An Empirical Investigation of Why Doctors Migrate and Women Fail to Go for Screening

by

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To Enuma, Kamtoya and Nebenma

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CHAPTER I

Introduction

There is a lot of concern about the outflow of skilled medical professionals particularly physicians, from the developing to the developed world (WHO, 2006). Mullan (2005) estimates that between 23 percent and 28 percent of practicing physicians in the United States, the United Kingdom, Canada and Australia are physicians trained in other countries; with between 40 to 75 percent of these coming from lower-income countries. This has raised the specter of shortages in these sending countries, many of which already have an inadequate number of health professionals based on ratios defined by the World Health Organization. Even though there is widespread agreement that the patterns and magnitude of physician migration are worrying, there is much less agreement over what should be done about it. This is due, at least in part, to the fact that there is still considerable uncertainty as to what the key drivers of international physician migration are. A number of studies (see for example Hagopian et al., 2005; Astor et al., 2005) have spotlighted different factors ranging from better training opportunities in destination countries to poor working conditions in source countries but the relative contributions of each are still far from clear. This gap in the literature is what has given rise to the first two of my three dissertation papers. In both of these papers I attempt to provide causal estimates of the impact of some of these *push* factors.

In my first paper I investigate the impact of short-term economic shocks on physician migration using a new panel dataset on the annual outflow of physicians from 31 Sub-Saharan African countries to the United States and the UK. This dataset covers the period from 1975 to 2004. The question I attempt to answer in this paper is "to what extent does physician emigration respond to changes in economic conditions in source countries". I estimate distributed-lag regressions of log migration on economic growth, including country fixed effects and controlling for country-specific time trends. To account for omitted variable bias and measurement error, I instrument economic growth using rainfall and changes in terms of trade. Overall I find a significant effect of economic conditions: a one percentage point decline in lagged growth increases physician out-migration by approximately 0.3 percent. The IV estimates are larger, suggesting that estimates from the fixed effects models are biased downwards. In the IV models, a one percentage point decline in lagged growth increases physician out-migration by between 3.4 and 3.6 percent.

In my second paper, I tackle another commonly mentioned *push* factor - low wages. Many policy experts and researchers have argued that low wages (relative to their counterparts in more developed countries) are probably the most important factor contributing to the increasing emigration of health professionals from poor countries (see for example Dovlo and Martineau, 2004). This has naturally led to a call to increase the compensation of health professionals in developing countries. In this paper, I exploit a "natural experiment" to identify the causal impact of wages on physician emigration. The Ghanaian Government in 1998 instituted a scheme known as the Additional Duty Hours Allowance (ADHA) Scheme which compensated doctors for any additional hours worked beyond the standard 40 hours a week/160 hours a month. Estimates suggest that the ADHA scheme increased doctors incomes in Ghana by between 75-150 percent. Using innovative new methods developed by Abadie et al. (2007) and applying it to physician stock data collected by Docquier et al. (2007), I find that by 2004 - six years after the program was instituted - the foreign stock of Ghanaian physicians had reduced by about 10 percent and attribute this directly to the effect of the ADHA program.

In my third paper I examine the demand for preventive care. Cervical cancer is the most common cancer among women in developing countries. Of the roughly 273,000 deaths from cervical cancer worldwide in 2002, nearly 80 percent of these deaths occurred in developing countries. Despite compelling evidence that cervical cancer screening has reduced morbidity and mortality in developed countries, screening rates in many developing countries are still very low. In this paper I examine the importance of demand-side factors on take-up of cervical screening. Using a randomized design, I test for the impact of price. I also test for the impact of receiving a conditional cancer treatment subsidy. Overall I find a quantitatively and qualitatively significant impact of both interventions. A N10 increase in price of screening reduced take-up of the program by between 7 and 8 percentage points while women selected to receive the cancer treatment subsidy were about 4 percentage points more likely to participate in screening.

CHAPTER II

African Doctor Migration: Are Economic Shocks to Blame?

2.1 Introduction

There aren't very many topics in the health policy arena that generate as much buzz as the issue of health professional migration.[1] It is clearly not a new phenomenon, judging from some of the earlier published work (see for example Jonish, 1971; Mejia, 1978) but within the last couple of years it has become the *de rigueur* topic within international/global health policy circles. No less a publication than the 2006 World Health Organization (WHO) Report was devoted to discussing the current crisis in the global health workforce and the role of health professional migration. A legitimate question then to ask might be why this resurgence of interest in health professional migration?

There are probably a couple of answers to this. One is that the rate of emigration seems to have accelerated over the last two decades. Hagopian et al. (2005) estimate that the number of physicians in the US from Nigeria and Ghana, (two major exporters of physicians to the US from sub-Saharan Africa) increased by more than 1,000 percent between 1981 and 2002. Labonte et al. (2006) document a similar phenomenon for Canada albeit not as striking.

The extent of migration is significant. Nearly 61 percent of all doctors produced in Ghana between 1985 and 1994 were in the US or the United Kingdom by 1998 (Dovlo and Nyonator, 1999). In a different paper, Hagopian et al. (2004) estimate that about 20 percent of Uganda's doctors and 43 percent of Liberia's physicians were working in the US or Canada by 2002. More recent data, which include many more destination countries, present an even more pessimistic picture. Clemens (2007)

[1] Sometimes referred to in the more popular media as medical brain drain.

shows that more than a quarter of all African countries (16 out of 53) in the year 2000 had at least 50 percent of their doctors living overseas.

Many experts believe that health professional migration has a variety of negative effects on the country of origin (see discussion in Hagopian et al., 2005) though one recent paper has questioned that assumption (Clemens, 2007). Clemens argues forcefully that we need to re-examine the assumption that migration of health professionals somehow reduces welfare in the country of origin. In his analysis, he shows that the poor health outcomes in many African countries, arguably one of the key concerns, may have little to do with the migration of health professionals from those countries. Clearly, the jury is still out on whether health professional migration has net negative welfare effects and a lot of research still remains to be done.

In this paper I sidestep the issue of whether migration has adverse effects on the sending country and instead focus on *why* health professionals might be emigrating in increasing numbers. This is an interesting question in its own right and has tremendous policy significance; but should one need further motivation, I argue that since many countries have revealed a strong preference to want to reduce the outflow of health professionals, then this is a sufficient condition to motivate research that seeks to understand how to help them do so. There is mounting evidence that from the perspective of developing countries the question is not whether something should be done about migration of health professionals, it is what. To the extent that we accept this as a guide, research that improves our understanding of the causes of health professional migration is critical.

When it comes to the question of why health professionals migrate, there is no shortage of candidate answers. Surveys of health professionals suggest several potential explanations, ranging from worsening economic conditions, poor working conditions and low salaries, to political instability and poor governance (Astor et al., 2005; Awases et al., 2004). Surprisingly however, there has been very little systematic testing of these alternative explanations and it remains unclear the extent to which many of these factors explain observed emigration rates. In this paper I test whether adverse economic outcomes in countries of origin lead to increased migration of health professionals. I focus on physician emigration from sub-Saharan Africa to two popular destinations, the US and the UK. In Figure 2.1 below, I graph physician migration over time for the 31 countries in my sample.

Notice that 2003 is somewhat of an outlier. It is driven by a more than 3-fold increase in emigration from South Africa to the UK between 2002 and 2003. This sharp increase was likely a response by South African doctors to changes to the UK

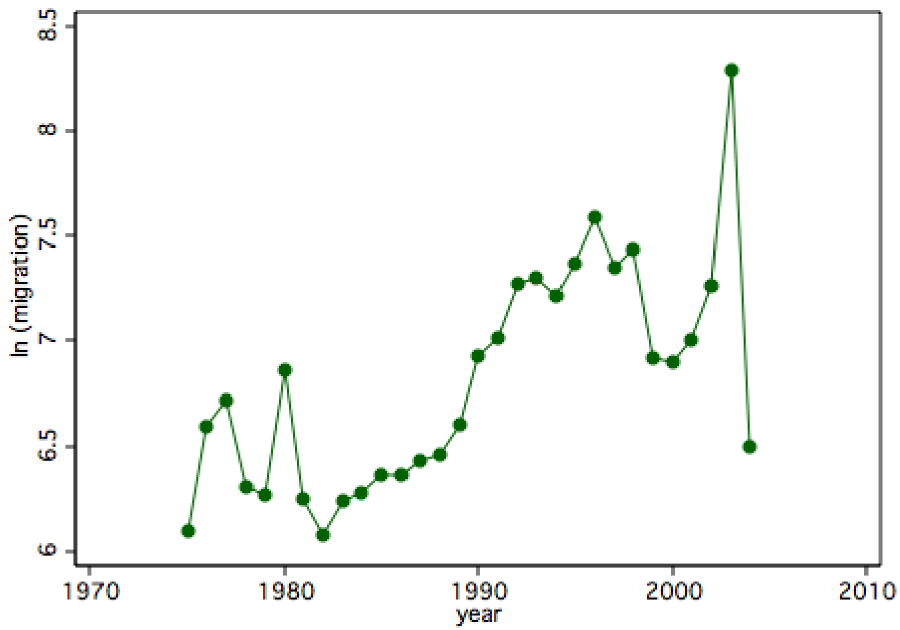

Figure 2.1: Physician Emigration Trends (1975-2004)

Medical Act. Prior to 2003 the Medical Act allowed doctors with qualifications from certain universities in Australia, New Zealand, Malaysia, Singapore, Hong Kong and *South Africa* a direct route to full or provisional registration. Following changes to the Medical Act which were to take effect from 31 December 2003, they in common with all other doctors who did not benefit from the freedom of movement provisions of European law, would now have to provide additional evidence of their capability for practice, which at a minimum involved taking a test of their professional knowledge and skill.[2] As will become clear later on this does not present a problem for my analysis. Overall, the graph provides some evidence that physician migration has increased. The number of physicians migrating trended upwards until around 2000. It has declined somewhat since then.

2.2 Economic Shocks and Migration

There is a fairly extensive literature on the economics of migration: Borjas (1994) provides a nice overview. Even though much of the empirical literature is focused on economic outcomes (of migrant and domestic workers) and the role of selection and skills, and less so on the determinants of migration, there is some evidence that

[2]Personal communication received from the General Medical Council

economic conditions in countries of origin play a role in migration. Borjas (1987) shows that there is a negative correlation between origin country incomes per capita and migration to the US, Karemera et al. (2000) document a similar finding using panel data on migration to the US between 1976 and 1986.

A different subset of studies has looked at return migration: Yang (2006) for example shows that the return migration of Filipino workers abroad is sensitive to fluctuations in the exchange rate, while Docquier et al. (2007) show that skilled workers appear to be more likely to return to their home countries after the home country experiences periods of sustained economic growth. See also work by Hatton and Williamson (2003) and Blanchard and Katz (1992) both of which show a positive correlation between economic conditions and in-migration. Even though these latter studies deal with return migration, they certainly raise the possibility that out-migration may also be responsive to economic shocks.

Many people also believe that economic conditions in developing countries contribute to the out-migration of health professionals (Bundred and Levitt, 2000; Pang et al., 2002). This seems intuitively plausible but there is surprisingly little empirical evidence. Much of the evidence we do have tends to be indirect. For example, work by Miguel and Kremer (2004) has shown that negative economic shocks can cause civil conflict and civil conflict in turn has been linked to health professional migration (see Clemens, 2007). Taken together however, the empirical evidence seems to suggest a potential role for economic shocks in explaining physician migration. But is there a theoretical explanation for why economic shocks might affect doctor migration? In this next section I lay out a simple model explaining how poor economic performance in a given developing country might affect health professional migration.

2.2.1 Simple Model

There are several economic models which highlight some of the factors that play into the individual decision to migrate (see e.g. Roy, 1951; Harris and Todaro, 1970). Underlying all of these models though is a simple cost-benefit calculation: *if the expected discounted benefits from migration (higher earnings, better quality of life etc) exceed the costs (transportation costs, job search costs, psychic costs etc) then an individual will migrate.* To state this more precisely, a health professional will migrate if:

$$E\left[\sum_{t=1}^{T} \frac{B_t}{(1+r)^t}\right] \geq C \tag{2.1}$$

Where B represents the benefits from migrating, C is the cost of migration and r

is the discount rate.

If we assume that that the only benefit from migration is higher earnings, so that $B = Y_t^F - Y_t^D$ where Y_t^F represents earnings in the foreign country and Y_t^D represents earnings at home, then migration will occur if;

$$E\left[\sum_{t=1}^{T} \frac{Y_t^F - Y_t^D}{(1+r)^t}\right] \geq C \tag{2.2}$$

Let us assume that the physicians in country j are identical save for the fact that they differ in C. In other words, there are individual-specific costs of migration. One can also think of this as reflecting individual preferences for migration in terms of equivalent income. I introduce the subscript i to denote the individual. C_i is exogenous and distributed according to the *pdf* $g(C)$. Each physician gets assigned a unique C_i. This suggests that some physicians will have high costs of migration and others will have low costs of migration.

For example, a physician born into a family with several members overseas will have a lower C_i than another physician who is exactly the same except for the fact that he is born into a family without a single member overseas, (in fact no one in his family has ever been abroad). For the first physician, the cost of migrating is lower for several reasons:

1. The direct costs are lower. For example adjustment costs and job search costs;
2. The psychic costs are also lower because he is more familiar with the foreign environment (perhaps he spent a couple of summer abroad while he was in medical school).

This implies that a threshold C exists, lets call it C^*, below which there is positive net benefit from migration and above which there is a negative net benefit from migration i.e.

$$C^* = E\left[\sum_{t=1}^{T} \frac{Y_t^F - Y_t^D}{(1+r)^t}\right] \tag{2.3}$$

Our simple stylized model would suggest that all doctors with $C_i < C^*$ in country j will migrate leaving behind only doctors with $C_i \geq C^*$.

If the stock of doctors in country j at time t is given by S_t, then the fraction of physicians in country j migrating in time t is given by $S_t G(C^*)$ where $G(C^*)$ is the CDF of C evaluated at C^*.

Given our formulation, a shock to earnings clearly alters C^*. A negative shock to domestic earnings Y^D will increase C^*, tipping more doctors over the edge. Some doctors with $C_i \geq C^*$ previously, will now find that because C^* has increased, they now meet the migration condition $C_i < C^*$.

The prediction from this admittedly stylized model is straightforward, *if there is a negative shock to domestic earnings, more physicians will migrate.*

2.2.2 Economic shocks and earnings

Even though I do not model economic shocks directly, one plausible way in which economic shocks can enter this model is through earnings, Y^D. If domestic earnings depend positively on economic conditions i.e. $Y^D = f(E)$; where $\frac{\partial Y}{\partial E} > 0$ and E represents aggregate economic conditions, then it follows that negative economic shocks will reduce Y^D and will increase the number of doctors migrating.

It is certainly plausible to think that earnings depend to some extent on economic conditions. Dräger et al. (2006) for example show a positive correlation between physician and nurse wages and GDP per capita. Realize also that in many African countries, a large fraction of medical care is paid for out-of-pocket. On average, private health expenditures in sub-Saharan Africa constitute about half of all health expenditure and 80 percent of that are out-of-pocket costs.[3] In bad economic times, firms reduce output, workers are laid off and households reduce consumption.[4] To the extent that medical care consumption is a normal good, consumption of medical care is reduced and this directly impacts a doctors earnings.[5] In countries where the government is the major employer of doctors, the link from aggregate economic performance to earnings is less straightforward but it is not uncommon to have non-payment of salaries because the government has run into fiscal difficulties (Zachariah et al., 2001). In addition, it is fairly common to see doctors who work in the public sector maintaining a private practice on the side (Ferrinho et al., 2004).

A less obvious but perhaps more important effect of negative economic shocks is its indirect impact. It may change expectations about future earnings. If economic shocks are not just purely transient but have longer term impacts so that a shock in period t, has an effect not just in period t, but in $t+1$, $t+2$ and so on, then rational expectations about the distribution of future earnings will change in response to a shock today. And from our model a reduction in future domestic earnings clearly affects migration. Here of course I are making the assumption that doctors are at

[3]Author's calculations based on health expenditure data taken from the World Development Indicators.

[4]Households are unable to borrow and smooth consumption because of imperfect credit markets. Because these are aggregate shocks, informal sources of credit are also affected.

[5]We ignore the possibility that doctors can stimulate demand in order to augment their income.

least partially forward looking. This appears to be consistent with the evidence (see e.g. Nicholson and Souleles, 2002)

Interestingly many surveys of physicians cite despair about the future as an important reason for migrating (see for example Awases et al., 2004). It appears therefore that one can make a theoretical case for poor economic conditions affecting the migration of health professionals.

But is the empirical evidence consistent with the theory? I proceed to test this hypothesis using a new dataset, which I introduce and describe, in the next section.

