treatment as dependent variable should constitute a proper test of the hypothesis linking political regimes to health care policies. The ARV variable varies from 0-100 and is calculated as the average of the years 2004-2007, partly to alleviate measurement error and partly get a measure of government policies over time rather than in a single year. However, data availability for ARV treatment varies quite substantially across countries for the years 2004-2007, with 2004 and 2005 having the greatest coverage with 126 and 155 countries, respectively, while 2006 and 2007 have the lowest with 103 countries. This is because the WHO collected data for high-income countries in 2004 and 2005, but not in the other years. For countries such as the USA, UK, Germany, Denmark, France and most other high-income countries, data is not available for the years 2006 and 2007. For countries that do not have complete data from 2004-2007, I therefore use data from the most recent year available.³ Most of the high-income countries are coded by the WHO in 2004 and 2005 as having an ARV coverage rate that is '>75', i.e. more than 75 percent of HIV victims are treated with ARV therapy (WHO 2005, 2006). In reality, the coverage in most of these countries is probably closer to 100 percent, but to err on the side of caution, they have been coded with the value 75 in the data.

² By far the most effective ARV treatment involves the use of three or more drugs in combination (Barnett and Whiteside 2006: 46-27).

³ As the reports on *World Health Statistics* for 2007 and 2008 both contain data for 2006, I use the most recent estimates in the 2008 report.

Independent Variables

To measure regime type, two widely used variables are employed: These are the Freedom House (2005) measure of *political rights* and the measure of *electoral contestation* developed by Przeworski et al. (2000).⁴ According to Freedom House (2005), political rights essentially concerns three features: The extent to which governments and legislatures are selected through free and fair competitive elections; the extent to which voters are allowed to participate in elections, with universal adult suffrage being the hallmark of democracy; and the extent to which elected governments operate in an open and transparent matter (e.g. by allowing media scrutiny) and make policy decisions without distortions caused by military intervention or extensive corruption.⁵ While this measure is relatively comprehensive, it nevertheless tries to assess two key features of electoral democracy – participation and competition – and as such works as a reasonable proxy for those procedural aspects of political regimes. In the OLS regression, the political rights enter with its average value for the period 1990-2003 (rather than a single-year point estimate), since the effects of democracy may only materialize in the longer term. The original 7-point scale is transformed into an index from 0 to 1, with high values indicating more extensive political rights. For the purposes of matching, however, the political rights index has been transformed into a binary variable, where countries with average values between 1 and 3 on the original index for the period 1990-2003 are coded as democracies. This corresponds to countries categorised by Freedom House (2005) as being ‘Free’. All other countries are coded as non-democracies. The second democracy measure – labelled ACPL – is developed by Przeworski et al. (2000) and updated by Cheibub and Gandhi (2004). Unlike the Freedom House measure, which is based on subjective assessments, the ACPL democracy measure can be replicated with objective data. The ACPL variable focuses on electoral

⁴ An often used alternative is the polity indicator of democracy. However, unlike the Freedom House and ACPL indicators, the polity variable includes a component measuring ‘executive constraints’, which conceptually has more to do with the presence of veto players in the political system, and not electoral participation and competition *per se*. As noted by Norris (2008: 61-62), this makes the polity index more similar to a measure of constitutional democracy rather than competitive or participatory democracy. Since this study does not deal power sharing, the polity variable is not used.

⁵ I do not use the civil liberties component from Freedom House because it includes issues like freedom of speech, personal rights, and the rule of law that are not narrowly related to electoral democracy.

contestation, and seeks to distinguish regimes where legislatures and executives are selected through competitive elections from regimes where competition is restricted (Przeworski et al. 2000: 15). The ACPL variable classifies regimes according to four features of the polity (Przeworski et al. 2000: 19-28; Norris 2008: 66): The executive must be elected; the lower house of the legislature must be elected; at least two parties must compete in elections; and, crucially, government power must have been proven to alternate between different parties following elections. This last condition implies that even if an incumbent is elected for office, but has never lost an election, the regime is classified as non-democratic, because government power has never alternated between different parties. On this basis, Przeworski et al. (2000) create a dichotomous variable, where political regimes are classified as either democratic (1) or autocracies (0), with nothing in between. Although contestation is surely an integral part of electoral democracy, Przeworski et al. (2000) neglect the other key component of democracy – participation – in the construction of their measure (Norris 2008: 66-67). That is, they do not consider whether elections are held in conditions of universal suffrage or not. Combined with the binary coding of regimes, this makes the ACPL variable a quite narrow and crude indicator of electoral democracy. In the spirit of Przeworski et al. (2000), the ACPL variable is coded as a binary indicator that is used for both OLS and matching. All countries with an average value of 0.9 or higher for the period 1990-2002 (the latest year for which data is available) are coded as democracies, while the remainder are coded as non-democracies. This definition of democracy is quite restrictive in the sense that countries must have been democratic for most of the period in order to qualify as democracies. Countries where elections are rigged (political rights) or where election results never results in alternation of parties in government (ACPL) are therefore not considered democracies, even though they may formally hold elections.

Control Variables for Regressions Analyses

Given the dearth of previous quantitative studies of treatment of HIV/AIDS, there is little empirical knowledge to lean on in the selection of control variables. The most obvious control variable is *GDP per capita* as a measure of economic wealth. Although the price of ARV medicine has declines rapidly in the past decade, the drugs are still comparatively expensive (Barnett and Whiteside 2006: 48). Therefore, wealthier countries are likely to have more extensive ARV coverage than poorer countries. Data on GDP are from the Penn World Tables 6.1 (Summers et al. 2004). A measure of (gross) *secondary enrolment* is included to account for the possibility that

higher education in the population may increase information about HIV/AIDS treatment, implying that demand and popular pressure for better and more extensive health care may increase too. Data is from the World Bank's (2009) World Development Indicators. *Government spending* on health care, measured in per capita terms, enters too, to account for public funding allocated to the health sector. These data are obtained from the World Health Statistics (WHO 2006, 2008, 2009a, 2009c). In addition, *corruption* may imply that resources and access to services are diverted away from their intended use and distributed unequally, based on favours, bribes and political privilege, rather than need (Gizelis 2009: 127; Patterson 2006: 22). As a proxy for corruption, I use the corruption measure provided by the World Bank's Governance Indicators (Kaufmann et al. 2009). This variable attempts to measure the extent of corruption – both in small and large form – defined as the pursuit of private ends using public power. The index of *ethnic fractionalization* from Alesina et al. (2003) is included to account for the possibility that ethnically diverse groups may find it difficult to agree on the provision of public goods and broad-based public policies (Easterly and Levine 1997). The actual *prevalence of HIV* in the population may also affect the availability of treatment. For instance, if HIV is a widespread disease in society, demand for treatment may increase. Data on HIV infection rates is from the World Health Statistics (WHO 2006). The availability of HIV/AIDS treatment in developing countries may also be increased by inflows of *foreign aid*, in so far as aid is allocated to health care services (Patterson 2006: 131; Sachs 2005: 206-207, 2008: 318-319). Since foreign aid may only work in the relatively long term, I use an average of foreign aid as percentage of GNI over a ten-year period from 1994-2003. Data is from the Global Development Network Growth Database. Finally, I include a dummy variable indicating whether countries' have been subject to European colonization, and three regional dummy variables for sub-Saharan Africa, South East Asia, and the Middle East and North Africa (MENA).⁶ Further details on the variables and their measurement are available in the appendix.

Regression Estimates

Results from a series of regressions with ARV treatment as dependent variable are displayed in Table 1. The estimates in the odd numbered columns are obtained using OLS regression with the

⁶ Data on colonies are from Grier (1999) and data on regions are from Pippa Norris' *Democracy Time Series Dataset*,

January 2009 release, available at <http://www.hks.harvard.edu/fs/pnorris/Data/Data.htm>

two democracy measures as the key independent variables. To test the robustness of the results to the potential effect of outlying observations, the OLS regressions are replicated in the evenly numbered columns using a robust regression technique, which downweights the impact of outlying observations.⁷

[Table 1 about here]

Columns 1-4 show a series of basic models, in which only a limited set of controls is included. In models 5-6 and 9-10, the measures of corruption and HIV prevalence are added. Finally, foreign aid is included in models 7-8 and 11-12, implying that the sample is reduced to developing countries alone. In both the base models and the models including additional controls, the results for the regime variables are fairly similar. The results are also fairly similar across estimation method, although the significance levels tend to increase somewhat when outliers are downweighted in the robust regressions.

In all specifications, democracy correlates positively with ARV treatment, and all estimates are significant at conventional levels. The Freedom House measure of political rights has the strongest and most significant impact on coverage by ARV treatment, whereas the dichotomous democracy measure of Przeworski et al. (2000) is slightly less significant. However, the effect of political regimes tends to become weaker in models 6-7 and 11-12 where the sample is reduced to developing countries in isolation. While these results provide initial support for the hypothesis that democratic governments tend to provide more extensive coverage of ARV treatment for victims of HIV/AIDS than their non-democratic counterparts, they also provide reason for caution, given that the effect appear to weaken for developing countries. This is also illustrated by the magnitude of the estimates, which suggests that the impact of democracy may be anything from rather marginal to quite substantial. Since all regime variables are measured on a scale from 0 to 1, the coefficients show the effect on ARV treatment of moving from a pure dictatorship to a full-scale democracy. The estimates from the political rights variable suggest that such a move results in an increase in

⁷ See Lundström (2005) for a brief description of this type of robust regression. Robust regression is implemented in Stata 10 using the *rreg* command.

coverage of ARV treatment of roughly 21-29 percentage points. While this is a quite large effect, the results based on the ACPL variable are more conservative and suggests that the magnitude of the effect, while being significantly positive, amounts to an 8-16 percentage point increase in medical coverage, all else equal. These estimates of the substantial effect of democracy therefore differ quite a lot, depending on the metric of democracy that is used. Nonetheless, in all cases, the results suggest that there may well be an impact of political regimes on ARV treatment in the hypothesised direction, and that, if anything, the effect tends to be positive. That is, the electoral constraints facing democratic governments do at first glance seem to provide incentives to provide more extensive medical treatment for victims of HIV/AIDS than is the case in non-democracies.

In terms the control variables, higher levels of per capita GDP is associated with increased access to ARV treatment, which is unsurprising. Note, however, that the effect of wealth generally weakens once we reduce the sample to developing countries (models 7-8 and 11-12), and in model 12, the effect even becomes statistically insignificant. This indicates that wealth by itself does not necessarily increase access to medical treatment of HIV/AIDS victims. It is much more surprising that government health expenditure per capita is uncorrelated with ARV-treatment in all but two models, which suggests that large government budgets for health care alone is not sufficient to increase access to ARV treatment. In fact, all the remaining control variables are not robustly related to variations in access to ARV treatment. A few variables – such as corruption and the MENA dummy – are statistically significant in some specifications, but these results are quite weak and generally not very robust. Somewhat surprisingly, this suggests that neither education nor foreign aid has any notable impact on the coverage of ARV treatment. This means that only two variables – political regimes and GDP – appear to have a reasonably consistent effect on ARV treatment across the different models specifications.

Beyond OLS: Matching Methods

While linear regression is a useful starting point, OLS estimates do not necessarily establish causal relationships. The usual way around this problem is to search for a valid instrumental variable (IV), but since good instruments for democracy are very difficult to find (and defend), we need to look for alternatives to IV regressions, which similarly offer the promise of improving upon causal inference. Matching methods constitute a way to do so, not least because with matching, inference

is not based on extrapolations across very dissimilar cases, but rather on comparisons of the most similar cases.

The basic idea of matching is to approximate the estimation of causal effects in the same way as in a controlled experiment, but in a framework where we are confined to rely on observational rather than experimental data. At its simplest, the point of matching is to find (pairs of) cases which are similar on all observable (and unobservable) characteristics except that one group possesses a particular feature, $D=1$, that is absent in the other, $D=0$, and then compare the average outcome of interest between the groups. Differences in outcomes are then attributed to differences in the feature D . In this way, matching is a quantitative version of the ‘most similar’ research design well-known from political science (Sekhon 2008: 282).

Compared to OLS, matching has at least three advantages. Firstly, matching relaxes the linearity assumption that is an integral part of OLS and similar (IV) regression-based methods (Persson and Tabellini 2003: 136-138). In fact, matching methods do not presuppose any particular functional form, and as such they do not rely on assumptions that are as strong as in the case of OLS and IV regressions. Secondly, the central idea of matching is to draw inference from ‘local’ comparisons of cases (Cameron and Trivedi 2005: 871; Persson and Tabellini 2003: 139). That is, by comparing cases to close matches, which are reasonably equivalent in terms of observed characteristics (e.g. income levels), inference is based only on comparisons of similar cases. Thereby we avoid extrapolating across the entire sample, e.g. from rich to poor countries, and we avoid basing inference on ‘extreme’ counterfactuals that are far away from what we actually observe (King and Zeng 2006; Persson and Tabellini 2003: 137-138). Rather, the construction of counterfactuals for estimating causal effects is based on comparison of comparable cases. Finally, in contrast to OLS, matching turns attention away from modelling the direct causes of the outcome (‘dependent’) variable, and towards modelling treatment assignment – something we often know more about. For instance, we have considerably better theoretical models of the determinants of democracy – the causal variable – than the processes determining variations in the medical treatment of HIV/AIDS patients – the outcome variable. Hence, with matching we attempt to model the causes of the causal variable, rather than the causes of the outcome variable.