2.3 Data

One of the factors that has hindered research in this area has been a lack of reliable data. Data on health professional migration from the countries of origin are notoriously unreliable, which has meant that the vast majority of published work has had to rely on destination country data. The problem with destination country data is that it only captures migration to that country. To get a sense, let alone an accurate measure, of the magnitude of health professional migration from a given country one would have to collect data from many different destination countries. For example until very recently there was no simple way to answer the question, "what percentage of South Africa's physicians have emigrated?", a basic question that would seem fundamental to the study of physician migration, but to answer that one would have to get data from the US, the UK, Canada, Australia and a host of other destination countries, by no means a simple task.

Fortunately two data sources now exist that attempt to quantify physician emigration from all African countries: the Bhargava and Docquier dataset (Docquier and Bhargava, 2007) hereinafter referred to as the BGD, and the Clemens and Pettersson dataset (Clemens and Pettersson, 2008) hereinafter referred to as the CPD.

Both are ambitious and admirable attempts to quantify migration flows but both have limitations. The BGD attempts to measure the stock of physicians from African country j in 16 OECD countries in every year between 1991 and 2004 but is limited by not having annual data for some of the countries (and thus relies on interpolation) and is further limited because the definition of the African doctor is not consistent across the entire sample.[6] The CPD measures the stock of physicians from African country j in nine different developed countries in the year 2000. It improves on the BGD by

[6] In their data, the African doctor is defined based on country of qualification (73% of the sample in 2004), then where that data is not available, by country of birth (18% of the sample in 2004) and where that is not available, by country of citizenship.

defining the African doctor consistently across the entire sample (based on country of birth), but suffers from one important limitation, it only gives us a snapshot of emigration at one point in time and misses out on all the interesting dynamics of emigration over time.

Neither data set is quite ideal for answering the question posed in this paper; the CPD because of its cross-sectional nature and the BGD because it fails to capture emigration in the 1980s, a period of significant economic upheaval and turbulence. Also both datasets capture stock, not flow. Due to these limitations, I use an alternative data source which I describe in the next section.

2.3.1 Data Sources

The data set used in this paper captures migration from 31 sub-Saharan African countries to two of the most important destination countries for emigrating African doctors, the US and the UK (which together account for nearly two-thirds of the stock of African physicians abroad). More importantly I capture migration over the period 1975-2004.

US data comes from the US Educational Commission for Foreign Medical Graduates (ECFMG), which issues certificates to all foreign medical graduates. Note that without ECFMG certification a foreign-trained physician cannot practice medicine in the United States.[7] I have data on the number of ECFMG certificates issued in each year from 1975-2004 to physicians from 31 Sub-Saharan African countries. As I argue later, year of ECFMG certification is a good proxy for migration to the US especially for physicians from this region. UK data comes from the General Medical Council (GMC), which plays a similar role as the ECFMG in the US. I have data on the number of doctors from SSA granted registration for the first time in every year between 1975 and 2004. For more details about the registration process for foreign medical graduates in the UK, (see the excellent discussion in Constable et al., 2002).

In the next section I discuss my data and I compare it to earlier datasets. I restrict my attention to the CPD because it is the more recent dataset and marginally improves on the earlier effort by Bhargava and Docquier.

2.3.2 What our data is and what it is not

First of all, I follow the majority of the literature and define the *African* physician based on country of medical training. In other words, a doctor trained in an African

[7] Certificates are only issued after the foreign medical graduate has passed all examinations and his/her medical diploma has been verified (see detailed discussion in Boulet et al., 2006).

country is an African physician. I prefer this definition to the country of birth definition for a couple of reasons. The country of birth measure counts a physician has having migrated from country j if he/she was born in that country. This means that a British child born in Kenya for example, who then returns to England and decides to become a doctor is counted as having migrated from Kenya. Or take the case of an Indian child born in Uganda who returned to India with his parents following the Idi Amin purge of the 1970s, decided to go to medical school and who then later migrated to the UK. The country of birth definition counts him/her as a Ugandan physician who has migrated. As I show in Table A.2 in the appendix, this introduces systematic upward biases in migrant counts for certain SSA countries which have/had large white (or non-native) populations or are popular travel destinations for foreigners; countries such as Kenya, Uganda and Tanzania.

Even if a physician is a true native, i.e. born in Africa to African parents, but emigrated as a child with his parents, it is not obvious that he should be counted as a migrant physician from his country of birth because it is not clear that he would have chosen to train as a doctor if he had not migrated. It is also likely that from the perspective of the country of origin, he would not be counted as a migrant physician. I therefore think the more relevant measure is the number of physicians who trained in those countries who then decide to migrate.

The country of training definition is not without it's own problems however. For example, by definition, countries without a medical school show up in the data as having zero migration. In the data, 12 countries in SSA show up as having zero migration because they did not have a medical school over most of the period covered by our data (see appendix). These turn out however to be very small countries and islands which have small populations and comparatively few physicians.[8] Two other concerns that may be raised about our data are: (1) I do not observe actual migration and (2) I only capture one-way migrations flows. I proceed to tackle each in turn.

In the US data, what I observe is the number of ECFMG certificates issued (not actual migration) and at least in theory it is possible to receive ECFMG certification without being in the US. First I argue that year of certification is a close proxy for actual migration especially in SSA because of the significant cost of getting certified and I make the case that physicians who go to the trouble of getting ECFMG certified have decided to migrate and are either in the US already or are on their way there.

To receive certification in the US, foreign-trained physicians must incur substantial

[8]Botswana, Eritrea, Gambia, Equatorial Guinea all now have medical schools established some time after 2000.

fixed costs. Currently, registration for the exams alone, which is a prerequisite for certification, costs nearly 3000 US dollars. This represents a significant investment for physicians in SSA countries where the average per capita income in 2005 was $1000[9], and it makes sense to think that African doctors who incur that expense are at most within a few years of emigrating. As will be seen later on when I introduce the econometric strategy, I include several lags of economic shocks to account for this. As an additional check I compare my US data with US data from the CPD which comes from the 2000 US census, and show that the correlation between both is 0.98 (see Figure A.1 in the appendix).

The UK data counts the number of first-time registrations with the General Medical Council in a given year by physicians trained in SSA country j, implying that what I observe is actual migration because registration can only be done if the physician is in the UK. The problem is, and this brings us to the second concern, I only observe one-way flows i.e. *to* traffic. It is entirely possible, even likely that some physicians are only coming to the UK for post-graduate medical training. In other words, they are not migrating permanently: they stay for a few years, finish their training and then return to their countries of origin. This is arguably less of a problem in the US where earlier work has shown that the rates of return migration are very low (Mick and Worobey, 1984), but even for the UK, the percentage of foreign-trained physicians that remain in the UK after completing their training is high at between 60-70 percent (Kangasniemi et al., 2007). We have no way of identifying those who return and so to the extent that this is true, I may be slightly overstating the true extent of migration. One reason why our South Africa numbers may differ from the CPD data may be for precisely this reason (see Appendix). In addition to return migration, another explanation for why the South African migration numbers to the UK in my dataset might be that much higher than the comparable number in the CPD is that South African physicians may simply use the UK as a staging point and go on from there to other popular destinations such as Australia and New Zealand (Padarath et al., 2003). The correlation between both datasets nevertheless is still high at 0.77 (see Figure A.2 in the appendix). In the analysis which follows, I include and then exclude South Africa from the sample as a sensitivity check and show that my conclusions remain unchanged.

[9]In real dollars. By way of comparison, the world average in 2005 was $7200 USD and the average for OECD countries was just over $26,000 (World Bank, 2008).

2.3.3 The case for using only data on migration to the US and UK

Clearly there is emigration to other countries other than the US and the UK and this is not captured in our data. As can be seen from Table 4.2, English-speaking SSA countries generally have higher migration numbers to the US and the UK than French-speaking countries. This makes sense because one would expect more migration between countries that share a common language. In other words, one would expect relatively more migration from Cote D'Ivoire (a French-speaking West African nation) to either France or Belgium than to the US or the UK.

While this is undoubtedly true, for the vast majority of countries (the exceptions are Burkina Faso, Central African Republic, Chad, Gabon and Guinea-Bissau), there is non-zero migration to one or both of our destination countries. In addition, both our destination countries account for a substantial amount of migratory flows. In the year 2000, approximately 20 percent of all African doctors living abroad were in the US alone; 43 percent were in the US and UK combined.[10] When I restrict the sample to the 48 Sub-Saharan countries, the percentages are even larger: 23 percent in the US and 60 percent in the US and UK combined. In my dataset of 31 countries the percentages are 24 percent and 62 percent respectively. In Figure A.3 in the appendix, I plot the fraction of physicians from each SSA country in the US and the UK combined using data from the CPD.

2.4 Descriptives

Migration statistics for the 31 countries in the sample are reported in Table 4.2. Migration to the US is in Column 1, migration to the UK is in Column 2 and the total is in Column 3. To enable comparison across countries, each country's total migration figure is scaled relative to it's 2004 population (column 4). To put the numbers in perspective I also report the number of doctors per 100,000 in each country (column 5). With the exception of Ethiopia (2003), Nigeria (2003), Tanzania (2002) and Somalia (1997), the number of domestic doctors per 100,000 reported is for 2004.

While these numbers are at best a rough approximation, they give a sense of the scale of the problem for each country. So for example only 94 Liberian doctors migrated to the US and the UK between 1975 and 2004, which seems like a small number, but expressed as a per capita ratio, it becomes 2.9 per 100,000. Compare this to the number of physicians per 100,000 living in Liberia in 2004, 3. On the other hand, 299 physicians migrated from Tanzania over the same period (which is more

[10] Authors calculations from Clemens (2006).

than triple the number of physicians who left Liberia) but this translates to 0.8 per 100,000, compared to a 2004 per capita ratio of 2.3.

As an aside, note that while our data is suitable for answering questions such as the one posed in this paper, it is not the right kind of data to answer questions such as "what fraction of a country's physicians are abroad?" The reason is simple: I only capture migration to two destination countries, and secondly these numbers can more accurately be described as capturing gross and not net migration. If the intent is to simply to quantify the extent of migration from a given SSA country, then one is better served using the kind of data collected by Clemens and Pettersson, and Bhargava and Docquier. The numbers presented in Column 4 should therefore be interpreted cautiously with those caveats in mind.

2.4.1 Data on economic conditions

Data on economic performance come from the World Development Indicators (2007). One can think about measuring changes in economic conditions in different ways. Rodrik (1994) for example measures economic shocks using shocks to terms of trade; Ruhm (2000) uses unemployment rates as his measure of economic performance. In this paper I proxy for economic conditions using changes in average per capita incomes. A negative economic shock is defined as a decline in real GDP per capita between year $t-1$ and year t while a positive economic shock is an increase in real GDP per capita between $t-1$ and t. The key explanatory variable,

$$GROWTH = \frac{GDPCAP_t - GDPCAP_{t-1}}{GDPCAP_{t-1}}$$

Summary statistics for growth are presented in Table 3.4. In all, I have 864 country-year observations. Notice that there is a significant amount of within-country as well as between-country variation in growth rates. The average for the entire sample is 0.04 percent with a standard deviation of 7 percent. Thirteen countries have negative average growth rates, with the Democratic Republic of Congo performing the most poorly, while 18 countries experienced positive growth on average, led by Mauritius with an average growth rate of 4.2 percent.

Next I ask the question, is there a correlation between economic growth and physician migration over the period covered by the data as a whole? First I plot log total emigration against growth (see Figure 4.3). Here each observation is a country. The question I ask is *does one observe more migration from countries that performed worse on average?*

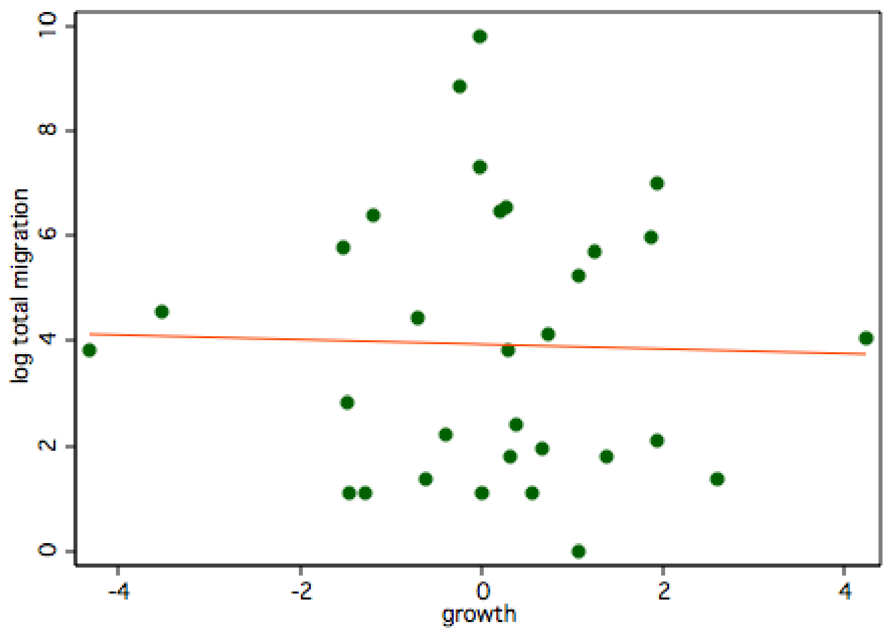

Figure 2.2: Is growth correlated with migration? (Cross-section)

As is evident from Figure 4.3, there does not appear to be a relationship. Next I exploit the longitudinal nature of the data and pose a slightly different question; *does one observe an increase in physician migration when the economy does poorly and a decrease in migration when the economy does better?* In other words, is there a counter-cyclical relationship between economic performance and physician emigration? I average over all the countries in the sample and plot log total migration in each year on one axis and mean growth in the same year on the other axis (see Figure 3.3).

There is not an obvious relationship but there does appear to be some counter-cyclicality. Between the early-80s and the mid-90s when growth is mostly negative, physician migration appears to increase and from the mid-90s to the mid-2000s when growth is positive, physician migration appears to reduce. Even if we observed a clear inverse relationship, this would still not imply causality for obvious reasons and so we proceed to the econometric analysis.

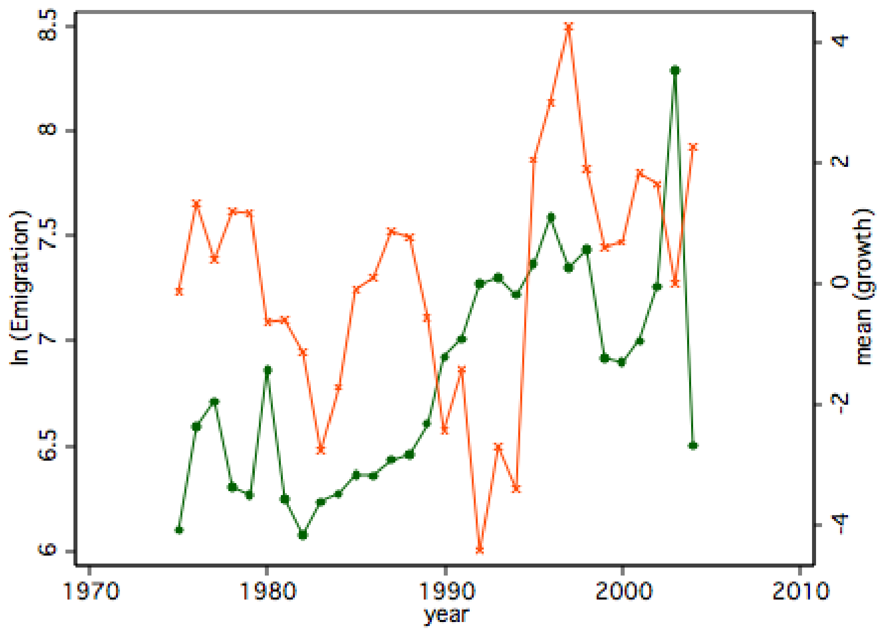

Figure 2.3: Is growth correlated with migration? (Time-series)

2.5 Econometric Model

The base model is an OLS regression taking the following form:

$$ln(MIGR_{jt}) = \alpha_0 + \sum_{s=0}^{S} \beta_{1+s} GROWTH_{j,t-s} + X'_{jt}\gamma + \epsilon_{jt}$$

The dependent variable is log migration for country j in year t. In other models I specify the dependent variable as log migration per 100,000 population in year t. The latter is a measure of physician density and accounts for differences in overall health system size. Previous work by Arah (2007) has shown that migration statistics are sensitive to how migration is measured. Growth enters in with several lags for two reasons: (1) to account for the fact that migration is probably not instantaneous. From the time the decision to migrate is made, it may take anywhere from a few months to a few years before the physician is able to relocate. In our basic specification, S=2 but in sensitivity analyses I include more lags of growth. (2) I lag growth to account for the fact that economic shocks in one period may have impacts that extend into future periods. For example, an economic shock in period 1 may affect migration not just in period 1 but also in period 2. If for example, a physician's expectations about economic growth (and therefore earnings) in period $t + m$ are based at least in part on observed growth in period t and periods $t - s$, then one might expect a shock in $t - s$ to affect migration in t. One can therefore think of this lagged specification as a crude reduced form model of expectations.