Matching Preliminaries and Estimation Strategy

In order to obtain the actual matching estimates, we need to go through a number of preliminary steps. An integral part of matching involves the use of a proper research design, particularly with regards to case selection. Since the idea of matching is to maximize homogeneity between the treatment and control groups, we cannot include treatment cases for which there are no comparable matches in the control group. For every observed value of X , we therefore need to have cases in both the treatment and control groups (Cameron and Trivedi 2005: 871; Morgan and Winship 2007: 117). That is, there must be some common support between treated and non-treated cases on the set of covariates in X . Otherwise, no good – that is, similar – matches exist. By implication, observations that are outside the region of common support are inappropriate for estimating causal effects, precisely because they have no comparable matches. Such cases are therefore often dropped (Persson and Tabellini 2003: 143, 2008: 563; Sekhon 2009: 495-96). Importantly, dropping observations outside common support may actually reduce bias and make the treatment and control cases more equivalent, whereby unit homogeneity is increased (Sekhon 2009: 496).

In cross-country analyses of political regimes, the common support assumption constitutes a particularly severe problem since democratic countries are most frequently found at high levels of economic development, e.g. in Western Europe and North America, whereas non-democracies are mostly found at lower income levels. Practically all high-income countries belong to the treatment group, and they have no close matches in the control group. And as noted by Przeworski (2007: 160) “We would not want to match the wealthy treatment cases with controls from poor countries”. The point of matching is precisely to match treated cases to similar controls, and disregard information from dissimilar cases in order to avoid drawing inference from incomparable cases. To increase equivalence between treatments and controls, all rich, developed nations are therefore *a priori* excluded from further analysis.⁸ Obviously, this means that the sample of countries consists of low and middle-income countries, and that any treatment effects are valid only for this group. By implication, covariate balance should be improved, i.e. the distributions of covariates should become more equivalent in the treatment and control groups.

Having done so, the matching procedure would ideally aim to compare remaining democratic countries with non-democratic countries with exactly equivalent values on the set of

⁸ Developed nations are defined as the countries of Western Europe, North America, Australia, New Zealand, and Japan.

covariates, X . However, exact matching is practically impossible, particularly if the set of covariates in X is large and some of the variables are continuous (Guo and Fraser 2009: 132; Ho et al. 2007: 212; Sekhon 2009: 497). In a seminal paper, Rosenbaum and Rubin (1983) showed that this problem may be significantly alleviated by matching cases with similar propensity scores. The results of Rosenbaum and Rubin (1983) imply that matching cases based on their probability of being selected into treatment, conditional on X , corresponds to matching countries with equivalent values of X . In practice, this approach involves estimating a propensity score model – e.g. a logit model – with treatment status as dependent variable and the covariates in X as explanatory variables. The major benefit of this approach is that countries can then be matched according to a uni-dimensional metric – the predicted propensity score (Guo and Fraser 2009: 132; Rosenbaum and Rubin 1983). However, while propensity score matching is intuitively appealing, it does have some drawbacks. In particular, most studies using propensity scores are not just interested in estimating causal effects, but also in assessing the statistical significance of their estimates. Most applied research do this by constructing standard errors using bootstrapping (e.g. Persson and Tabellini 2003; Imai 2005). However, in a matching context, Abadie and Imbens (2006a) have shown that bootstrapping generates biased variance estimates, which are therefore invalid for purposes of significance testing (cf. Guo and Fraser 2009: 215-16).

The most widely used alternative to matching on propensity score is matching based on some distance metric, for instance Mahalanobis distances (Abadie et al. 2004; Guo and Fraser 2009: 145-46; Sekhon 2008: 278, 2009: 497). Based on the distance algorithm, cases are typically matched to their ‘nearest neighbour(s)’. However, this also comes at a cost, at least if continuous variables are used. Abadie and Imbens (2006b) have shown that when continuous variables enter the distance calculations, matching estimators become biased. To correct this, Abadie et al. (2004) have developed an alternative estimator – where matching still relies on a distance measure – that corrects the bias and enables calculation of robust standard errors.

In what follows, I use a combination of propensity scores and matching based on distance metrics. Specifically, subsequent to dropping developed nations, the second step of the analysis consists of using propensity scores to ‘preprocess’ the data further, the ultimate aim of which is to increase covariate balance between the treatment and control groups (cf. Ho et al. 2007). Specifically, I estimate a series of logit regressions – with the binary democracy indicators as dependent variables – the sole aim of which is to identify the region of common support. The set of

covariates used in the propensity score models does not completely overlap with the control variables in the OLS regressions. First of all, this is because matching entails modelling selection into treatment, i.e. the determinants of democracy, rather than the direct causes of ARV-treatment, as with OLS regression. Secondly, at least some of the control variables – education, health care spending, corruption, and foreign aid – from the OLS regression are partly consequences rather than causes of democracy. These variables are therefore not used for matching. While the presence of these variables in OLS regressions may also artificially reduce the estimated effect of political regimes, including them in a model of the direct causes of democracy appears particularly problematic and may result in post-treatment bias (cf. King and Zeng 2006: 147-48; Sekhon 2009: 489). Therefore, I restrict the set of covariates in the democracy models to variables which may generally be considered exogenous to democracy and which are less likely to be affected by political regimes.⁹

A total of eight variables are included in the logit and matching models. Based on the argument of modernization theory, the most important variable is GDP per capita, with the expectation that increasing levels of wealth raise the propensity of countries to establish democracy. Although GDP is not necessarily exogenous to democracy, because countries' level of wealth may in part be affected by the type of political regime (Persson and Tabellini 2008), it is included since most studies find that income is one of the main determinants of democracy (e.g., Boix and Stokes 2003). An additional advantage is that GDP is highly collinear with other standard development indicators such as education and life expectancy, and therefore to a large extent captures the effects of these variables too. GDP per capita (log) is measured in 1990 and is from the Penn World Tables 6.1 (Summers et al. 2004). The variables measuring ethnic fractionalization, former colonial status, and the three regional dummy variables are also included. To account for potential cultural trajectories of democracy, two variables measuring the proportion of Christians and Muslims, respectively, in society as of 1970, are also included (cf. Boix and Stokes 2003).¹⁰ Estimated propensity scores are obtained based on these variables, and are used to determine the region of common support, variously defined. Detailed results from the logit regressions from which propensity scores are obtained are available in the appendix. Countries that fall outside common

⁹ Since the treatment variable in the matching estimations is the binary democracy indicators, and since these are measured over the period 1990-2002/03, all covariates on which matching is based are measured prior to 1990.

¹⁰ Data are retrieved from Robert Barro's webpage at http://www.economics.harvard.edu/faculty/barro/data_sets_barro

support – variously defined – are then dropped from the analyses on the grounds that they have no comparable matches on the set of covariates (Persson and Tabellini 2003: 143, 2008: 563; Sekhon 2009: 495-496).

The third step of the analysis consists in testing whether the balancing property is satisfied, i.e. whether the distribution of covariates for the democracies and non-democracies that are on common support is (roughly) equivalent. This is tested using simple *t*-tests for comparison of group means. The results of the balance tests are shown in Table 2.

[Table 2 about here]

In Table 2, I follow the approach of Persson and Tabellini (2003: 148-49) and show results from equal-means *t*-tests for both the full sample and for countries within selected strata of the propensity score. It is clear from column 1 that for the full sample, covariate balance is only achieved for two variables – the dummy variables for colonialism and South East Asia – whereas equal means are rejected for the remainder. However, in matching we want to compare countries with similar covariate distributions (or propensity scores), and therefore it makes more sense to test balance when the data is stratified based on the propensity score. This is done in columns 2–4 where countries are divided into three strata based on their propensity scores (cf. Persson and Tabellini 2003: 148). Doing so implies that the distribution of covariates achieves much better balance. In fact, equal means are never rejected at conventional significance levels within any of the strata. The balance property is therefore more likely to be satisfied, once comparison it is confined to similar countries that are likely to be matched. The balance tests do, however, reveal a potential problem with the three regional dummies, namely that they have no observations for certain categories of the propensity score strata. To check the sensitivity of the matching results to the inclusion of the regional dummies, estimates are obtained from matching procedures that both includes and excludes the regional dummies.

Matching Estimates

Given the matching preliminaries, we can proceed to the actual matching procedure. Various procedures have been developed for this purpose (Cameron and Trividi 2005: 874-877; Morgan and Winship 2007: 107-114). Here I use the ‘nearest-neighbour’ matching algorithm developed by Abadie et al. (2004), implemented in Stata 10 using the *nnmatch* command. To date, this algorithm appears to offer the best opportunity for obtaining consistent and unbiased estimates of (the significance of) treatment effects. In essence, nearest-neighbour matching consists in matching each treated case to its closest equivalent in the control group, i.e. the country whose distance from the treated case is smallest (Morgan and Winship 2007: 107-08). Hence, the actual matching is not based on propensity scores but on the distance between matched cases (Abadie et al. 2004). The nearest neighbour in the control group is then used to construct the counterfactual for the treated case. The main drawback of nearest-neighbour matching is that sometimes even the nearest neighbour may still be quite far away. With this caveat, the final treatment effect (for the treated) is the average of all such pairs of treatment–control comparisons on the outcome variable. Based on the nearest-neighbour procedure, the matching estimates below report the average effect of treatment on the treated (ATT) (Persson and Tabellini 2008: 549; Sekhon 2009: 496). ATT is equivalent to $\tau | (D=1) = E(y_{i,1} | D=1) - E(y_{i,0} | D=0)$. That is, the ATT shows the effect of democracy for countries that are actually democracies – relative to being in a non-democratic state – rather than for some randomly drawn country from the sample or for countries that are actually non-democracies (Morgan and Winship 2007: 42-43; Persson and Tabellini 2008: 549).¹¹ In this way, ATT estimates causal effects by asking how outcomes would differ for democracies, given that we could observe them as non-democracies.

[Table 3 about here]

The results obtained from the matching procedure are shown in Table 3. Matching is implemented using the two binary democracy variables as treatment indicators, and then matching on all covariates listed in Table 2. The outcome variable is coverage by ARV treatment, and the estimates

¹¹ The latter two effects would be the average treatment effect (ATE) and the average effect of treatment on the controls (ATC).

show the average effect of treatment on the treated (ATT). Models 1-6 use the ACPL indicator as causal variable, while models 7-12 use the political rights indicator. All estimates are obtained using the bias-adjustment procedure developed by Abadie et al. (2004), and all z-values are based on heteroskedasticity-consistent standard errors. To test the robustness of the results, common support is variously defined, as shown in the third and fourth row. Models 1-2 and 7-8 follow Persson and Tabellini (2003: 143) and define the region of common support as the interval between the minimum estimated propensity score for the treated ($D=1$) and the maximum estimated propensity score for the controls ($D=0$). Models 3-4 and 9-10 narrow the region of common support to countries with propensity scores between 0.1 and 0.9. Models 5-6 and 11-12 define support for the treated as the interval between the minimum and maximum propensity score for the controls, which should ensure that only treated cases for which there are support among the controls are matched. In addition, the table shows the number of countries on and off common support. To test the effect of including the potentially problematic regional dummy variables, the evenly numbered columns show results from replications of the matching procedure for models excluding the regional dummy variables.¹²

The matching estimates in the top part of Table 3 show a quite consistent pattern, regardless of democracy indicator and common support definition. The ATT estimate for the ACPL indicator shows that democracy has a significantly positive effect on the coverage of ARV treatment in all but one model. The estimates indicate that the average effect of democracy ranges between 14–20 percentage points, which is a somewhat larger than the OLS estimates in Table 1. The political rights indicator is also significantly positive throughout all specifications, with ATT estimates in interval of 10-17 percentage points, which is lower than the OLS estimates. Nonetheless, for both the ACPL and political rights indicators, the effect of democracy remains positive. Within the confines of the estimated treatment effects, this supports the idea that, for democratic countries, there are benefits from democracy in terms of health care treatment for HIV/AIDS victims. In other words, countries that are actually democracies seem have a significantly higher coverage of ARV treatment relative to the counterfactual of being in a state of non–democracy. These results do lend some support to the idea that democracy has some discernable effects on health care policies, even in the developing countries. However, there is also reason to be cautious about attributing too much significance to the effect of political regimes.

¹² Each set of propensity scores used to determine common support in these models is based on a logit model that also excludes the regional dummies. See the logit regressions (models 2 and 4) in the appendix for detailed results.