X is a vector of various time-varying and time invariant controls. Economic theory predicts that the cost of migration will have a significant impact on migration. To proxy for the cost of migration, I include a dummy variable for whether the country is a former British colony. I hypothesize that physicians trained in those countries are more likely to migrate to our two destination countries relative to physicians from former French (and other) colonies. Because of the similarity in language, training and curriculum between former British colonies and our destination countries, transition costs should be lower for physicians trained in those countries.

I also include a distance variable which measures the distance in kilometers between country i and country j where i is either the United States or the United Kingdom and j is one of the SSA countries in the sample. The distance measure is taken from the CEPII bilateral distance dataset[11] and measures the distances between the biggest cities of countries i and j, those inter-city distances being weighted by the share of the city in the overall country's population (Head and Mayer, 2002). Again, I hypothesize that migration costs rise in relation to distance.

I also include a dummy variable for civil conflict to account for the fact that civil conflict is probably correlated with both economic growth and migration. Fearon and Laitin (2003) and Miguel and Kremer (2004), amongst others, have shown that civil conflict negatively impacts economic growth and work by Clemens (2007) suggests that civil conflict may lead to increased health professional migration. It is certainly possible however that conflict/war may reduce migration if in times of civil conflict, the cost of migration increases. For example, the pecuniary costs might increase if travel arrangements become more difficult e.g. because a country closes it's consulate or scales down services. Alternatively, the psychological cost of migration might increase if one is concerned about leaving family behind in a conflict situation.

It is also possible that civil conflict increases certain types of migration (e.g. to neighboring countries as refugees) while reducing other types e.g. migration to OECD countries, which require more planning and preparation. Hatton and Williamson (2003) for example highlight the fact that only a tiny fraction of African refugees displaced by conflict end up outside Africa; most of the displacement is to neighboring countries.

Data on civil war/conflict comes from the well-known UCDP/PRIO Armed Con-

[11] Available at http://www.cepii.fr/anglaisgraph/bdd/distances.htm.

flict Dataset (Version 4, 2008), developed by the International Peace Research Institute of Oslo, Norway, and the University of Uppsala, Sweden (Gleditsch et al., 2002). WAR is a dummy variable equal to 1 in years in which more than 1,000 battle-related deaths occurred in a given country. This definition is also taken from the PRIO database and is commonly used in the conflict literature. I include it with one lag.

The Roy model (Roy, 1951) predicts that macro-level factors such as political stability will affect migration and so I include a dummy variable for coups and coup attempts. This data comes from McGowan (2003) and covers the period from 1956 to 2001. Inclusion of this variable in the models reduces the sample size to 712. In the models reported here $COUP$ is a dummy variable equal to 1 in years in which at least one coup attempt took place. The results are robust to specification of $COUP$ as a dummy for at least one *successful* coup attempt or as the number of coup attempts in year t. I also include it one lag.

Other control variables include the domestic physician per capita ratio in 1975[12] because I hypothesize that physicians in countries with a higher physician per capita ratio may be more likely to migrate if for example medical education is subsidized so that too many physicians are produced i.e. there is excess supply, or conversely may be less likely to migrate if a higher per capita ratio is acting as a proxy for unobserved demand for medical care (and by extension for medical care inputs). $GDP75$ is real GDP per capita in 1975 included to account for the fact that ceteris paribus, physicians from richer countries may be less likely to emigrate. To allow for non-linearities in the effect of GDP per capita, I divide real GDP in 1975 into quartiles and enter $GDP75$ as a categorical variable. The results are qualitatively similar to specifying $GDP75$ as a flexible polynomial. Data comes from the Penn World Tables (version 6.2). Results for the basic OLS specification are in Table 2.4 (Column 1).

Note that the choice of control variables is also constrained by the data available. For example I would have liked to control for government per capita health expenditure as a way to proxy for investments in health capital but I only have that data for a small subset of countries and for a small subset of years. Other variables that potentially belong in this model are the wage differential between origin and destination country as well as the inequality differential, but even if these data were available, their effect could not be separately identified from those of the year fixed

[12]Or the earliest year available for the following countries: Congo (1978), Benin and Mozambique (1980), Guinea and Malawi (1981), Angola (1984) and Cote D'Ivoire (1985).

effects which we include in most specifications.

Next I estimate fixed effects models where I include country fixed effects to control for time-invariant country-specific factors, and year fixed effects to account for overall trends in migration. Note that changes in US/UK immigration policy over time, changes in US/UK demand for foreign medical graduates etc, should affect all the countries in our sample similarly (relative to if I had a more heterogeneous sample for example) and should therefore be captured by the time fixed effects. In some specifications I allow for country-specific time trends. The basic model I estimate has the following form:

$$ln(MIGR_{jt}) = \beta_0 + \sum_{s=0}^{S} \beta_{1+s} GROWTH_{j,t-s} + \sum_{t=1}^{T} \delta_t T + X'_{jt}\gamma + \upsilon_j + \epsilon_{jt}$$

The dependent variable is again log migration in year t; δ_t captures common time trends, X is a vector of controls and υ_j are the country fixed effects. I overwhelmingly reject the null of common time trends (F-statistic = 14254) and so in the preferred specification I allow for country-specific time trends. I again control for contemporaneous and lagged conflict, a quadratic of population as well as contemporaneous and lagged coup attempts; but the other variables, which do not vary over time, drop out of the equation. By estimating fixed-effects models, the parameter of interest is identified off of within-country variation in economic conditions over time. Summary statistics are in Table 4.3. I estimate all models first for total migration (Table 2.4 columns 2-4) and then separately for US and UK migration (Table 2.5 and Table 2.6). Results for the alternative specification where the dependent variable is log migration per 100,000 population are shown in Table 2.7.

From Table 2.4 column 1, we see that the coefficient on $BRITISH$ has the expected sign and is statistically significant suggesting that physicians from former British colonies are indeed much more likely to migrate to our two destination countries relative to physicians from former French and Portuguese colonies for example. The coefficient implies that relative to countries not colonized by the British, ex-British colonies have more than double the amount of physician migration (144%). The coefficient on $DOCTORS75$ suggests that physician migration is higher from countries with higher physician per capita ratios. An increase of 1 per 100,000 in the domestic physician per capita ratio in 1975 is associated with a 7.2 percent increase in physician migration. The signs on WAR suggest a positive effect of contemporaneous civil conflict (with >1000 battle-related deaths) on physician migration and a negative effect of lagged war, but none of the coefficients approach significance. The coefficient on distance is negative as hypothesized but also not statistically significant.

The pattern of coefficients on GDP per capita even though not significant suggest that the effect of country wealth is non-linear.[13] Going from the bottom quartile of GDP per capita to the next quartile *increases* physician migration by about 3 percent but subsequently the effect of increasing wealth is negative. This seems to align with previous work (Hatton and Williamson, 2005; Lucas, 2005) which has suggested that there might be an inverse U-shaped relationship between migration and per capita income, with less migration occurring at low levels of income per capita (because of binding liquidity constraints) and at high levels of income per capita (because of smaller gains from migration).

Neither growth nor lagged growth is significantly associated with physician migration in the OLS specification. In the fixed effects specifications however (columns 2-4), contemporaneous growth becomes statistically significant and the coefficients on lagged growth reverse sign and become negative. The estimates are robust to inclusion of year fixed effects (column 3) and country-specific time trends (column 4). The results suggest that, consistent with the model, economic shocks have a significant impact on physician migration. A one percentage point decrease in contemporaneous growth *increases* physician migration by between 0.4 and 0.6 percent and a one percentage point decrease in last year's growth *increases* physician migration this year by approximately 0.3 percent. This is a relatively small effect. It implies that a negative growth shock equivalent to 1 SD in this data will result in the migration of approximately one extra physician on average.

Relative to the fixed effects estimates, the OLS estimates, which one would obtain from a cross-country regression, not only understate the impact of migration by about a factor of ten, they, in the case of lagged growth, also have the wrong sign. These basic pattern of results continues to hold when we look at US migration separately from UK migration (Table 2.5 and Table 2.6). Negative economic shocks lead to increased physician migration. Economic shocks appears to impact migration to the US and the UK similarly. For the other coefficients though I find heterogeneous impacts: lagged war for example significantly decreases migration to the US at the same time as it increases migration to the UK. Not surprisingly, the effect of $BRITISH$ is about 1.5 times larger in the UK regression. In other words, being an ex-British colony has a larger effect on migration to the UK than it has on migration

[13]The lack of significance is perhaps not surprising given that there isn't a tremendous amount of variation in GDP per capita in this sample.

to the US. Notice also that in the UK regression (Table 2.6 column 1), distance is now statistically significant. The point estimate implies that if we have two countries A and B that are similar in every respect except that B is further away from the UK than A by 1000 kilometers, then B will have 16 percent less physician to the UK than A.

Even though coups (which acts as a proxy for political instability) on average do not seem to have a statistically significant effect on migration in any of the models, it turns out that is because they have heterogenous effects. Lagged coup attempts (which imply less political stability) do infact increase physician migration as hypothesized, but only in richer countries. I estimate the models in Table 2.4 separately for each GDP quartile and find that for countries in the top two income quartiles, coups in the previous period (successful or not) increase physician migration in the present period by between 14 and 18 percent ($p<0.05$).[14]

In Table 2.7 I present an alternative specification. The dependent variable here is log migration per 100,000 domestic population. The difference between this and the previous specification is that migration here is scaled by each country's population. Total migration is in column 1, migration to the US is in column 2 and migration to the UK is in column 3 and all models include country-specific time trends which are the preferred specification. Overall, we see that the results continue to hold. A one percentage point decline in last year's growth increases physician migration per 100,000 by 0.05 percent. Or to put it differently, a negative growth shock equivalent to 1 SD will result in an extra 0.13 physicians per 100,000 population migrating.

For illustrative purposes, in Figure 4.4, I graph the impulse response function for the median country in the sample, Malawi. The underlying model is a vector autoregression model with two lags. It plots out the full adjustment path for migration following a permanent one percentage point shock to growth in time t. We see that a negative (positive) shock to growth in year 0 increases (decreases) migration in years 1 through 3. The peak is in year 1 with an increase (decrease) in physician migration of about 3 percent. By year 4 however, migration returns to pre-shock levels. We have a short time-series with only 25 observations and so the confidence interval includes zero but this graph serves to illustrate the point.

Even though I control for unobserved time-invariant country characteristics by including country fixed effects and allow for country-specific time effects, it is still possible that potentially important variables which belong in this model have been omitted. If these variables are correlated with both growth and physician migration,

[14]Those results are available from the author on request.

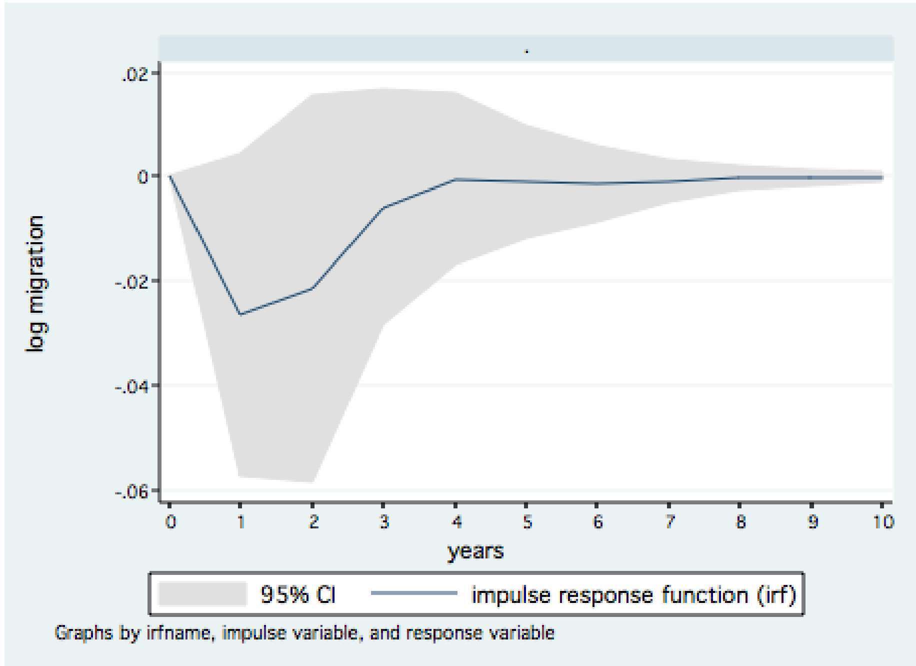

Figure 2.4: Impulse Response Function

then the estimates reported are biased. One can think of variables which might belong in this model which are not included. For example, income inequality which theory predicts will impact migration and which is also correlated with economic growth. Immigration policy is another example. Clearly destination country immigration policy will affect the flow of migrants and it may very well be correlated with economic growth in the source country. For example, UK immigration policy is more accommodating towards physicians from EU countries relative to physicians from non-EU countries and EU countries on average experience better economic performance. To the extent that these omitted variables are time-invariant or at least change very slowly (and over the long run), then they are captured by the country fixed effects. If they vary over time in a way that is not markedly different across the countries of origin in the sample, then they are subsumed to a large extent by the time fixed effects.

Another reason why the error term might still be correlated with growth is measurement error. Previous work has raised the issue of measurement error in national accounts data (Heston, 1994). If GDP per capita is measured with error, then growth is also measured with error, which will tend to bias our coefficients towards zero if the measurement error is classical. This may explain the small coefficients I find. There are several ways one could account for both these concerns. One approach would be to use instrumental variables. If for example, I had another variable that was correlated with growth but not with the error term, in other words an instrument, then I could get an unbiased estimate of the effect of economic growth on physician migration.

2.6 Instrumental variables

I instrument for growth with rainfall and terms of trade. Rainfall is plausibly exogenous and has become quite a popular instrument in the development literature (Miguel and Kremer, 2004; Paxson, 1992; Ciccone and Brückner, 2008). It is particularly appropriate in this sample because agriculture is a major component of GDP for many African countries, but at the same time, irrigation farming is only a small proportion of agriculture in Africa (most agricultural land is rain-fed). Rainfall data comes from the NASA Global Precipitation and Climatology Project (GPCP). This measures monthly rainfall in millimeters and is a combination of rainfall gauge data and satellite data. The rainfall series starts in 1979.

Following methodology described in Miguel and Kremer (2004) I calculate total yearly rainfall for each 2.5-latitude/longitude degree node within a country and then aggregate data over all the country nodes to get yearly rainfall estimates for each country. The rainfall instruments are rainfall growth in year t, i.e. percentage increase in rainfall in mm between $t-1$ and t; rainfall growth lagged one year and rainfall growth lagged two years. Other specification of rainfall including rainfall levels, rainfall levels with various lags, flexible polynomials of rainfall and mean deviations of rainfall resulted in a weaker first stage.

A country's terms of trade are the ratio of its export price index to its import price index. Terms of trade are plausibly exogenous because they depend on movements in commodity prices, which are in turn determined by world aggregate demand and supply conditions. The exogeneity argument of course relies on the assumption that an individual countrys decisions are unlikely to significantly affect world market prices. Note that this assumption may be less true for large countries like the US and China, which can single-handedly affect commodity prices, but it is likely to hold for the countries in our sample.

Terms of trade data come from two sources: the primary source is the World Development Indicators and for most countries data is available from 1980.[15] and the other dataset which I use to fill in missing observations, mostly from 1975-1980,

[15] The terms of trade variable in the WDI is called *net barter terms of trade* (base year = 2000).

is the New York University Development Research Institute (DRI) macro time series dataset.[16] I instrument for growth using the percentage change in net barter terms of trade. First stage regressions are reported in Table 2.8 and the IV results are contained in Table 2.9. Because growth $(t-2)$ is small and insignificant across all the models and does not markedly improve model fit, I opt for the more parsimonious specification including only one lag of growth.

The results in Table 2.8 show a strong relationship between current and lagged rainfall growth and economic growth. The point estimate implies that a 5 percentage point increase in rainfall growth increases economic growth by approximately 0.25 percentage points. These results are robust to inclusion of various fixed effects. In all models, I cluster standard errors at the country level to account for within group correlation. Because the model is over-identified (I have two endogenous variables and four instruments), I can test the validity of the over-identifying restrictions. I report various test statistics including the Anderson-Rubin F-statistic (Anderson and Rubin, 1949) and the Sargan-Hansen statistic. The Anderson-Rubin test statistic has a chi-square distribution with N degrees of freedom where N is equal to the number of instruments and is weak instrument robust. The null hypothesis is that the coefficients on the excluded instruments are jointly equal to zero in the reduced form equation with the full set of instruments included as instruments. In all specifications I fail to reject the null. The Sargan-Hansen test is a test of over-identifying restrictions. It also has a chi-square distribution and tests the null hypothesis that the instruments are correctly excluded from the estimated equation. Again in every specification, I fail to reject the null, which suggests that the instruments are valid.

I test for the strength of the excluded instruments and report the F-statistic for the Kleibergen-Papp Wald test, preferred here to the Cragg-Donald F-statistic, because it remains valid in the presence of non-i.i.d errors (Kleibergen and Paap, 2006). The Cragg-Donald F-statistic relies on the i.i.d assumption and is problematic in this case because I cluster the standard errors. One concern here is the strength of the instruments: it is now fairly well known that 2SLS estimates are biased in finite samples and that this bias can be quite large when the correlation between the endogenous variable and the instruments is weak (Nelson and Starz, 1990; Bound et al., 1995) . It has also been shown that the sampling distribution of 2SLS estimates is non-normal and that the standard hypothesis tests are unreliable in the presence of weak instruments (Stock et al., 2002).

[16] Publicly available at:
http://www.nyu.edu/fas/institute/dri/globaldevelopmentnetworkgrowthdatabase.html.

Stock and Yogo (2005) in their seminal work, derive critical values for assessing whether instruments are weak, and rejection of the null relative to the critical value implies that the bias in the coefficient estimates is probably small and that the usual tests of inference are valid. In the case of multiple endogenous variables, the critical values are based on the Cragg-Donald F-statistic, which as earlier mentioned, relies on the assumption of homoskedastic errors. It is unclear the extent to which those critical values generalize to the case of serial correlation and/or heteroskedasticity in the error term, but to address concerns about the strength of the instruments, I re-estimate the model using the Fuller LIML estimator, which is more robust to weak instruments (Hahn and Hausman, 2003; Stock et al., 2002). The Fuller k-estimator sets $k = k_{LIML} - \frac{b}{T-K}$ where K is the number of instruments (Fuller, 1977) . Following most of the literature I set the Fuller parameter b equal to 1

Results using the Fuller estimator are reported in Table 2.9 (column 4). The coefficient estimates are virtually identical to those in column 3 and our conclusions remain unchanged. I estimate each model separately for US and UK migration and our pattern of results continue to hold, which is reassuring. Results are in Table 2.10 and Table 2.11.

Overall I find that IV estimates are several orders of magnitude larger than the OLS estimates suggesting that our OLS estimates were biased towards zero. The signs are in the expected direction and the coefficient on lagged growth is statistically significant. The point estimate implies that a one percentage point decrease in lagged growth increases physician migration by between 3.4–3.6 percent. The coefficients on lagged growth again have similar magnitudes for US and UK migration. I find no effect of contemporaneous growth shocks.

2.6.1 Robustness Checks

It is possible that South Africa, which by far has the largest number of migrating physicians, is driving the results of this analysis and so I omit South Africa from the sample (results not shown). The results are essentially unchanged. I also drop countries one by one and the results are similar. I go on to test whether the results are robust to excluding all country-year cells with zero out-migration (those results are available on request) and find that the qualitative conclusions remain the same. Next, because the war variable might be collinear with the coup variable, I include only WAR or only $COUP$ in other specifications and the results hold.

In other analysis, I explore whether the impact of growth is non-linear. It is possible for example that negative economic shocks have different impacts from positive shocks. In columns 1-2 Table 2.12, I have restricted the sample to include only observations where growth is negative and in columns 3-4 I have restricted the sample to include only observations where growth is positive. Both models include country-specific time trends. I find that the coefficients are roughly similar in magnitude even though the 2SLS estimates are not significant, probably as a result of the small sample size.

I also examine whether the effect of growth depends on the level of income. One way to do this would be to interact growth with GDP per capita levels, but I choose instead to estimate the model separately for each quartile of income. This is a more flexible specification because it allows the effect of each explanatory variable to also depend on income. This amounts to a fully interacted model where all the variables are interacted with income levels.[17] In Table 2.13 Columns 1–4, I present the fixed effects specification and find a positive coefficient on lagged growth for the lowest income quartile which suggests that for the lowest income countries, growth may infact increase migration. It is statistically indistinguishable from zero however. In Column 5 I present the 2SLS equivalent of Column 1 i.e. the coefficients from the 2SLS regression for the lowest income countries and show that the sign becomes negative and similar in magnitude to the coefficients in Table 2.9.

2.7 Conclusion

This is one of the first papers to systematically examine one of the push factors often mentioned as contributing to health professional migration. I estimate distributed lag fixed effects regressions controlling for time-invariant country characteristics and time trends, and I also estimate instrumental variable regressions to account for unobserved heterogeneity and measurement error. Overall I find a statistically and substantively significant effect of economic shocks on physician migration. According to the IV estimates, a one percentage point decline in lagged economic growth causes an increase in physician migration by between 3.4–3.6 percent and these results are robust to a variety of specifications and robustness checks. This paper presents fairly compelling evidence that short-term economic shocks have indeed contributed to the documented acceleration in physician migration to developed countries from sub-Saharan Africa.

[17] As discussed earlier, I found that the effect of some of the explanatory variables also varied by country wealth.

Table 2.1: Physician migration from 31 countries

country	**US**	**UK**	**Total**	**Per capita**	**2004 Stock**[1]
Angola	2	1	3	.019	8
Benin	6	0	6	.073	4.5
Burundi	8	1	9	.12	2.8
Cameroon	159	29	188	1.2	19
Congo	3	0	3	.077	20
Cote D'Ivoire	0	3	3	.017	12
DRC	37	8	45	.081	11
Ethiopia	594	52	646	.92	2.7
Ghana	927	553	1480	6.8	15
Guinea	7	0	7	.079	11
Kenya	512	178	690	2.1	14
Liberia	67	27	94	2.9	3
Madagascar	17	0	17	.094	29
Malawi	13	33	46	.36	2.2
Mali	1	0	1	.0076	7.9
Mauritius	58	0	58	4.7	106
Mozambique	5	3	8	.041	2.7
Niger	3	0	3	.022	3
Nigeria	3509	3533	7042	5.1	28
Rwanda	6	0	6	.068	4.7
Senegal	9	2	11	.097	5.7
Seychelles	4	0	4	4.8	151
Sierra Leone	71	13	84	1.6	3.3
Somalia	53	8	61	.77	4
South Africa	2936	15356	18292	39	77
Sudan	0	1111	1111	3.1	22
Tanzania	213	86	299	.8	2.3
Togo	4	0	4	.067	4.5
Uganda	237	148	385	1.4	8.3
Zambia	118	203	321	2.8	12
Zimbabwe	231	369	600	4.6	16

[1] Number of domestic physicians per 100,000 population

Table 2.2: Economic Growth (1975-2004)

country	**mean**	**sd**	**N**	**fracneg***
Angola	.01	7.7	24	.29
Benin	.32	3.3	30	.37
Burundi	-.4	4.6	30	.6
Cameroon	1.1	6.6	30	.33
Congo	.55	6.7	30	.5
Cote D'Ivoire	-1.5	4.6	30	.67
DRC	-4.3	4.9	30	.83
Ethiopia	.21	7.9	23	.52
Ghana	-.011	4.6	30	.23
Guinea	.67	1.5	25	.36
Kenya	.27	2.3	30	.47
Liberia	-3.5	24	30	.67
Madagascar	-1.5	4.6	30	.63
Malawi	.3	5.4	30	.37
Mali	1.1	5.7	30	.4
Mauritius	4.2	1.7	24	0
Mozambique	1.9	7.5	24	.38
Niger	-1.3	5.5	30	.63
Nigeria	-.25	5.3	30	.53
Rwanda	1.4	12	30	.43
Senegal	.39	4.2	30	.37
Seychelles	2.6	6.8	30	.43
Sierra Leone	-.7	7.9	30	.47
Somalia	.75	8.7	16	.5
South Africa	-.0071	2.3	30	.43
Sudan	1.9	5.8	30	.3
Tanzania	1.3	2.3	16	.25
Togo	-.61	6.4	30	.5
Uganda	1.9	3.2	22	.14
Zambia	-1.5	4	30	.6
Zimbabwe	-1.2	5.7	30	.63
Total	.042	7.3	864	.45

* Fraction of years with negative growth

Table 2.3: Summary Statistics

variable	**mean**	**sd**	**min**	**max**
Growth (t)	-.143	7.46	-50.5	89.8
Growth (t-1)	-.18	7.53	-50.5	89.8
Growth (t-2)	-.215	7.6	-50.5	89.8
War (t)	.125	.331	0	1
War (t-1)	.125	.331	0	1
Pop/100,000	155	193	.617	1280
Coup (t)	.0787	.269	0	1
Coup (t-1)	.0801	.272	0	1
British	.41	.492	0	1
Doctor stock in 1975	8.84	10.3	1.16	53.9
Real GDP in 1975	1805	1464	489	6874
GDP 1st quartile	787	189	489	1042
GDP 2nd quartile	1181	68.9	1085	1291
GDP 3rd quartile	1520	205	1370	1991
GDP 4th quartile	4013	1732	2093	6874
Distance (US)/1000	11.9	2.35	7.83	16.5
Distance (UK)/1000	6.47	1.63	4.32	9.89
Rainfall in mm (t)	1072	489	132	2588
Rainfall (t-1)	1078	491	132	2588
Rainfall growth (t)	.874	18.9	-55	89
Rainfall growth (t-1)	.893	19	-55	89
Rainfall growth (t-2)	1.08	18.9	-55	89
Change in Terms of trade	.335	17.8	-56.8	102

Table 2.4: Fixed Effects Models - Total Migration (Dependent variable is log migration)

	(1) OLS	(2) FE	(3) FE	(4) FE
Growth (t)	-.00067	-.0043**	-.0045*	-.0057***
	(.0031)	(.0019)	(.0025)	(.0015)
Growth (t-1)	.00029	-.0025*	-.0024	-.0032**
	(.002)	(.0013)	(.0017)	(.0016)
Growth (t-2)	.00082	-.00057	.00057	-.00065
	(.0041)	(.0029)	(.0026)	(.0033)
War (t)	.073	.058	.052	-.039
	(.12)	(.12)	(.13)	(.092)
War (t-1)	-.011	-.06	-.031	-.16
	(.13)	(.14)	(.14)	(.11)
Coup (t)	-.0067	-.14	-.16	-.057
	(.13)	(.12)	(.11)	(.062)
Coup (t-1)	.083	-.068	-.078	.0081
	(.12)	(.1)	(.089)	(.047)
Distance in km/1000	-.091			
	(.059)			
British	1.44***			
	(.27)			
Doctors per 100,000 pop in 1975	.072***			
	(.0099)			
Real GDP in 1975* (2nd quartile)	.28			
	(.26)			
Real GDP in 1975 (3rd quartile)	-.17			
	(.35)			
Real GDP in 1975 (4th quartile)	-.088			
	(.32)			
Country Fixed Effects	No	Yes	Yes	Yes
Year Fixed Effects	No	No	Yes	No
Country-Specific time trend	No	No	No	Yes
N	712	712	712	712
R^2	0.83	0.22	0.29	0.43
AIC	1579	908	881	685

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
All standard errors are clustered at the country level
* Comparison group is countries in the bottom quartile of income

Table 2.5: Fixed Effects Models - Migration to the US (Dependent variable is log migration)

	(1) OLS	(2) FE	(3) FE	(4) FE
Growth (t)	-.0033	-.0039*	-.0042**	-.0042**
	(.0031)	(.002)	(.002)	(.0018)
Growth (t-1)	-.0019	-.0018	-.0016	-.0018
	(.0029)	(.0014)	(.0017)	(.0011)
Growth (t-2)	-.0041	-.0019	-.00011	-.0013
	(.0051)	(.004)	(.0032)	(.0048)
War (t)	-.25	-.0094	.0077	-.14
	(.23)	(.12)	(.12)	(.11)
War (t-1)	-.24	-.15*	-.076	-.25***
	(.16)	(.08)	(.076)	(.083)
Coup (t)	.057	.006	-.0085	.02
	(.12)	(.07)	(.064)	(.058)
Coup (t-1)	.017	-.023	-.047	-.0068
	(.13)	(.096)	(.089)	(.063)
Distance in km/1000	-.044			
	(.044)			
British	.84***			
	(.29)			
Doctors per 100,000 pop in 1975	.054***			
	(.0083)			
Real GDP in 1975* (2nd quartile)	.0019			
	(.36)			
Real GDP in 1975 (3rd quartile)	-.17			
	(.29)			
Real GDP in 1975 (4th quartile)	-.15			
	(.28)			
Country Fixed Effects	No	Yes	Yes	Yes
Year Fixed Effects	No	No	Yes	No
Country-Specific time trend	No	No	No	Yes
N	712	712	712	712
R^2	0.68	0.060	0.18	0.30
AIC	1707	968	910	756

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
All standard errors are clustered at the country level
* Comparison group is countries in the bottom quartile of income

Table 2.6: Fixed Effects Models - Migration to the UK (Dependent variable is log migration)

	(1) OLS	(2) FE	(3) FE	(4) FE
Growth (t)	-.002	-.0037	-.002	-.0048*
	(.0038)	(.0031)	(.0034)	(.0025)
Growth (t-1)	-.0012	-.0021	-.0011	-.0029**
	(.0019)	(.0015)	(.0016)	(.0014)
Growth (t-2)	.0036	.0027	.0039	.00042
	(.0024)	(.0022)	(.0024)	(.0019)
War (t)	.26	.13	.11	.17
	(.16)	(.16)	(.15)	(.12)
War (t-1)	-.0071	-.042	-.091	.033
	(.2)	(.18)	(.18)	(.14)
Coup (t)	-.14	-.22	-.22	-.063
	(.16)	(.14)	(.13)	(.089)
Coup (t-1)	-.035	-.11	-.087	.025
	(.11)	(.071)	(.075)	(.047)
Distance in km/1000	-.16**			
	(.068)			
British	1.2***			
	(.21)			
Doctors per 100,000 pop in 1975	.081***			
	(.011)			
Real GDP in 1975* (2nd quartile)	.28			
	(.25)			
Real GDP in 1975 (3rd quartile)	-.36			
	(.29)			
Real GDP in 1975 (4th quartile)	-.089			
	(.27)			
Country Fixed Effects	No	Yes	Yes	Yes
Year Fixed Effects	No	No	Yes	No
Country-Specific time trend	No	No	No	Yes
N	712	712	712	712
R^2	0.80	0.37	0.43	0.64
AIC	1524	936	909	542

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All standard errors are clustered at the country level

* Comparison group is countries in the bottom quartile of income

Table 2.7: Fixed Effects Models - Dependent Variable is log migration per 100,000 population

	(1) FE(total)	(2) FE(US)	(3) FE(UK)
Growth (t)	-.00053	-.00023	-.00037
	(.00039)	(.00025)	(.0003)
Growth (t-1)	-.0005**	-.00027**	-.00029*
	(.00022)	(.00011)	(.00017)
Growth (t-2)	-.00055	-.00054	-5.1e-05
	(.00039)	(.0004)	(7.1e-05)
War (t)	-.0025	-.012	.0079
	(.0071)	(.0094)	(.0071)
War (t-1)	-.051	-.026	-.036
	(.041)	(.017)	(.037)
Coup (t)	.0035	.0065	-.003
	(.006)	(.0057)	(.0039)
Coup (t-1)	.0042	.0024	.0012
	(.0062)	(.0052)	(.004)
N	712	712	712
R^2	0.21	0.29	0.36

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
All standard errors are clustered at the country level
All models include controls for country-specific time trends

Table 2.8: First-Stage Results for Growth

	(1) FE	(2) FE	(3) FE
Rainfall Growth (t)	0.052***	0.044**	0.052***
	(0.015)	(0.019)	(0.015)
Rainfall Growth (t-1)	0.060***	0.054**	0.059***
	(0.015)	(0.020)	(0.017)
Rainfall Growth (t-2)	0.046***	0.022	0.046***
	(0.013)	(0.017)	(0.015)
Percent change in terms of trade	0.014	0.017	0.015
	(0.015)	(0.016)	(0.017)
N	544	544	544
Partial R2	0.036	0.024	0.036
F	6.64	3.21	5.69

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
All standard errors are clustered at the country level
F-statistic on excluded instruments

Table 2.9: 2SLS Models - Total Migration

	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) Fuller LIML
Growth (t)	.0033	.029	-.0017	-.0022
	(.018)	(.023)	(.017)	(.016)
Growth (t-1)	-.023	-.011	-.036**	-.034**
	(.014)	(.018)	(.016)	(.014)
N	544	544	544	544
Kleibergen-Papp F-stat	4.95	3.49	5.33	5.33
Hansen J-stat	0.035	0.32	0.011	0.011
Hansen chi2 P-value	0.98	0.85	0.99	0.99
Anderson-Rubin F-stat	0.95	0.71	2.01	

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
All standard errors are clustered at the country level

Table 2.10: 2SLS Estimates - Migration to the US

	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) Fuller LIML
Growth (t)	-.023	.012	-.016	-.016
	(.026)	(.022)	(.027)	(.026)
Growth (t-1)	-.026**	-.026	-.027**	-.026**
	(.012)	(.017)	(.013)	(.012)
N	544	544	544	544
Kleibergen-Papp F-stat	4.95	3.49	5.33	5.33
Hansen J-stat	1.11	0.45	0.76	0.78
Hansen chi2 P-value	0.57	0.80	0.68	0.68
Anderson-Rubin F-stat	2.06	1.14	2.01	

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
All standard errors are clustered at the country level

Table 2.11: 2SLS Estimates - Migration to the UK

	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) Fuller LIML
Growth (t)	.033*	.033	.016	.017
	(.017)	(.022)	(.017)	(.017)
Growth (t-1)	-.0099	.0028	-.026*	-.027*
	(.015)	(.013)	(.013)	(.014)
N	544	544	544	544
Kleibergen-Papp F-stat	4.95	3.49	5.33	5.33
Hansen J-stat	2.26	3.42	1.44	1.37
Hansen chi2 P-value	0.32	0.18	0.49	0.50
Anderson-Rubin F-stat	2.17	1.98	2.35	

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
All standard errors are clustered at the country level

Table 2.12: Non-linearities in impact of growth shocks

	Growth<0		Growth>0	
	FE	2SLS	FE	2SLS
Growth (t)	-.013***	.03	-.0041	-.073
	(.0032)	(.033)	(.0036)	(.12)
Growth (t-1)	-.0054*	-.036	-.0031	-.046
	(.003)	(.024)	(.003)	(.045)
N	354	260	389	284

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
All regressions include controls for country-specific time trends
All standard errors are clustered at the country level

Table 2.13: Does the effect of growth depend on country wealth?