Firstly, about half the matching estimates achieves significance only at the 0.1 level. Secondly, at least for the political rights indicator, the magnitude of the effect based on the matching procedure – approximately a ten percentage point difference between democracies and non-democracies – is much lower than the estimated effect obtained by the regression estimates. Thus, while the estimates obtained using matching suggests that – if anything – democracy has a positive effect on access to ARV treatment, the results also show that we should be careful about attributing too much importance to political regimes.

Conclusion

HIV/AIDS has evolved into being *the* major killer in developing countries today. Indeed, it is no longer confined to being an issue of public health. Due to its devastating human and economic impact, HIV/AIDS has become a major development issue. Yet we know very little about why medical treatment of HIV/AIDS victims is provided on a large scale in only some countries, and we know practically nothing about the role of political regimes in this process. The purpose of this paper has been to fill this gap by analysing the impact of political regimes on public health care policies relating to the treatment of HIV/AIDS victims by ARV drugs. The general argument linking political regimes to government performance implies that competitive democratic elections impose an accountability constraint on governments. In theory, democratic politics creates incentives for government to accommodate to voter preferences and rely on a relative large supply of public goods and broad-based services as means to stay in power. By contrast, a non-democratic government does not face any formal accountability constraints through competitive elections. This creates incentives for such governments to rule by distributing selective benefits targeted specifically at coalition members, rather than by means of general public services available to broad segments of citizens. Using regression and matching methods, this paper has tried to assess the relative merits of democracy in the case of medical treatment of HIV/AIDS patient. Across various model specifications, the results have provided some support for the idea hypothesis that democracies tends to perform better than non-democracies in terms of providing access to treatment by anti-retroviral drugs for HIV/AIDS patients. While this suggests that political regimes do have some tangible effects on public health policies – at least in this limited, but vital, policy area – there is also reasons for caution, in particular since the substantial effect of democracy compared to non-democracy may not amount to more than a ten percent difference in ARV coverage. At the very

least, we therefore need a more detailed understanding of the conditions under which democracy works as predicted by theory – and the conditions under which it does not.

References

- Abadie, Alberto, David Drukker, Jane L. Herr, and Guido W. Imbens 2004. "Implementing matching estimators for average treatment effects in Stata." *The Stata Journal* 43: 290-311.
- Abadie, Alberto and Guido W. Imbens 2006a. "On the failure of the bootstrap for matching estimators." Working paper. Harvard University.
- Abadie, Alberto and Guido W. Imbens 2006b. "Large sample properties of matching estimators for average treatment effects." *Econometrica* 74: 235-67.
- Acemoglu, Daron, and James Robinson 2001. "A theory of democratic transitions." *American Economic Review* 91 (4): 938-63.
- Acemoglu, Daron and James Robinson 2006. *Economic Origins of Dictatorship and Democracy*. Cambridge: Cambridge University Press.
- Adserà, Alícia, Carles Boix and Mark Payne 2003. "Are you being served? Political accountability and quality of government." *Journal of Law, Economics, and Organization* 19(2): 445-490.
- Alesina, Alberto, Arnaud Devleeschauwer, William Easterly, Sergio Kurlat, and Romain Wacziarg 2003. "Fractionlization." *Journal of Economic Growth* 8: 155-194.
- Avelino, George, David S. Brown, and Wendy Hunter 2005. "The effects of capital mobility, trade openness, and democracy on social spending in Latin America, 1980-1999." *American Journal of Political Science* 49 3: 625-41.
- Barnett, Tony, and Alan Whiteside 2006. *AIDS in the Twenty-First Century: Disease and Globalization*. Basingstoke, Hampshire: Palgrave Macmillan.
- Barro, Robert J. 1973. "The control of politicians: An economic model." *Public Choice* 14(1): 19-42.
- Barro, Robert J. 1996. "Democracy and growth." *Journal of Economic Growth* 1: 1-27.
- Barro, Robert J. 2009. "Religion Adherence Data." <http://www.economics.harvard.edu/faculty/barro/>

- Baum, Matthew A. and David A. Lake 2003. "The political economy of growth: Democracy and human capital." *American Journal of Political Science* 47(2): 333-347.
- Besley, Timothy 2006. *Principled Agents? The Political Economy of Good Government*. Oxford: Oxford University Press.
- Besley, Timothy, and Masayuki Kudamatsu 2006. "Health and democracy." *American Economic Review* 96 (2): 313-318.
- Boix, Carles and Susan Stokes 2003. "Endogenous democratization." *World Politics* 55, 517-549.
- Bueno De Mesquita, Bruce, Alistair Smith, Randolph M. Siverson,, James D. Morrow 2003. *The Logic of Political Survival*. Cambridge MA: MIT Press.
- Cameron, A. Colin, and Pravin Trivedi 2005. *Microeconometrics: Methods and Applications*. Cambridge: Cambridge University Press.
- Cheibub, José Antonio, and Jennifer Gandhi 2004. "Classifying political regimes: a six-fold measure of democracies and dictatorships." Presented at the Annual Meeting of the American Political Science Association, Chicago.
- Dixon, Simon, Scott MacDonald, and Jennifer Roberts 2001. "AIDS and economic growth in Africa." *Journal of International Development* 13: 411-26.
- Easterly, William and Ross Levine 1997. "Africa's growth tragedy: Policies and ethnic divisions." *Quarterly Journal of Economics* 112(4): 1203-1250.
- Epstein, Helen 2007. *The Invisible Cure: Africa, the West, and the Fight Against AIDS*. London: Penguin Group.
- Fearon, James D., and David D. Laitin 2003. "Ethnicity, Insurgency, and Civil War." *American Political Science Review* 97 (1): 75-90.
- Ferejohn, John 1986. "Incumbent performance and electoral control." *Public Choice* 50 (1-3): 5-25.
- Freedom House 2005. "Freedom in the world: Comparative rankings 1973-2005." <http://www.freedomhouse.org/>

- Gizelis, Theodora-Ismene 2009. "Wealth alone does not buy health: Political capacity, democracy, and the spread of AIDS." *Political Geography* 28: 121-131.
- Global Development Network Growth Database. <http://www.nyu.edu/fas/institute/dri/index.html>
- Grier, Robin 1999. "Colonial legacies and economic growth." *Public Choice* 98, 317-335.
- Guo, Shenyang and Mark W. Fraser 2009. *Propensity Score Analysis: Statistical Methods and Applications*. Thousand Oaks: Sage Publications.
- Haggard, Stephan (1990): *Pathways From the Periphery: The Politics of Growth in Newly Industrializing Countries*. Ithaca: Cornell University Press.
- Ho, Daniel E., Kosuke Imai, Gary King, and Elizabeth A. Stuart 2007. "Matching as nonparametric preprocessing for reducing model dependence in parametric causal inference." *Political Analysis* 15: 199-236.
- Imai, Kosuke 2005. "Do get-out-the-vote calls reduce turnout? The importance of statistical methods for field experiments." *American Political Science Review* 992: 283-300.
- Iqbal, Zaryab, and Christopher Zorn 2010. "Violent conflict and the spread of HIV/AIDS in Africa." *Journal of Politics* 72 (1): 149-162.
- Jamison, Dean T, Jeffrey Sachs, and Jia Wang 2001. "The effect of the AIDS epidemic on economic welfare in Sub-Saharan Africa." Commission on Macroeconomics and health. Working paper series, paper no. WG1: 13.
- Kaufman, Robert R., and Alex Segura-Ubiergo 2001. "Globalization, domestic politics, and social spending in Latin America: A time-series cross section analysis, 1973-97." *World Politics* 53 4: 553-87.
- Kaufmann, Daniel, Aart Kraay, and Massimo Mastruzzi 2009. "Governance matters VIII: Aggregate and individual governance indicators 1996-2008." World Bank: Policy Research Working Paper 4978.
- King, Gary, and Langche Zeng 2006. "The dangers of extreme counterfactuals." *Political Analysis* 14: 131-59.

- Lacina, Bethany, and Nils Petter Gleditsch 2005. "Monitoring trends in global combat: A new dataset of battle deaths." *European Journal of Population* 21: 145-66.
- Lake, David A. and Matthew A. Baum 2001. "The invisible hand of democracy: Political control and the provision of public services." *Comparative Political Studies* 34(6): 587-621.
- Lorentzen, Peter, John McMillan, and Romain Wacziarg 2008. "Death and development." *Journal of Economic Growth* 13: 81-123.
- Lundström, Susanna 2005. "The effect of democracy on different categories of economic freedom." *European Journal of Political Economy* 21: 967-980.
- McGuire, Martin C., and Mancur Olson 1996. "The economics of autocracy and majority rule: The invisible hand and the use of force." *Journal of Economic Literature* XXXIV(March): 72-96.
- Mills, Anne, and Sam Shillcutt 2006. "Communicable diseases." In *How to Spend \$50 Billion to Make the World a Better Place*, ed. Bjørn Lomborg. Cambridge: Cambridge University Press.
- Morgan, Stephen L., and Christopher Winship 2007. *Counterfactuals and Causal Inference: Methods and Principles for Social Research*. Cambridge: Cambridge University Press.
- Mulligan, Casey B., Richard Gill, and Xavier Sala-i-Martin 2004. "Do Democracies Have Different Public Policies than Nondemocracies?" *Journal of Economic Perspectives* 18(1): 51-74.
- Norris, Pippa 2009. *Democracy Time Series Dataset*, January 2009
<http://www.hks.harvard.edu/fs/pnorris/Data/Data.htm> s
- Norris, Pippa 2008. *Driving Democracy. Do Power Sharing Institutions Work?* Cambridge: Cambridge University Press.
- Olson, Mancur 1993. "Dictatorship, democracy and development." *American Political Science Review* 87(3): 567-576.
- Patterson, Amy 2006. *The Politics of AIDS in Africa*. Boulder, CL: Lynne Rienner Publishers.
- Persson, Torsten, and Guido Tabellini 2003. *The Economic Effects of Constitutions*. Cambridge, MA: MIT Press.

- Persson, Torsten, and Guido Tabellini 2008. "The growth effect of democracy: Is it heterogenous and how can it be estimated?" In *Institutions and Economic Performance*, ed. Elhanan Helpman. Cambridge, MA: Harvard University Press.
- Pinto, Pablo M. and Jeffery F. Timmons 2005. "Political determinants of economic performance: Political competition and the sources of growth." *Comparative Political Studies* 381: 26-50.
- Przeworski, Adam 2007. "Is the science of comparative politics possible?" In *The Oxford Handbook of Comparative Politics*, eds. Carles Boix and Susan Stokes. Oxford: Oxford University Press.
- Przeworski, Adam and Fernando Limongi 1993. "Political regimes and economic growth." *Journal of Economic Perspectives* 73, 51-69.
- Przeworski, Adam, Michael E. Alvarez, José A. Cheibub, and Fernando Limongi 2000. *Democracy and Development: Political Institutions and Well-Being in the World 1950-1990*. Cambridge: Cambridge University Press.
- Rodrik, Dani and Romain Wacziarg 2005. "Do democratic transitions produce bad economic outcomes?" *American Economic Review* 95(2): 50-55.
- Rosenbaum, Paul R. and Donald B. Rubin 1983. "The central role of the propensity score in observational studies for causal effects." *Biometrika* 701: 41-55.
- Ross, Michael 2006. "Is democracy good for the poor?" *American Journal of Political Science* 50 (4): 860-74.
- Sachs, Jeffrey 2005. *The End of Poverty: How we can make it happen in our lifetime*. London: The Penguin Press.
- Sekhon, Jasjeet S. 2008. "The Neuman-Rubin model of causal inference and estimation via matching methods." In *The Oxford Handbook of Political Methodology*, eds. Janet M. Box-Steffensmeier, Henry Brady, and David Collier. Oxford: Oxford University Press.
- Sekhon, Jasjeet S. 2009. "Opiates for the matches: Matching methods for causal inference." *Annual Review of Political Science* 12: 487-508.

- Stasavage, David 2005. "Democracy and education spending in Africa." *American Journal of Political Science* 49 2: 343-58.
- Summers, Robert, Alan. Heston, and Bettina Aten 2004. "Penn World Tables, Mark 6.1." University of Pennsylvania: Center for International Comparisons.
- Tavares, José and Romain Wacziarg 2001. "How democracy affects growth." *European Economic Review* 45: 1341-1378.
- Tullock, Gordon (1987). *Autocracy*. Dordrecht: Kluwer Academic Publishers.
- UNAIDS 2008a. *Report on the Global AIDS Epidemic*. Geneva: Joint United Nations Programme on HIV/AIDS.
- Wade, Robert (1990). *Governing the Market: Economic Theory and the Role of Government in East Asian Industrialization*. 2003 paperback edition. New Jersey: Princeton University Press.
- World Health Organization. Various years. *World Health Statistics*. Geneva: World Health Organization. <http://www.who.int/whosis/en/>
- World Health Organization 2009. *World Health Statistics: Indicator Compendium*. Geneva: World Health Organization.
- Wintrobe, Ronald 1998. *The Political Economy of Dictatorship*. Cambridge: Cambridge University Press.
- World Bank. *World Development Indicators*. Washington, DC. www.worldbank.org