	(1)	(2)	(3)	(4)	(5)
	FE	FE	FE	FE	2SLS
Growth (t)	-.0042	-.012*	-.0059**	-.0062	-.00081
	(.0034)	(.0055)	(.0022)	(.0047)	(.038)
Growth (t-1)	.0014	-.012	-.0026	-.0046	-.045
	(.0027)	(.011)	(.0024)	(.0064)	(.035)
N	179	202	197	165	157

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
All regressions include controls for country-specific time trends
All standard errors are clustered at the country level

CHAPTER III

Doctors Across Borders: Do Higher Salaries Lead to Less Physician Migration?

3.1 Introduction

It is by now common knowledge that physician migration from sub-Saharan Africa has increased significantly over the last two decades. Quantitative estimates vary, but overall, the same pattern emerges across different studies (see for example Dovlo and Nyonator, 1999; Hagopian et al., 2004; Okeke, 2008). This trend of increasing migration has sparked a debate about the potential consequences of physician migration for the countries of origin. Hagopian et al. (2005) for example argue that unimpeded migration of a country's physicians may impair delivery of health services; Awases et al. (2004) posit that it may reduce the quality of care provided in these countries (perhaps because more care will be provided by less skilled professionals). A slightly different argument is that countries of origin suffer financial losses when domestically trained physicians emigrate. The training of physicians in Africa for the most part is publicly financed and it is estimated that the cost of training a medical student is somewhere in the neighborhood of $10,000 per student per year, in a region where the average per capita income was approximately $1000 in 2005 (World Bank, 2008). Dovlo and Nyonator (1999) for example estimate that between 1986 and 1995, Ghana lost an estimated US$6 million in tuition costs from the 61 percent of medical graduates from one of its medical schools that emigrated. Kirigia et al. (2006) calculate the cost of educating a doctor from primary school to university in Kenya and conclude that for each doctor who emigrates, the country loses approximately US$ 517,931 in returns from that investment.

Even though the debate about the consequences of physician migration is still ongoing, there is general agreement among policy makers that the scale and the extent of migration are worrying. Doctor-to-population ratios in many developing countries are already critically low and there is concern that migration of health professionals can only exacerbate these problems. In its 2006 Report, the World Health Organization (WHO) identifies some of the challenges to meeting the Millennium Development Goals and highlights the critical role of health professionals (WHO, 2006). In a bid to at least slow the migration of doctors and other health professionals from developing countries, various strategies have been called for. These strategies range from those which sensibly attempt to address what are thought to be the underlying causes of migration (such as low wages) to those which seek to place barriers in the way of health professionals who want to migrate.

One can think of these strategies as falling into one of two groups: those that accept migration as inevitable and therefore propose alternatives such as training of other cadres of health workers and mid-level providers (see for example Vaz et al., 1999; Dovlo, 2004); and those that seek to reduce/prevent migration through use of financial and non-financial incentives. See a discussion of some of these incentives in Bärnighausen and Bloom (2009). See also Hagopian et al. (2005). Most of the initiatives, I would argue, fall into the latter category. Despite their potential importance, little is known about the extent to which many of these incentive schemes work. There are few rigorous evaluations in the literature and much of the evidence that is available is anecdotal. In this paper, I make an initial contribution to filling what is a rather large gap in the literature by studying the impact of one commonly proposed mechanism: increasing doctors' salaries.

3.2 Previous Literature

There are numerous papers which discuss the factors that contribute to the migration of physicians and other health professionals (Bärnighausen and Bloom, 2009, provide a good overview). A survey of this literature soon reveals the fact that many researchers are in agreement that low wages play an important role in the migration of physicians. Dovlo and Martineau (2004) go as far as to call remuneration *the most* important factor for retaining health workers. Clearly, there is still disagreement about the importance of wages relative to other factors,[1] but it appears there is fairly broad consensus that increasing salaries are an important mechanism for slowing the migration of health professionals (see for example Pang et al., 2002; Mensah et al., 2005). Awases et al. (2004) provide some empirical evidence in keeping with this

[1] Rightly so as this is still an open question.

point of view. In a survey of health workers in six countries conducted in 2002,[2] between 68 percent (Cameroon) and 90 percent (Senegal) of health workers said they would not migrate if they were better paid. In another survey of health professionals conducted in five different countries (Colombia, India, Nigeria, Pakistan and the Phillipines) by Astor et al. (2005), 91 percent of respondents rated a desire for higher income as a highly significant motivating factor for migrating abroad and more than 80 percent agreed that increasing physicians' salaries would be an effective way of reducing physician migration.

As it turns out, there aren't very many studies which directly examine the relationship between doctors' salaries and migration – Vujicic et al. (2004) is one exception – but there are papers which examine the impact of wage differentials more broadly, and one can certainly draw inference from these studies. Hatton and Williamson (2003) for example estimate the effect of wages on net migration using a panel dataset of twenty-one African countries covering the years 1977 to 1995. In their IV models, they find that a 10 percent increase in the foreign to domestic wage ratio for unskilled labor increases out-migration by nearly one per thousand of the population. Clark et al. (2002) find qualitatively similar results using a different panel dataset on migration to the US from 81 countries between 1971 and 1998. They find that a 10 percent decrease in a country's income per capita (relative to the US) increased migration to the US by about 6 percent. Ortega and Peri (2009) examine migration flows from 74 countries of origin into 14 OECD countries between 1980 and 2005 and find that income per capita differences have a positive and significant effect on bilateral migration flows. Increasing the income differential by $1,000 PPP increased migration flows by approximately 10 percent.[3]

When evaluated as a whole, the empirical evidence appears to be consistent with the various theoretical models which predict that wage differentials will have an impact on migration (see Borjas, 1987). This suggests that increasing doctors salaries should decrease the rate of migration. There are several reasons however why increasing salaries may not have an effect on the observed migration rate: (1) The existence of credit constraints in developing countries is fairly well established (see for example Diagne et al., 2000; McKenzie and Woodruff, 2006; De Mel et al., 2008; Banerjee and Duflo, 2008), and this might act as a binding constraint limiting physician migra-

[2] The six countries were Cameroon, Ghana, Senegal, South Africa, Uganda and Zimbabwe. In total, 2382 health professionals were interviewed, approximately 336 of whom were doctors.

[3] It is possible to frame physician migration as a locational choice problem. If one does, then there is an extensive literature on physician location choice within developed countries that becomes relevant. Ernst and Yett (1985) was an early influential piece of work in this area.

tion. There are large fixed costs associated with international migration, especially for physicians, who in addition to moving costs, need to also pay for licensing examinations which are often required to practice in a foreign country. The US medical licensing examinations for example, costs nearly 3000 US dollars (which amounts to nearly one year of wages for physicians in some developing countries) and the UK licensing exams though less expensive are not far behind. If salaries are sufficiently low as to make saving impossible or at least very difficult, then borrowing is the only other option for financing a move overseas. If credit constraints are present, then it is certainly possible that there are some doctors at the margin who would like to migrate, but who cannot because they cannot finance the move. This raises, at least in theory, the possibility that increasing salaries may actually lead to an *increase* in migration rather than a decrease, because by increasing salaries, we loosen the financial constraint on migration.

An intriguing alternative explanation for why raising salaries may lead to increased migration is suggested by Hagopian et al. (2005). They raise the possibility that increasing salaries may increase pass rates on foreign exams (and consequently increase migration) because doctors can substitute away from work hours towards study hours.

(2) It is also possible that destination country immigration restrictions act as the binding constraint. Most countries, we know, place limits on the number of immigrants allowed in over a given period. Imagine for example that every year m doctors would like to migrate from i to j but there are only pm visas available where $0 < p < 1$. If the number of visas that can be issued to doctors from country i is fixed and the number of doctors wishing to migrate from i to j strictly exceeds the number of visas available, then it is easy to show that under plausible conditions, increasing doctors' salaries may have no effect on the migration rate.

That immigration restrictions bind is unlikely to be true in practice however, because doctors often have multiple routes for emigration. Take the case of the US for example: in order to work in the US, the typical skilled worker needs a work visa, usually the H-1B (which is capped) but physicians in training can also work on a J-1 visa (for which there is no cap).[4] In addition, even though the number of work visas (H-1B) that can be issued each year is fixed, the cap does not apply to physicians who work at a not-for-profit hospital affiliated with an institution of higher education.

[4]The H-1B visa is a non-immigrant visa category which allows U.S. employers to temporarily employ foreign workers in specialty occupations. For 2009, the H-1B cap is set at 65,000 with an additional 20,000 visas available to those with a graduate degree from a US institution. The J-1 is an exchange visitor visa.

Keep in mind also that physicians are usually accorded special status when converting from temporary to permanent status.[5]

So even though at first glance it seems obvious that paying doctors more should reduce migration, more careful consideration reveals that in fact it is not so clear. Also as Vujicic et al. (2004) argue, if the wage premium is very high, then the wage elasticity of migration in that range might be very small.

3.3 A brief history of wages

Wage differentials between countries in sub-Saharan Africa and OECD countries are quite large. US physicians earn more than $100,000 annually (Guglielmo, 2003), while wages in the UK range from 23,000-42,000 pounds for junior and mid-level positions on average, to up to 60,000 pounds for specialists (Kangasniemi et al., 2007). Wages in Ghana and Nigeria (two prominent exporters of physicians) on the other hand, are estimated to range from $3,600–$12,000 per annum. As Kangasniemi et al. rightly point out, on average, prices are lower in sub-Saharan Africa (SSA) so that the true wage differentials are smaller if one takes into account purchasing power differences.

It is certainly plausible to think that low wages have contributed to the increase in physician migration. In many SSA countries real wages declined during the 1980s and 1990s. Lienert and Modi (1997) document the fact that between 1986 and 1996, real wages for civil servants fell in 26 of the 32 sub-Saharan Africa countries for which data were available. In Nigeria for example, the official monthly wage of senior civil servants in constant 1995 international dollars (adjusted for purchasing power) dropped from US$820 in 1980 to US$234 in 1993 (Oluwu, 1997). Perhaps not surprisingly we find a marked increase in migration over this period.

In recent years, several countries have attempted in various ways to increase the pay of health workers. Tanzania implemented the Selected Accelerated Salary Enhancement scheme (SASE) which began in 2001. This mostly affected doctors in senior administrative positions (Dominick and Kurowski, 2004). Uganda also increased health worker salaries in 2004, with junior doctors getting a 60% bump in pay (Dambisya, 2007). Kenya in 2002 introduced various allowances for health workers in the public sector which reportedly increased pay for doctors in some cases as much as three-fold (Mathauer and Imhoff, 2006) and in 2005, the government of

[5]In the US for example, physicians can apply for permanent residency (green card) under a special category defined by the U.S. Citizenship and Immigration Services (USCIS).

Swaziland increased pay for all workers in the civil service including health workers by 60% (Dambisya, 2007). This is despite the fact that we still do not know whether salary increases reduce health professional migration, and if they do, to what extent.

Some mostly anecdotal evidence suggests that increasing salaries may have reduced migration. In Kenya and Swaziland for example, raising salaries resulted in hundreds of doctors applying for civil service jobs (Mathauer and Imhoff, 2006; Kober and Van Damme, 2006) and reduced the number of health workers exiting from the public sector (Palmer, 2006), but there has been no systematic assessment of most of these retention schemes (WHO, 2006). In this paper I evaluate one such program implemented in Ghana, the Additional Duty Hours Allowance (ADHA) scheme. Using innovative new econometric techniques developed by Abadie et al. (2007), I obtain causal estimates of the impact of wages on physician migration.

3.4 Ghana's Medical Brain Drain

Ghana currently has four medical schools. On average somewhere between 70-150 doctors are produced each year from all the medical schools. According to data from the World Development Indicators, Ghana has a domestic stock of about 15 physicians per 100,000 people (about 2000 physicians in total).[6] Approximately two-thirds of these serve in the public sector whilst the remainder provide services in a variety of parastatals and in the private sector. Ghana is one of the sub-Saharan countries most affected by health professional migration. Studies on migration of doctors trained by the University of Ghana Medical School show that, in the ten years between 1986 and 1995, 61 percent of the output of the Ghana Medical School had left the country (Dovlo and Nyonator, 1999). If one expresses the number of Ghanaian doctors abroad as a fraction of the domestic doctor stock, then about 30 percent of the domestic physician stock in Ghana is working overseas (WHO, 2006). To make the situation even more dire, in the Awases et al. study cited earlier, about 62 percent of the health workers surveyed in Ghana were actively considering migrating. There is at least some suggestive evidence that migration may be taking a toll on delivery of health services. Nurse vacancy rates in the public health sector in Ghana increased by 100 percent between 1998 and 2002 while doctors' vacancy rates increased from 42.6 percent to 47.3 percent over the same period, despite supply rates of over 100 doctors per annum (Dovlo and Nyonator, 1999; Buchan and Dovlo, 2004).

[6] To put this in perspective, consider that the US has about 260 physicians per 100,000 people.

3.4.1 Ghana's ADHA Scheme

In response to major challenges in recruiting and retaining doctors, the Ghanaian Federal Government in 1998 instituted a scheme known as the Additional Duty Hours Allowance (ADHA) Scheme. It was introduced as a negotiated settlement to strikes by doctors over the issues of long hours and low pay.[7] The stated objectives of the ADHA scheme were to *compensate doctors for any additional hours worked beyond the standard 40 hours a week/160 hours a month* as well as to *motivate health workers for higher performance towards provision of improved quality care* (Ruwoldt et al., 2007). A memorandum of understanding was signed between the Ghanaian government and the Ghanaian Medical Association in December 1998 with implementation to begin the following year.

For political reasons, the ADHA was not explicitly referred to as a salary increase – by calling it something else, the government hoped to prevent other health workers from demanding similar increases. In reality however, that is exactly what it was. Doctors were allowed to claim reimbursement for up to 200 additional hours of work per month provided the work was duly authorized and documented, but in many cases doctors did not even have to submit claim forms to get reimbursed.[8] Even though precise estimates are hard to come by, several reports suggest that the ADHA scheme had a significant impact on doctors incomes increasing it by as much as 150 percent (Mensah et al., 2005). Most estimates put the increase in doctors incomes post-ADHA at between 75-150 percent.[9] Note that average monthly basic salaries in Ghana in 1999 ranged from $200-$300 depending on seniority (Dovlo and Martineau, 2004).[10]

The starting budget for the ADHA scheme was 3.7 billion cedis (approximately $1.6 million)[11] and over time as the program expanded to include nurses and other public health professionals, the budget grew to 800 billion cedis in 2005 (approximately $89 million). In 2005, the Ghanaian government discontinued the ADHA and the payments were folded into the base salary of health professionals.

How much of an impact did the ADHA scheme have on doctors' incomes? A 2005 survey of doctors in two regions in Ghana revealed average annual incomes of

[7] Note that it was not primarily intended as a strategy to reduce physician migration.

[8] The Ghanaian Medical Association publicly stated that its members were not required to complete duty rosters or submit claim forms.

[9] As a further sign of the impact it had, there is some evidence that the increase in incomes allowed some health workers to purchase homes for the first time (Ruwoldt et al., 2007).

[10] Basic salaries make up roughly between 30-60% of salaries. Various allowances make up the remainder.

[11] The exchange rate in 1998 was about 2300 cedis to $1.

approximately $14,000, nearly half (46%) of which were ADHA payments.[12] Although the ADHA was originally intended as a bonus for working extra hours, 97% of health workers surveyed described the ADHA as a fixed payment (Witter et al., 2007).

To date there has been no rigorous evaluation of the impact of the ADHA on physician migration. There is some (anecdotal) evidence that the number of newly trained doctors migrating may have declined following introduction of the ADHA, but a 2004 report by the Ministry of Health ADHA Task force was unable to document any reduction in migration rates of health professionals. This is not to say that it did not have other impacts. For example, there is some evidence that it improved job satisfaction and increased applications to nursing school (Mensah et al., 2005; Ruwoldt et al., 2007).

3.5 Methods

The problem here is to identify the effect of the wage increase on physician migration. Under normal circumstances, this sort of problem would lend itself to standard difference-in-difference regression methods but in this case as there is only one treated unit, identification is more complicated. The treatment here, a salary increase, is applied at the country level, and potentially all doctors in Ghana are exposed to the treatment. It seems reasonable therefore that the appropriate level of analysis is at the country level especially given the fact that the outcome of interest is international (cross-country) migration. Estimates obtained from a simple before and after analysis of migration rates are flawed for obvious reasons and so the usual methods to identify impacts of such programs rely on comparing the treated unit to some combination of untreated units. Ideally one wants a comparison unit that is as similar as possible to the treated unit.

One major drawback to the usual methods is that the choice of comparison group(s) often relies on the researchers' own subjective evaluation of which untreated unit(s) are the most similar, and therefore the most relevant for comparison, to the treated unit. Some studies avoid making a choice by simply using the set of all untreated units as the comparison group. If for example a policy intervention is applied at the state level, then all the other untreated states are used as the comparison. One of the key insights of Abadie et al. (2007), whose methods I discuss and then apply in this paper, is that a weighted average of untreated units, where higher weights are assigned to unexposed units that are more similar on explicit quantifiable dimensions to

[12]The exchange rate in 1998 was about 9000 cedis to $1.

the treated unit, results in a much better comparison group than one in which all the untreated units are essentially given the same weight. In the discussion which follows I summarize the key points from their 2007 paper with a few minor modifications.

Let J represent all the countries in the sample where $j \in \{1, \ldots, J\}$. Let E denote exposure to some policy intervention, P, and let U denote lack of exposure. Let T_0 represent the pre-intervention period and T_1 represent the post-intervention period where $t \in \{1, \ldots, T\}$ and $1 \leq T_0 < T$. Let Y represent the outcome of interest. Without loss of generality, if we assume that country 1 is the only country exposed to the policy intervention, P, then the impact of P is given by;

$$\alpha_{1t} = Y_{1t}^{E} - Y_{1t}^{U} \qquad \forall t > T_0$$

The problem is that we only observe the first term but not the second, often referred to as the *counterfactual*, because the country cannot be both exposed *and* unexposed at the same time. The key contribution of Abadie et al. (2007) is to show that under certain conditions $\sum_{j=1}^{J-1} w_j^* Y_{jt}$ is a good approximation of Y_{1t}^{U}. The w_js are an optimally chosen vector of weights where $w_j^* \in W^* = (w_1^*, \ldots, w_J^*)'$, $w_j^* \geq 0$ and $\sum_{j=1}^{J-1} w_j^* = 1$. The impact of the intervention, P, can therefore be estimated by;

$$\widehat{\alpha_{1t}} = Y_{1t}^{E} - \sum_{j=1}^{J-1} w_j^* Y_{jt} \qquad \forall t > T_0$$

The insight here is that from a weighted average of all the other unexposed countries in the sample, one can construct a counterfactual Ghana *without* the salary increase and then compare the migration rates in this *synthetic* Ghana with the actual migration rates observed in Ghana following the implementation of the ADHA scheme. This will allow us determine whether the salary increase had the desired impact. Obviously, a critical component of this is how the weights are determined. The basic intuition here is that the optimal vector of weights $\boldsymbol{W^*}$ is one which recreates, as closely as possible, the Ghana before the salary increase. More accurately, it attempts to match as closely as possible the values of a set of predictors of migration rates for Ghana before implementation of the ADHA scheme. More formally, $\boldsymbol{W^*}$ is chosen from the universe of all possible $\boldsymbol{W}$s in order to minimize the following function $\boldsymbol{(X_1 - X_0W)'V(X_1 - X_0W)}$ where $w_j \geq 0$ and $\sum_{j=1}^{J-1} w_j = 1$. $\boldsymbol{X}$ is a matrix of K country characteristics which predict migration; typically defined for $t \in \{1, \ldots, T_0\}$. The subscripts 0 and 1 denote unexposed and exposed countries respectively.

The variables included in $\mathbf{X}$ are guided by the literature on the determinants of physician migration and they include real GDP per capita, domestic physician stock and the lagged stock of physicians abroad (to capture network effects). Clearly, all determinants are not created equal and so a matrix ($\mathbf{V}$) assigns weights to each determinant in relation to how strongly it predicts migration rates. $\mathbf{V}^*$ is chosen to minimize the mean square prediction error of the estimator i.e. $\boldsymbol{E}[(\boldsymbol{Y_1}-\boldsymbol{Y_0}\boldsymbol{W^*})'(\boldsymbol{Y_1}-\boldsymbol{Y_0}\boldsymbol{W^*})]$. In the absence of strong priors regarding the relative importance of each predictor, $\mathbf{V}^*$ can be chosen to minimize $\boldsymbol{E}[(\boldsymbol{Y_1}-\boldsymbol{Y_0}\boldsymbol{W^*})'(\boldsymbol{Y_1}-\boldsymbol{Y_0}\boldsymbol{W^*})]$ for $t < T_0$ i.e. for the pre-intervention period.

3.6 Data

For this analysis, I use physician migration data collected by Alok Bhargava and Frederic Docquier which I will hereafter refer to as the BGD (see detailed description of this dataset in Docquier and Bhargava, 2007). The BGD measures the annual stock of physicians from developing country i in country j where J is a set of 16 OECD countries.[13] It is a panel dataset spanning the period 1991-2004. Descriptives are in Table 4.1.

The BGD represents an ambitious attempt to present a comprehensive picture of physician migration from the developing world, but like all pioneering efforts of this type it suffers from a number of limitations. First of all, the definition of the emigrant physician is not consistent thoughout the dataset. For six out of the sixteen countries: the US, UK, France, Canada, New Zealand and Norway, migrant physicians are defined based on the country of qualification (the data is obtained from medical registers); for six other countries: Australia, Belgium, Ireland, Denmark, Sweden and Austria, data on country of qualification is unavailable and so the definition of the migrant physician is based on the country of birth (data comes from national censuses and registers). Finally for the remaining four countries: Germany, Italy, Portugal and Switzerland, neither information on country of qualification nor information on country of birth is available and so migrant physicians are defined according to citizenship (data comes from a variety of sources). See Appendix Table 1. A second issue is the fact that even though the BGD purports to be an annual dataset, annual data is only available for a subset of the sample, and in the remaining cases is interpolated from t years of census data where $t \geq 2$ for all countries except Italy.[14]

These data problems present more or less of a challenge depending on what the

[13] Together these 16 countries account for more than 90 percent of all skilled immigrants in the OECD.

[14] Interpolation is done using a log-linear adjustment.

dataset is to be used for. Docquier and Bhargava (2007) argue that these issues present less of a concern for two reasons: (1) for the majority of the data (73 percent of the sample in 2004), migrant classification is in fact consistent – it is defined based on country of qualification; and (2) data on migration to the most important destination countries: the US, UK, Canada, New Zealand and Germany, which together account for about 75 percent of all the medical migrants, is in fact annual and does not require interpolation. I go further to argue that the BGD is the best data available for the analysis presented in this paper.

There are only a handful of datasets that measure migration from African countries to more than one destination country. Clemens and Pettersson (2008) is one example of a cross-sectional dataset.[15] To our knowledge there are only two panel datasets. One is the BGD, the other is Okeke (2008). The latter is a panel dataset somewhat similar to the BGD. It covers a longer time period but fewer destination countries. In addition, while the BGD measures the annual *stock* of physicians, Okeke (2008) measures the annual *flow* of physicians to its two destination countries, the US and the UK. Both concepts are related. To see this, let the stock of physicians from country i in country j at year t be S_{ijt} and let the net flow into j in year t be F_{ijt} where $F_{ijt} = I_{ijt} - O_{jt}$. I is the gross inflow from i to j in t and O is the outflow from j (this includes return migration to i as well as repeat migration to a different country $k \neq i$). Let D represent depreciation of the physician stock each year due to death, retirement, change of career etc. Then $S_{ijt} = S_{ij,t-1} + F_{ijt} - D_j$.[16] Notice that if the outflow from j is sufficiently small, then $F_{ijt} \approx \mathrm{I}_{ijt}$ so that $\Delta S_{ij} \approx I_{ijt} - D_j$. Notice that the change in the stock of doctors from country i in country j is an increasing function of the inflow of doctors from i. Notice also that if the depreciation factor is relatively constant (a not unreasonable assumption), then a decrease in I_{ij} in year t will have a permanent effect on S_{ij} because it not only affects S_{t+1}, but it also affects S_{t+n} for $n > 1$ through the relationship between current and lagged stock.

The BGD is preferred here primarily because it has wider coverage. It therefore increases the probability of finding a suitable weighted average of countries that closely resembles Ghana. In addition because it measures stock and not flow, the BGD offers another advantage; the stock of physicians in year t is almost invariably larger than the flow thus avoiding the problem of years with zero migration. It also helps that the year of the intervention sits almost exactly in the middle of the period covered by the

[15] OECD (2005) and Mullan (2005) are two other examples.

[16] This is analogous to writing $S_{ijt} = (1 - \delta_j)S_{ij,t-1} + F_{ijt}$ where δ_j is the country-specific depreciation rate.

BGD. In the analysis which folllows, I present results using the entire BGD sample and as a robustness check, I restrict the sample to the four OECD countries (USA, UK, Canada and France)[17] for which the emigrant definition is consistent (based on country of qualification) and for which there is annual data. I show that the results are not driven by choice of sample.

3.7 Results

If the salary increase had the anticipated effect of reducing physician migration, then we would expect to see a decrease in the outflow of physicians following the introduction of the ADHA in 1998. Using data from Okeke (2008), I graph the outflow of physicians from Ghana over time to the UK and compare it to the average outflow from other "untreated" SSA countries (see Figure 3.1). In Figure 4.3, I do the same for migration to the US. I focus only on the US and the UK because those two countries combined account for nearly 90 percent of all physician migration from Ghana. Notice in Figure 3.1 the upward trend prior to 1998. Beginning in 1998 however, we see that the number of Ghanaian physicians emigrating to the UK drops off sharply. The trend for the other countries mimics that of Ghana prior to 1998 but there is no drop-off in 1998. In fact average migration from the other countries remains fairly steady until 2004.

The picture for the US is slightly complicated by the fact that the US Educational Commission for Foreign Medical Graduates (ECFMG), the body which oversees certification of all foreign medical graduates, introduced an additional exam in 1998 that FMGs had to now take and pass before receiving certification. Prior to 1998, the US medical licensing exams (USMLE) consisted of two steps both of which could be taken in various centers around the world. The introduction of an additional exam - the Clinical Skills Assessment (CSA) - a clinical exam which could only be taken in the US, had the effect of increasing the cost of migration for FMGs. It not only cost more than the other two steps, it also required a trip to the US in order to take the exam. Given an increase in the cost of migration, one would expect a decrease in migration and this shows up in the data - for Ghana as well as for the other countries in the sample. Notice however that the drop-off for Ghana is steeper than the average suggesting that the introduction of the ADHA may have contributed to the overall decrease in migration to the US. Overall, this preliminary analysis provides the first evidence that the ADHA scheme may have reduced the migration of physicians from

[17] Hereafter referred to as Group 1 countries.

Ghana. Next we turn to our main analysis.

The predictors chosen were guided by the literature on the determinants of migration as well as by data availability. The variables used include log per capita incomes (to capture the benefits from migration – on average the wealthier the source country, the smaller should be the benefit from migrating), the domestic stock of physicians (to capture supply – *ceteris paribus*, countries with a larger domestic stock of doctors should have a larger foreign stock) and the number of coup attempts and a dummy for conflict (as proxies for political instability). I also include three lags of migrant stock to capture network effects. The values of the predictors for the real Ghana, the "synthetic" Ghana and a simple average of all the countries in the donor pool are contained in Columns 1, 2 and 3 respectively in Table 4.2. Data sources are described in Appendix A. We started out with 44 other African countries as potential candidates for the donor pool but excluded Lesotho and Swaziland because they had zero migration over the period covered in the data. Comoros and Sao Tome were also excluded because of missing data, and Tanzania and Kenya were excluded because similar programs were implemented there over the time period covered in this analysis. Lastly we exclude South Africa because it is in many ways atypical amongst sub-Saharan African countries.[18] The final sample consists of 37 countries. The primary outcome variable is $\sum_{j=1}^{J} S_{ijt}$, the total migrant stock of physicians from country i.

Table 3.1: Predictors

Variable	**Ghana**	**Synthetic**	**Average**
Log GDP per capita	7.08	6.63	7.17
Lagged foreign stock (1991)	423.09	423.22	88.99
Lagged foreign stock (1993)	513.17	517.96	110.85
Lagged foreign stock (1997)	586.78	587.18	130.05
Coup attempts	0.00	0.01	0.11
Conflict	0.00	0.34	0.20
Log domestic physician stock	1.86	1.52	2.01

All variables except lagged foreign stock are averaged for 1991-1997

Looking at the results in Table 4.2, we see that the synthetic Ghana formed from a weighted average of untreated countries is very similar to the real Ghana on a set of variables which predict migration. The one exception is the conflict variable which turns out to be a very weak predictor of migration rates and is thus assigned a small weight in the weighting matrix, $\boldsymbol{V}$. Ghana is one of the more stable countries in the

[18] Amongst other things, it is itself an important destination country (see Clemens, 2007).

sample and experiences no conflict between 1991 and 2004. We also notice that a simple average of the other countries in the pool does not provide a good comparison for the treated unit, Ghana. Notice that while an average of the other countries matches the pre-intervention values of Ghana on some of the predictor variables, it provides a very poor approximation on all of the lagged stock variables which are important predictors of migration rates. It is evident from Figure 3.1 and Figure 4.3 that Ghana in terms of raw numbers has a high physician migration rate relative to the average.

In Table 3.4 we show the optimal weighting matrix $\boldsymbol{W}^*$. The best approximation of Ghana is a weighted average of five SSA countries: Ethiopia, Nigeria, Sudan, Uganda and Zimbabwe. All the other countries are assigned a zero weight. In Figure 3.3, I plot the outcome for Ghana and the synthetic control for each year in the data. The results confirm what we already suspected from looking at flow data: the ADHA scheme appears to have had an impact on migration. Prior to 1998, migration in the synthetic control Ghana tracks very closely with actual migration but shortly after 1998, the lines start to diverge. We also see that the effect on foreign migrant stock is increasing, which is consistent with theory. This result is quite compelling. By 2004, there are approximately 100 fewer Ghanaian doctors abroad than there would have been if the ADHA program had not been implemented. This is not a trivial number. A decrease of 100 physicians amounts to roughly 16 percent of the mean foreign migrant stock (of Ghanaian doctors) in all 16 OECD countries, and approximately 7 percent of the mean domestic stock. Alternatively, relative to what the foreign migrant stock would have been in 2004 had the ADHA scheme not been implemented, this translates to a 10 percent decrease in emigration.

As a robustness check, and to address some of the data problems inherent with the BGD, we repeat the analyses for a smaller subset of OECD countries: the US, UK, Canada and France. For these four countries, the data is annual and the definition of the emigrant is consistent throughout the sample.[19] Results from a balancing test for this sample of four destination countries are in Table 4.3. With this sub-sample of the data, the weighting matrix assigns positive weights to Ethiopia, Nigeria, Madagascar, Uganda and Zambia and zero weights to all the other countries (see Table 4.4). Overall, the results are quite similar to results obtained using the larger sample. We find a decrease of about 130 physicians in the foreign migrant stock by 2004 relative to the counterfactual. This translates to an approximately 13 percent decrease in emigration.

[19]Emigrant classification is based on the country of qualification.

Table 3.2: Predictors (Group 1 countries alone)

Variable	Ghana	Synthetic	Average
Log GDP per capita	7.08	6.74	7.17
Lagged foreign stock (1991)	310	306.65	88.99
Lagged foreign stock (1993)	392	395.49	110.85
Lagged foreign stock (1997)	455	453.91	130.05
Coup attempts	0.00	0.07	0.11
Conflict	0.00	0.25	0.20
Log domestic physician stock	1.86	1.86	2.01

All variables except lagged foreign stock are averaged for 1991-1997

In alternative models, we use migrant stock per capita as the outcome variable i.e. $\sum_{j=1}^{J} S_{ijt}/Pop_{it}$. We also express the lagged foreign stock and the domestic physician stock in per capita terms. Results from this alternative specification (not shown) do not show the same striking pattern. It is not a priori clear though that expressing the migrant stock in per capita terms offers any significant advantages over expressing it in levels. While the per capita version of the variable allows for some comparability across countries, that is not the point of this analysis. At the same time, interpretation of the results when the outcome is expressed as a fraction of domestic population is less straightforward. Saying that the ADHA scheme reduced the foreign migrant stock by 25 doctors, we think, is more meaningful (and easier to understand) than saying that it reduced the foreign migrant stock by 0.5 doctors per 1,000 domestic population in Ghana.