Appendix A: Variables and descriptive statistics

Variable description	Source	Mean	Std.dev.	Min.	Max.	N
ARV treatment, average 2004-2007 (% coverage among people with advanced HIV infections)	WHO 2005, 2006, 2008, 2009a, 2009b and World Health Statistics http://www.who.int/whosis/en/	39.7	27.8	0	97.5	157
Political rights (continuous), average 1990-2003	Freedom House	0.57	0.35	0	1	162
Political rights (dummy for 1990-2003)	Freedom House	0.47	0.50	0	1	162
ACPL (dummy for 1990-2002)	Przeworski et al. (2000). Cheibub and Gandhi (2004)	0.48	0.50	0	1	188
GDP/cap. (log), 2003 (PPP, constant int. \$).	Penn World Tables, mark 6.1	3.70	0.50	2.76	4.79	162
GDP/cap. (log), 1990 (PPP, constant int. \$).	Penn World Tables, mark 6.1	3.66	0.48	2.71	4.51	161
Government health expenditure/cap. 2003 (PPP, int. \$).	World Health Statistics http://www.who.int/whosis/en/	504.70	787.02	2	4135	186
Schooling 2002. Secondary gross enrolment rates.	World Development Indicators. www.worldbank.org	71.79	32.12	6.80	157.10	174
Schooling 1990. Secondary gross enrolment rates	World Development Indicators. www.worldbank.org	56.64	31.99	4.95	119.51	170
Former European colony	Grier (1999)	0.29	0.46	0	1	208
Sub-Saharan Africa dummy	Pippa Norris' <i>Democracy Time Series Dataset</i> , January 2009 release, available at http://www.hks.harvard.edu/fs/pnorris/Data/Data.htm	0.23	0.42	0	1	208
South East Asia dummy	Pippa Norris' <i>Democracy Time Series Dataset</i> , January 2009 release, available at http://www.hks.harvard.edu/fs/pnorris/Data/Data.htm	0.05	0.21	0	1	208
Middle East and North Africa dummy	Pippa Norris' <i>Democracy Time Series Dataset</i> , January 2009 release, available at http://www.hks.harvard.edu	0.09	0.29	0	1	208

	/fs/pnorris/Data/Data.htm					
Ethnic fractionalization, various years	Alesina et al. (2003)	0.44	0.26	0	0.93	187
Corruption 2003	World Governance Indicators (Kaufmann et al. 2008)	-0.015	1.01	-1.98	2.42	192
HIV prevalence 2003 (% adults aged 15-49).	WHO (2006) and World Health Statistics http://www.who.int/whosis/en/	2.73	6.21	0.1	38.8	148
Foreign aid (% of GNI)	Global Development Network Growth Database http://www.nyu.edu/fas/institute/dri/index.html	8.52	12.80	-0.025	97.08	143

Appendix B. Logit regressions for binary democracy indicators

Model	1	2	3	4
Dependent variable	ACPL	ACPL	Pol. rights	Pol. Rights
loggdp90	1.77** (2.23)	2.35*** (3.31)	1.47* (1.84)	1.52** (2.22)
colony	-0.08 (0.14)	-0.30 (0.60)	-0.31 (0.62)	-0.29 (0.60)
subaf	-1.84*** (2.78)	-	-1.31** (2.10)	-
seasia	-0.98 (1.00)	-	-1.13 (1.15)	-
midena	-0.71 (0.53)	-	-2.29 (1.54)	-
christian70	0.99 (1.27)	1.07 (1.47)	0.58 (0.70)	0.97 (1.27)
muslim70	-2.64* (1.71)	-2.94*** (2.68)	-1.63 (1.17)	-2.80** (2.39)
ethfrac	-0.87 (0.73)	-1.75* (1.66)	-0.10 (0.09)	-0.88 (0.86)
Constant	-5.55* (1.88)	-7.75*** (2.91)	-4.61 (1.58)	-5.20** (2.02)
Observations	129	129	115	115
Log-likelihood	-54.1	-58.5	-54.4	-57.6
<i>p</i> -scores used for common support in...	...models 1, 3, 5	...models 2, 4, 6	...models 7, 9, 11	...models 8, 10, 12

All results obtained from binary logit regressions with democracy dummies as dependent variables. *p*-scores = propensity scores. *z*-statistics in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1. Political regimes and HIV/AIDS treatment: OLS and robust regression estimates

Model	1	2	3	4	5	6	7	8	9	10	11	12
Method	OLS	rreg	OLS	rreg	OLS	rreg	OLS	rreg	OLS	rreg	OLS	rreg
Pol rights	22.09** (2.45)	38.24*** (5.40)			28.84*** (3.26)	38.32*** (5.58)	21.30* (1.80)	27.68** (2.36)				
ACPL			8.54* (1.64)	12.22** (2.52)					11.91** (2.31)	16.62*** (3.49)	9.54* (1.78)	10.46* (1.77)
GDP/cap (log)	28.03*** (3.06)	32.94*** (4.64)	28.18*** (3.65)	30.06*** (3.53)	30.86*** (2.83)	28.48*** (4.00)	30.42** (2.01)	26.22* (1.72)	22.23** (2.53)	24.71*** (2.77)	22.15* (1.66)	20.38 (1.42)
Govt health exp.	0.00 (0.70)	-0.00 (0.78)	0.01* (1.90)	0.00 (1.35)	-0.00 (0.62)	-0.01 (1.65)	0.02 (1.26)	0.01 (0.90)	0.00 (0.49)	-0.00 (0.18)	0.01* (1.78)	0.01 (1.29)
Schooling	-0.06 (0.54)	0.08 (0.91)	-0.06 (0.59)	-0.03 (0.23)	-0.08 (0.74)	0.01 (0.09)	-0.10 (0.62)	-0.08 (0.54)	-0.07 (0.66)	-0.03 (0.33)	-0.10 (0.74)	-0.09 (0.59)
Former colony	0.14 (0.04)	-0.01 (0.00)	2.92 (0.89)	3.07 (0.86)	0.52 (0.14)	0.48 (0.19)	2.43 (0.54)	2.38 (0.54)	2.35 (0.71)	2.74 (0.83)	4.15 (1.08)	5.11 (1.13)
Africa	-0.54 (0.07)	17.12*** (3.57)	-1.23 (0.17)	4.02 (0.62)	8.61 (0.86)	15.63** (2.48)	3.95 (0.33)	10.64 (0.99)	-0.37 (0.04)	6.03 (0.73)	-5.07 (0.50)	-0.73 (0.07)
Asia	6.63 (0.69)	2.84 (0.46)	5.07 (0.55)	-0.73 (0.09)	10.47 (1.03)	3.46 (0.62)	9.71 (1.03)	4.62 (0.56)	6.33 (0.68)	-0.11 (0.02)	6.37 (0.69)	1.09 (0.12)
MENA	-12.16* (1.68)	1.98 (0.36)	-13.85** (2.06)	-11.43* (1.68)	-6.73 (1.04)	1.76 (0.33)	-9.41 (1.39)	-5.77 (0.69)	-10.10* (1.65)	-6.75 (1.01)	-11.06* (1.88)	-9.59 (1.15)
Ethnic fract.	-4.91 (0.64)	0.30 (0.05)	-2.77 (0.37)	-3.87 (0.45)	-10.49 (1.34)	-2.67 (0.41)	-7.10 (0.63)	-8.15 (0.67)	-3.44 (0.47)	-4.76 (0.56)	1.40 (0.15)	0.81 (0.07)
Corruption					2.64 (0.71)	5.70* (1.95)	2.23 (0.35)	5.58 (0.97)	6.09* (1.76)	6.61* (1.89)	6.79 (1.23)	10.36* (1.87)
HIV					-0.37 (1.21)	-0.20 (0.86)	-0.24 (0.69)	-0.29 (0.77)	-0.03 (0.10)	-0.11 (0.33)	0.08 (0.26)	0.03 (0.06)
Foreign aid							0.27 (0.63)	-0.02 (-0.05)			0.18 (0.45)	0.02 (0.04)
Constant	-69.47* (1.98)	-114.3*** (4.95)	-65.27** (2.35)	-77.50*** (2.81)	-79.87* (1.87)	-89.33*** (3.55)	-79.06 (1.40)	-67.97 (1.22)	-41.61 (1.27)	-56.64* (1.82)	-42.76 (0.85)	-37.87 (0.71)
# Countries	113	113	130	130	106	106	81	81	122	122	97	97
R ² (adj.)	0.64	0.76	0.57	0.58	0.66	0.80	0.45	0.49	0.60	0.65	0.38	0.39

The dependent variable is the proportion of HIV patients being treated with anti-retroviral therapy, average of 2004-2007. Pol rights is the Freedom House measures of political rights. ACPL is the electoral contestation measure from Przeworski et al. (2000). All explanatory variables are measured prior to 2004. Robust t-statistics in parentheses for OLS regressions. Estimates for robust regression are obtained using the *rreg* command in Stata 10. *** p<0.01, ** p<0.05, * p<0.1.

Table 2. Testing for balance: t-tests for equality of means

	<i>Full sample</i>	<i>ps<0.33</i>	<i>0.33≤ps<0.66</i>	<i>ps≥0.66</i>
ACPL				
Log GDP cap. 1990	0.02	0.92	0.32	0.33
Colony	0.93	0.74	0.73	0.58
Africa	<0.01	0.91	0.81	..
Asia	0.49	0.63	0.89	..
MENA	0.03	0.51	..	0.59
Christians 1970	<0.01	0.45	0.44	0.28
Muslims 1970	<0.01	0.88	0.30	0.22
Ethnic fract.	0.05	0.75	0.71	0.67
Freedom House				
Log GDP cap. 1990	<0.01	0.79	0.73	0.91
Colony	0.72	0.40	0.71	0.51
Africa	<0.01	0.99	0.79	..
Asia	0.55	0.47	0.86	..
MENA	0.02	0.73
Christians 1970	<0.01	0.67	0.43	0.56
Muslims 1970	<0.01	0.57	0.12	0.97
Ethnic fract.	<0.01	0.56	0.53	0.64

Numbers show p-values for t-tests of equality of means for democracies (=1) and non-democracies (=0) under the null-hypothesis of equal means (equal variance assumed). Propensity scores (*ps*) obtained from logit regression of democracy variables on all variables in table. Strata defined within region of common support. ‘..’ denotes no countries in category.

Table 3. Political regimes and HIV/AIDS treatment: Matching estimates

<i>Model</i>	1	2	3	4	5	6	7	8	9	10	11	12
Regional dummies	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
ACPL	15.1** (2.07)	20.4** (2.34)	17.4* (1.74)	17.8* (1.76)	15.09** (2.04)	14.3 (1.63)	-	-	-	-	-	-
Pol. rights	-	-	-	-	-	-	10.1* (1.80)	10.0* (1.84)	10.1* (1.79)	12.7** (2.37)	10.3* (1.81)	17.1*** (3.50)
Common support definition	[min(D=1), max(D=0)]	[min(D=1), max(D=0)]	0.1< <i>ps</i> <0.9	0.1< <i>ps</i> <0.9	[min(D=0), max(D=0)]	[min(D=0), max(D=0)]	[min(D=1), max(D=0)]	[min(D=1), max(D=0)]	0.1< <i>ps</i> <0.9	0.1< <i>ps</i> <0.9	[min(D=0), max(D=0)]	[min(D=0), max(D=0)]
Region of common support	0.039≤ <i>ps</i> ≤0.84	0.048≤ <i>ps</i> ≤0.91	0.1< <i>ps</i> <0.9	0.1< <i>ps</i> <0.9	0.006≤ <i>ps</i> ≤0.84	0.007≤ <i>ps</i> ≤0.91	0.032≤ <i>ps</i> ≤0.84	0.019≤ <i>ps</i> ≤0.82	0.1< <i>ps</i> <0.9	0.1< <i>ps</i> <0.9	0.029≤ <i>ps</i> ≤0.84	0.027≤ <i>ps</i> ≤0.854
# Obs. on common support	87	90	71	78	102	109	91	93	73	73	94	95
# Obs. off common support	24	21	40	33	9	2	6	4	24	24	3	2

Outcome ('dependent') variable is ARV treatment, average 2004-2007. Estimates show average effect of treatment on the treated (ATT). All estimates are obtained through nearest-neighbour matching using the inverse of the sample variance as weighting matrix and one (the nearest) matched control per treated case. All covariates listed in Table 2 are used in the matching procedure. Matching is done with replacement, allowing control cases to be matched to more than one case in the treatment group. All estimates are obtained using the bias-corrected matching estimator developed by Abadie et al. 2004). Bias-adjusted is done according to values of all covariates in models. All z-values are based on heteroskedasticity-consistent standard errors calculated using one matched case. Estimates are obtained using the 'nnmatch' command in Stata 10. *** p<0.01, ** p<0.05, * p<0.1.