Next we estimate a model in which all the comparison countries are weighted equally. This would be akin to using all the available untreated countries as a comparison group within the standard regression framework. The question we ask is "how would the results differ if we used all the available untreated countries as a comparison group?" Not surprisingly, the resulting synthetic control is a very poor approximation of Ghana and does not do a good job of reproducing the time path of the outcome variable before the intervention (results not shown). One cannot therefore say with confidence that the post-intervention differences in outcomes between Ghana and its comparison group are not as a result of underlying differences in characteristics.

One way of examining the robustness of our findings is to replicate the experiment in a different setting. The question we ask is "if we applied the same methods to a different country which implemented a similar type of program, would we find similar results?" To answer that question, we turn to Tanzania. Tanzania in 2001 implemented a program called the Selected Accelerated Salary Enhancement scheme (SASE) which also increased doctors' incomes (Dominick and Kurowski, 2004). If we are able to document similar findings for a very different country over a different time period, it adds some credibility to our findings for Ghana. We've chosen to use Tanzania over the other countries which implemented some type of salary support program for doctors because it gives us the longest post-intervention period (3 years). The purpose of this exercise is not to estimate the causal impact of the program in Tanzania – because amongst other reasons we do not have as good a grasp of the institutional details as we do for Ghana – all we are trying to do here is to see if we can replicate our findings in a different context. We use the same set of predictor variables, but include four lags of foreign stock (1993, 1996, 1998 and 2000). Those results are in Figure 3.4. We find a similar pattern. Both lines start to diverge about a year before the program start date.[20] An important caveat though is that these results are not as robust as our main results for Ghana. They are somewhat sensitive to the choice of which variables (or which lags) we include.

Conventional tests of significance cannot be used with the synthetic control method and so to evaluate the statistical significance of our results, we conduct a series of placebo studies in which we iteratively assign the treatment to the other countries in the donor pool. The idea is to generate a distribution of outcomes for the untreated countries to which we can then compare our results. If our results are unusually large relative to the results obtained for the untreated countries, then we conclude that the ADHA program had a statistically significant impact. The outcome here is α_{jt} i.e. the difference in the dependent variable between the treated unit j and it's synthetic control. In Figure 3.5 we graph the distribution of outcomes for all the countries in the sample and compare it to Ghana's. The decrease for Ghana (dotted line) is clearly unusual relative to the other countries. There is only one other country which has a decrease larger than Ghana's and that may simply be due to poor fit – notice that the pre-intervention gap is also large. This translates to a probability of 5.5% (2/36).[21]

[20]This would be consistent with an anticipatory effect. Because we do not know the institutional details, it may very well be that the government announced the program a year or two before actual implementation.

[21]We omit Nigeria because no convex combination of the other countries is able to approximate outcomes for Nigeria. This is not surprising because it has the highest foreign migrant stock outside of South Africa, which we already exclude. The root mean square prediction error (RMSPE) for the pre-intervention period is approximately 1,110.

3.8 Conclusion

Increasing the incomes of health professionals in developing countries is often mentioned as a way to reduce emigration to richer countries. To our knowledge, this study is the first to provide estimates of the impact of increasing doctors' salaries. While the earnings differential between physicians in developing and developed countries is clearly an important factor contributing to physician migration, there has before now been very little direct evidence concerning what the potential impact of increasing salaries might be. In this paper, we exploit a natural experiment to estimate the impact of wages on physician migration. Using innovative new methods developed by (Abadie et al., 2007) and applying it to data collected by Alok Bhargava and Frederic Docquier (2007), we find that six years after Ghana implemented a salary increase program known as the Additional Duty Hours Allowance (ADHA) scheme, the foreign stock of Ghanaian doctors abroad had fallen by approximately 100 physicians relative to what it would have been without the program. This translates to roughly a 10 percent decrease in emigration. As a way of estimating how significant this decrease was, we iteratively assigned the treatment to the other unexposed countries in the sample and show that the decrease in the foreign stock for Ghana is very unusual relative to the distribution of outcomes for the other countries.

Our work has a number of limitations. First, even though we conducted an extensive review of the literature to identify (and exclude) countries that implemented some kind of large-scale program between 1991 and 2004, it is possible that we still have included countries which implemented some kind of program to reduce physician migration. In other words, it is possible that some of the comparison countries are actually treated countries. This is not a problem if those countries are assigned a zero weight in the weighting matrix. If however, one of the countries assigned a positive weight is in reality a treated country, then our results will be biased towards understating the true effect.

Secondly, we are unable to control for any Ghana-specific shock which coincided with the start of the ADHA program. Our estimates of the impact of the program will be biased if there was some other unobserved shock in 1998 in Ghana that also affected physician migration.[22] The direction of the bias will depend on whether the unobserved shock is positively or negatively correlated with migration. If we had data for another professional group in Ghana which was not affected by the

[22] Personal communication from Ghanaian officials at the Ministry of Health assure us that this was not the case.

ADHA scheme (engineers for example), it would be possible to separately identify the effect of the ADHA program. Similar panel data for a different professional group to our knowledge is unavailable.[23] To assuage this concern however, we run the same experiment for Tanzania which implemented a similar type of program in 2001. We find some evidence again of a decrease in the foreign migrant stock. This suggests that our results for Ghana are not driven by some unobserved shock that coincided with the start of the ADHA.

Another concern is that the ADHA scheme may have had other components e.g. a subsidized loan to enable purchase of a car, so that all of the impact of the program could not be attributed to a salary increase. In actual fact, and this is unusual for a program of this size, the ADHA was purely a salary support program.[24]

When we look at all of the evidence, starting with the obvious reduction in the flow of physicians from Ghana beginning in 1998 when the ADHA was implemented (Figure 3.1 and Figure 4.3), to the decrease in the foreign stock of Ghanaian doctors that is evident from our main results (Figure 3.3) and ending with our robustness check which finds a similar pattern for Tanzania which implemented a similar type of program, the evidence is persuasive. It is hard to argue that the ADHA scheme did not reduce the emigration of physicians from Ghana. To the extent that one accepts our findings, it is possible to do a simple back of the envelope calculation to gauge the cost-effectiveness of the program. One can also calculate a rough elasticity. If the ADHA scheme increased salaries by between 75 and 150 percent and we find a 10 percent decrease in emigration, that translates to an elasticity of between -0.07 and -0.13.

There are clearly valid concerns about the political feasibility of increasing salaries of doctors; not to mention the economic ability of developing countries to do so. One problem is that it is a slippery slope: how does one increase salaries for one professional group without increasing salaries for other groups? As we discuss early in the paper, Ghana ran into exactly that problem and had to dramatically increase the budget to accommodate other health professionals. Any effort to evaluate the cost-effectiveness of such a program must therefore take this spillover effect into account.

[23]We thought about using nurses but they were also affected by the ADHA.

[24]Personal communication from the Ministry of Health, Ghana.

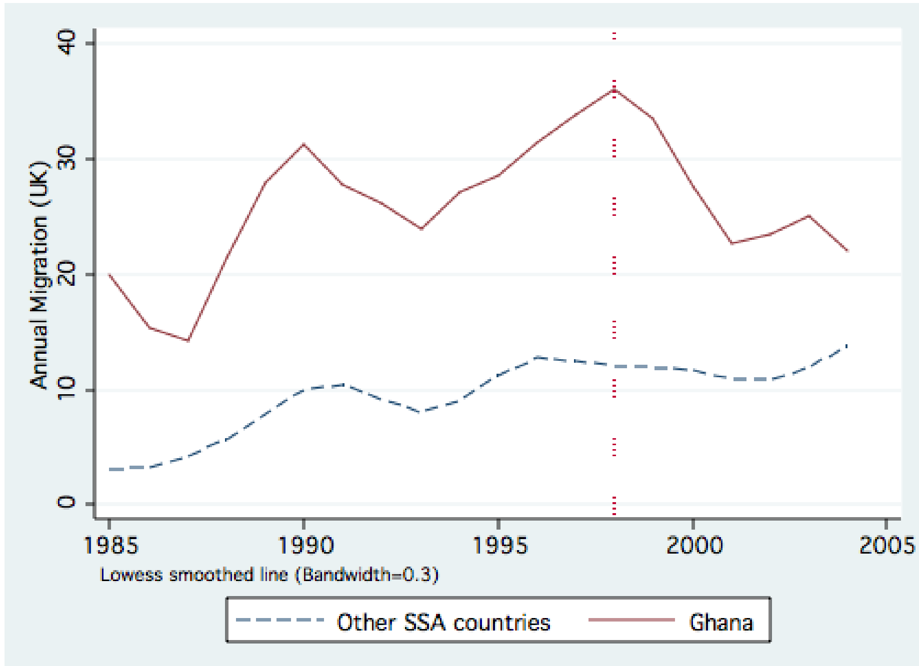

Figure 3.1: Physician Emigration to the UK

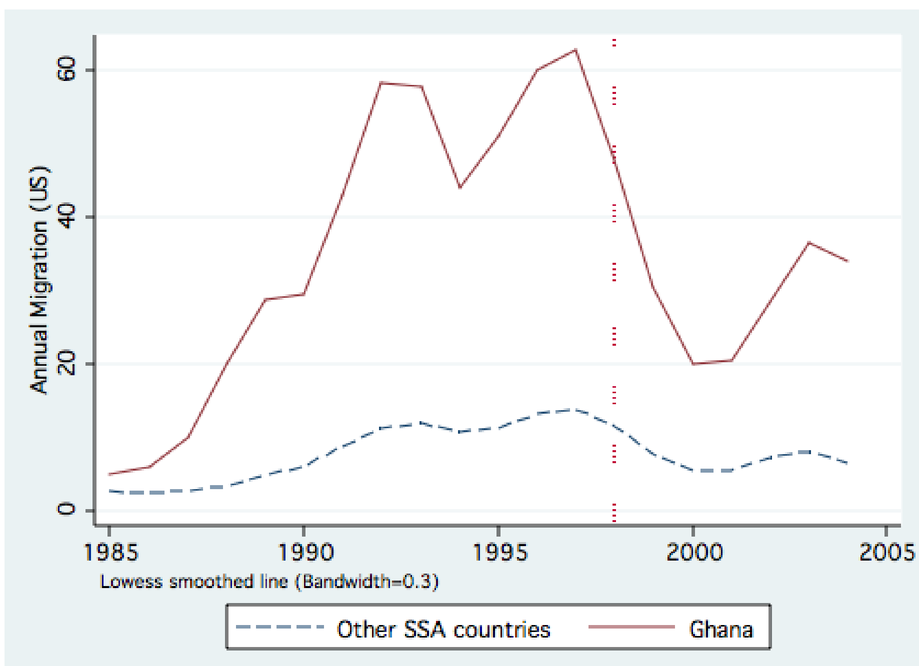

Figure 3.2: Physician Emigration to the US

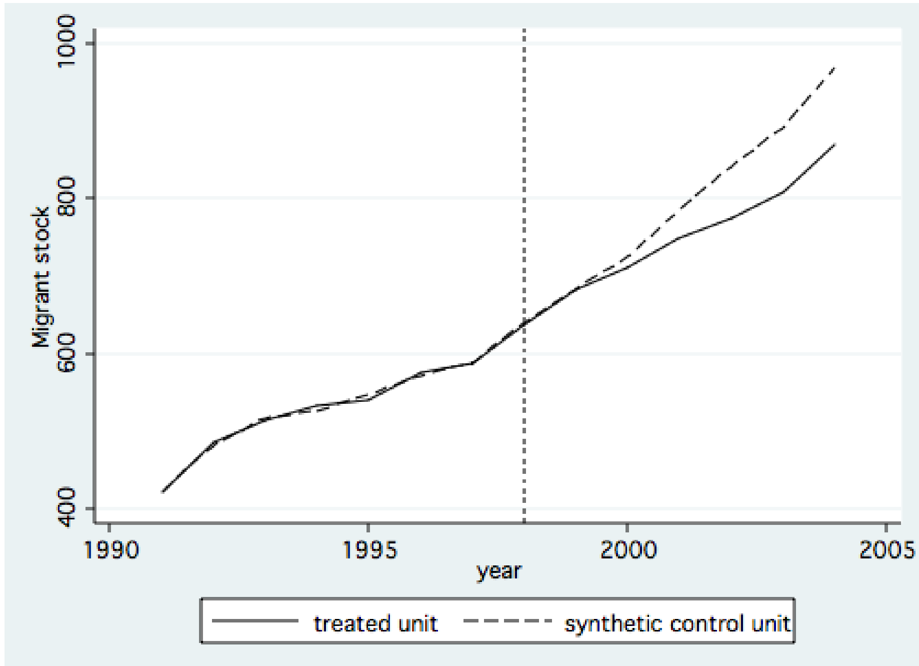

Figure 3.3: Did the ADHA scheme have an effect?

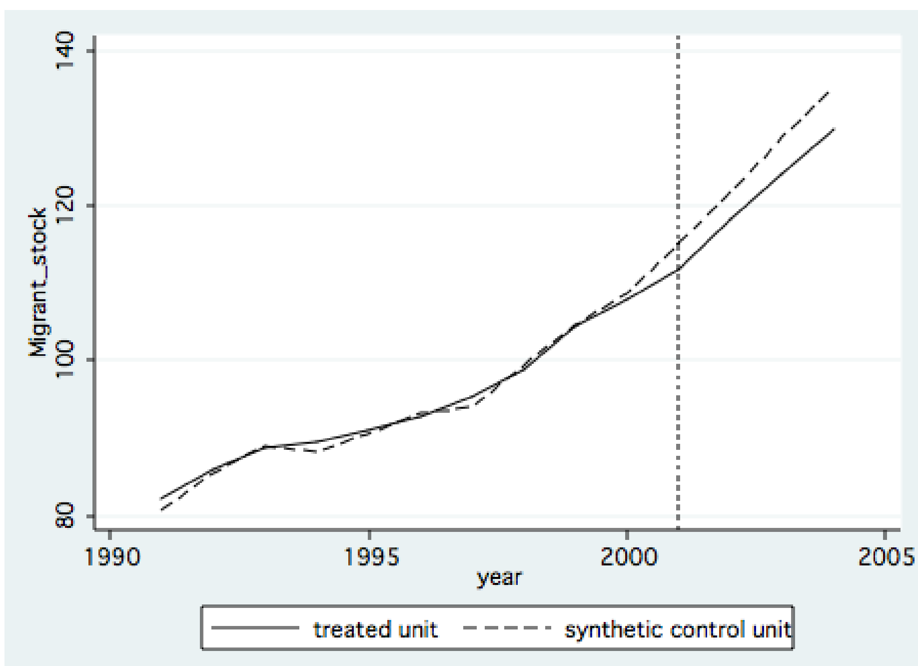

Figure 3.4: Do we find similar results for the SASE program in Tanzania?

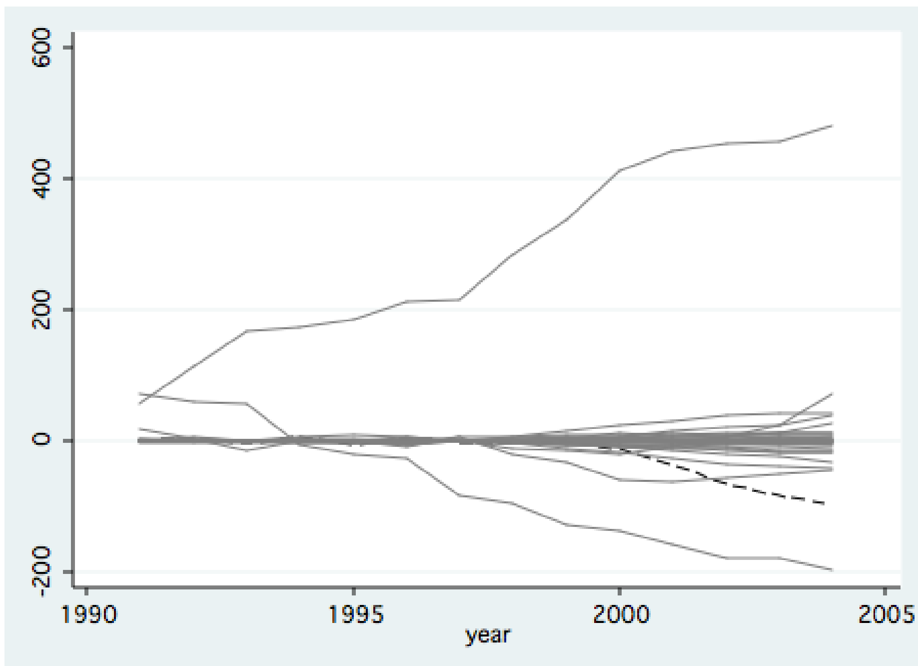

Figure 3.5: Placebo Test

Table 3.3: Descriptives

Country	**Foreign Stock**	**Fraction***	**Dom. Stock**	**Pop/1000**
Benin	9.5	.73	331	5843
Botswana	20	0	401	1566
Burkina Faso	1.8	.048	395	10641
Burundi	21	.12	352	6470
CAR	4	.7	133	3511
Cameroon	106	.25	1118	14227
Cape Verde	83	0	78	409
Chad	2.9	.54	223	7307
Congo	61	.66	820	3191
Cote D'Ivoire	10	.21	1320	14781
DRC	201	.073	3171	46582
Equatorial Guinea	1	.23	108	430
Eritrea	8.9	0	105	3833
Ethiopia	445	.31	1569	60644
Gabon	2.9	0	323	1180
Gambia	7.1	0	36	1210
Ghana	635	.79	1392	18463
Guinea	76	.021	760	6990
Guinea-Bissau	1.6	.19	213	1279
Kenya	235	.45	3563	28307
Liberia	44	.84	68	2936
Madagascar	31	.85	2104	14433
Malawi	46	.18	222	9801
Mali	16	.82	497	10235
Mauritania	1.7	0	298	2474
Mauritius	79	.041	978	1153
Mozambique	25	.047	294	16733
Namibia	5.8	0	477	1764
Niger	3.2	.67	294	9892
Nigeria	2408	.92	29613	118926
Rwanda	14	.076	178	7233
Senegal	26	.54	791	8913
Sierra Leone	26	.092	348	4762
Somalia	109	.07	340	8312
South Africa	10826	.83	26264	41362
Sudan	729	.64	3729	29730
Tanzania	101	.32	971	31552
Togo	33	.53	267	4205
Uganda	368	.78	909	21785
Zambia	144	.72	672	9345
Zimbabwe	266	.54	1193	11997

* Fraction of foreign stock of doctors in the Group 1 countries (USA, UK, Canada and France)

Table 3.4: Weighting matrix (full sample)

country	**weight**
Benin	0
Botswana	0
Burkina Faso	0
Burundi	0
Central African Rep.	0
Cameroon	0
Cape Verde	0
Chad	0
Congo	0
Cote D'Ivoire	0
Dem. Rep. of Congo	0
Equatorial Guinea	0
Eritrea	0
Ethiopia	.458
Gabon	0
Gambia	0
Guinea	0
Guinea-Bissau	0
Liberia	0
Madagascar	0
Malawi	0
Mali	0
Mauritania	0
Mauritius	0
Mozambique	0
Namibia	0
Niger	0
Nigeria	.107
Rwanda	0
Senegal	0
Sierra Leone	0
Somalia	0
Sudan	.117
Togo	0
Uganda	.247
Zambia	0
Zimbabwe	.071

Table 3.5: Weighting matrix (restricted sample)

country	**weight**
Benin	0
Botswana	0
Burkina Faso	0
Burundi	0
Central African Rep.	0
Cameroon	0
Cape Verde	0
Chad	0
Congo	0
Cote D'Ivoire	0
Dem. Rep. of Congo	0
Equatorial Guinea	0
Eritrea	0
Ethiopia	.167
Gabon	0
Gambia	0
Guinea	0
Guinea-Bissau	0
Liberia	0
Madagascar	.026
Malawi	0
Mali	0
Mauritania	0
Mauritius	0
Mozambique	0
Namibia	0
Niger	0
Nigeria	.173
Rwanda	0
Senegal	0
Sierra Leone	0
Somalia	0
Sudan	0
Togo	0
Uganda	.307
Zambia	.326
Zimbabwe	0

CHAPTER IV

What is the Price of Prevention? New Evidence from a Field Experiment

4.1 Introduction

Cervical cancer is the most common cancer among women in developing countries and the second most common among women worldwide (Walraven, 2003). Within the developing world, some of the highest rates of incidence are to be found in Africa - estimates range from about 36 per 100,000 in Mali to 42 per 100,000 in Uganda. In contrast, rates lower than 5 per 100,000 are found in most developed countries (Parkin et al., 2002). Because the disease is often diagnosed late in developing countries, attendant mortality is high. Of the nearly 300,000 deaths occurring worldwide from cervical cancer in 2002, more than 80 percent were in developing countries (Sankaranarayanan, 2006).

It is by now well known that cervical cancer is preventable. It is caused by persistent infection with human papilloma virus (HPV)[1] and develops slowly over 10-15 years. Prior to the development of invasive cancer however, cervical cells undergo pre-cancerous changes which can be detected and then treated. This reduces significantly a woman's risk of ever developing cervical cancer. There is some evidence that the large declines in cervical cancer incidence and mortality documented in many developed countries over the last half century have been due at least in part to the institution and maintenance of broad-based population screening programs (Antilla et al., 1999; Sankaranarayanan et al., 2001). Cervical cancer incidence rates have been reduced by as much as 80 percent in countries where screening rates and follow-up are high. Despite this, screening rates in many developing countries remain low (IARC, 2004). Gakidou et al. (2008) in a recent paper estimate that only about 19

[1] Over 99% of cervical cancers contain HPV DNA. (Walboomers et al., 1999)

percent of women, in the 30 developing countries for which they had data, had been screened within the last three years, compared to 63 percent on average in developed countries. In some countries like Ethiopia, the coverage rate was 1 percent.

These low screening rates can perhaps be blamed in part on lack of access to cervical screening; but even where screening programs exist, take-up rates are still low (Lazcano-Ponce et al., 1999). This becomes crucial when one considers that coverage rates have been identified as critical to the success or failure of cervical screening (World Health Organization, 2002). Various factors have been mentioned as possible barriers to take-up of screening ranging from poor information (Lartey et al., 2003), to cost (Bishop et al., 1995). In this study we eliminate the supply explanation by making screening available.[2] We then investigate the extent to which demand-side factors affect take-up of the program. In this paper we identify the impact of prices and estimate the effect of providing a subsidy for cancer treatment on participation in the program.

We find a qualitatively and quantitatively signicant impact of both interventions. A N10 increase in the price of screening reduces take-up of the program by between 0.7 and 0.8 percentage points - a decline of approximately 20 percent - while women selected to receive the cancer treatment subsidy were about 4 percentage points more likely to participate in screening - an increase of approximately 30 percent.

This paper makes a contribution to several related yet distinct areas of the literature. First, we make a contribution to the broader literature on the demand for preventive care, next, by identifying the impact of price on take-up, we make a contribution to the literature on user fees in developing countries, thirdly, we make a contribution to the literature on the psychological costs of testing and the use of conditional subsidies to induce desired health behaviors, and lastly, we add to the small but growing literature in development economics which studies the impacts of interventions using small scale field experiments. We review some of the relevant literature in the next section.

[2]We realize that there are supply-side factors other than availability of the program that are important, for example the quality of care provided, but for the purposes of this study, these are second-order concerns.

4.2 Previous work and theoretical considerations

4.2.1 Demand for Preventive care

There are clear differences between preventive and curative care. One important difference is that preventive care, unlike curative care, is typically purchased before the health state is known. It is also clear that these differences have important implications for the demand for preventive medical care (see Cropper, 1977; Kenkel, 2000).[3]

A substantial amount of research has been done to identify the impact of price on the demand for preventive care (Newhouse and Insurance Experiment Group, 1993; Kenkel, 1994; Stoner et al., 1998; Friedman et al., 2002). Data from the RAND Health Insurance Experiment, arguably the gold standard in the literature, suggests price elasticities of between 0.17 and 0.43 (Newhouse and Insurance Experiment Group, 1993). Much of this work though has been done in developed countries where health insurance is widely available. This presents a number of conceptual as well as methodological challenges.

First, health insurance plans often have an array of copayments, coinsurance rates, and deductibles in addition to regular premiums. This can make determining the relevant price difficult. It also means that estimates obtained from different studies may not be directly comparable depending on whether they exploit changes in coinsurance rates, changes in copayments or changes in deductibles (Solanki et al., 2000). Secondly, variation in price tends to be small, with identification often based off of small differences in co-payments (Trivedi et al., 2008). Thirdly, health insurance choices - including generosity of coverage, are endogenous and studies differ in their ability to control for selection effects. Another important challenge is that in the presence of health insurance, the demand for preventive care depends on the coverage for curative care. Pauly (1986) for example argues that generous coverage for curative care diminishes the incentives to invest in preventive care. Kenkel (1994) on the other hand argues that preventive and curative care may be complements rather than substitutes. This suggests that any attempts to estimate price elasticities for preventive care must carefully control for the generosity of coverage for curative care because of the possibility of moral hazard.

We make a contribution to this literature by providing clean estimates of the impact of price on demand for a preventive service, cervical screening. We avoid

[3] One implication for example is that the demand for preventive care is likely to be more price elastic (Ringel et al., 2002).

endogeneity concerns by randomly assiging price and because health insurance is not available in this environment we are able to estimate "pure" price elasticities.

4.2.2 User fees

There is a fairly large literature studying the impacts of user fees on utilization of health care in developing countries. James et al. (2006) and Lagarde and Palmer (2008) conduct excellent reviews of this literature. Though much of this literature does not discuss this explicitly, the underlying parameter of interest is the price elasticity of demand, because it is crucial for determining how health services should be priced. Most of these studies show significant increases (decreases) in utilization following removal (introduction) of user fees (Collins et al., 1996; Wilkinson et al., 2001; Ridde, 2003; Nabyonga et al., 2005). Though large in quantity, the average quality of many of these studies is low (see Lagarde and Palmer, 2008, for a critique of the existing literature). In addition, comparatively few studies have continuous variation in price (Abdu et al., 2004; Bratt et al., 2002, are notable exceptions), and even fewer examine utilization of preventive services (Bratt et al., 2002, is an example of one which does).[4] Many key questions therefore remain unresolved.

In addition to providing clean estimates of the effect of price on take-up of a preventive health care service, we examine a key assumption of those that advocate for user fees, namely that price serves to allocate resources to those who need it the most. We implement a simple test of this hypothesis by investigating whether women at higher risk of developing cervical cancer are also those with the highest willingness-to-pay. We contrast results obtained using a subjective measure of risk with results using an objective risk measure based on evidence from the medical literature. We also address the concern about the equity of user fees – a key concern of those who advocate against user fees – by providing estimates of the extent to which price responsiveness differs by household wealth.

4.2.2.1 Field Experiments

In recent years there has been an explosion in the number of randomized experiments being conducted in developing countries (see the overview of this growing field in Duflo, 2006). One notable spillover effect of this has been that experimental methodology is now increasingly being applied to pressing questions relevant to health policy. As should be evident from the preceding discussion, one of such questions is

[4]Part of the problem perhaps is that in many cases, preventive services were free.

the impact of pricing on take-up of various health services. This is still the subject of much debate.

On the one hand, advocates of charging positive prices argue that higher prices screen out individuals with low demand, and that when products or services are given away for free, they are not valued as much (Family Health International, 2005).[5] On the other hand, those who advocate for zero prices point to earlier work which found dramatic declines in utilization of health services when user fees were introduced. They also raise concerns about equity and argue that zero prices may be optimal especially if large externalities are present (Dupas, 2005).

Three recent papers (all field experiments) have attempted to provide some clarity to the debate on the impact of prices on take-up/use of health services/products. All three, coincidentally, deal with some form of prevention. In Ashraf et al. (2007), a water disinfectant product was offered for sale to households in Zambia at randomly chosen prices.[6] They found that take-up of the product decreased at higher price levels: a 100kw increase in the offer price resulted in a 7 percentage point reduction in the probability of take-up - an elasticity of about -0.6. They also examined the question of whether those who paid higher prices (conditional on the initial offer price) were more likely to use the product: they found statistically insignificant effects. Lastly, they examined whether prices served to target the product to those who could benefit from it the most (families with young children and pregnant women) and found no evidence.

Miguel and Kremer (2004) is a widely cited study on the effects of a deworming program on schooling in Kenya. Three years after this program was introduced, the NGO administering the program started charging parents for the deworming drugs. Kremer and Miguel (2007) found that take-up of the deworming medication fell dramatically after introduction of user fees. In the treatment schools relative to the control schools, take-up fell by 56 percentage points (note that the "treatment" here is cost-sharing. Both treatment and control schools were part of the deworming program). They found however that conditional on a positive price being charged, take-up did not vary with higher prices. They also found no evidence that higher prices helped to target treatment to children with the most need.

Cohen and Dupas (2007) studied the impact of prices on take-up and use of insecticide-treated bednets (ITN) among pregnant women in Kenya.[7] They found

[5] They also argue that these fees are necessary long-term sustainability of health programs.

[6] This was done via a door-to-door marketing campaign.

[7] The bednets were distributed at antenatal clinics.

reduced take-up of ITNs at clinics with higher prices. Relative to clinics offering ITNs for free, take-up rates at clinics offering nets at 10 Ksh, 20 Ksh and 40 Ksh were respectively 7, 17 and 61 percentage points lower. They also found no evidence that prices helped to target ITNs to women with the most need. Pregnant women paying higher prices were no sicker – as evidenced by blood hemoglobin levels – than women in the comparison group. Holla and Kremer (2009) however suggest that this latter finding might be due to credit constraints, arguing that the sickest women would also be those with the least ability to pay.

We reexamine some of these findings within the context of a clinic-based preventive service. We argue that water disinfectant, deworming medication and ITNs may not be viewed as "health care" products quite in the same way as cervical screening, which is usually performed by a nurse/doctor in a clinic. All three products for example, can be purchased in any general goods store or supermarket in most developing countries. In addition, it is more than likely that other viable substitutes existed that were used by the individuals in the study before the new technology was introduced which might explain the large reductions in demand when prices were raised. In particular, the result in Kremer and Miguel (2007) may represent a "legacy" or "entitlement" effect because the families were used to the deworming medication being given away for free. Raising the price might be seen as unfair and would tend to have large impacts in the short run. Presumably though the impact over the longer-term would be less dramatic as families adjusted (Benjamin et al., 2001).

4.2.3 Psychological costs of testing

Each time one gets tested for a disease, there is a theoretical chance of finding out that one has the disease. A growing body of work in behavioral economics suggests that fear of bad news is one reason why people are reluctant to get tested, or conditional on getting tested, why they are reluctant to learn the results (Caplin and Leahy, 2001; Caplin and Eliaz, 2003; Camerer et al., 2003; Koszegi, 2003). Some of this work suggests that if individuals are particularly averse to receiving bad news, then strategic ignorance can be an optimal outcome. The importance of this effect has been questioned in practice though (see Thornton, 2008).

While fear of bad news might be an important reason why screening rates are low (see for example Lerman et al., 1996), we posit that a simpler, more benign explanation may be at work. As Eddy (1980) argues, a medical test is only useful to the extent to which the information gained allows one to improve one's prognosis.[8] If

[8]Strictly speaking, the information is also useful, independent of its effect on prognosis, if it

no treatment exists for the disease, then finding out that one has the disease imposes utility costs without offsetting gains and there is very little incentive to get tested.[9] By the same token, if treatment exists but is unaffordable, the incentive to get tested is diminished.[10] In developing countries where health insurance is not yet widely available and most private health care expenditures represent out-of-pocket costs, we argue that the latter explanation might be more important. Consistent with this line of reasoning, cost of treatment is often cited as a major barrier to receiving medical care (World Bank, 2005).

We test our hypothesis by offering a conditional cancer treatment subsidy to a randomly chosen subset of women. The subsidy is only paid out in the event that the woman is diagnosed with cervical cancer. If fear of receiving bad news is the dominant explanation, then the subsidy should have little or no effect. If on the other hand, women are reluctant to get tested because they are sensibly concerned that if diagnosed with the disease, treatment might be unaffordable, then one will expect a qualitatively and quantitatively significant increase in the take-up rate among women selected to receive the treatment subsidy relative to women in the control group. One might also expect this effect to be strongest among women from the poorest households.

4.3 A Simple Model

To formalize the intuition in the previous section, I develop a simple two-period expected utility model similar to the model in **?**. Like **?** I assume that utility is a function of health status H, where H captures both quantity as well as quality of life. In the first period, each woman decides whether or not to undergo screening at a cost of c to obtain information about her health. Let us assume that the woman can only belong to one of thee possible health states denoted by the subscript i where $i \in \{1, 2, 3\}$. Without loss of generality, let us further assume that State 1 is the healthy state i.e. $H = H_1$ if the woman is healthy. If $i \neq 1$, then the woman is ill. Let the probability of each state be denoted by p_i. For simplicity, I assume that the screening test is completely accurate. Without screening, her exact state of health is not revealed until the next period.

The expected utility (EU_0) if the woman chooses not to undergo screening is given

allows the individual make valuable lifestyle changes.

[9] This may have been the case in the early days of AIDS.

[10] There is another margin along which this might operate: if the test itself is imperfect i.e. low sensitivity and/or specificity, then the information learned might have little value.

by the following expression:

$$EU_0 = p_1U(H_1) + p_2U(H_2) + p_3U(H_3) \tag{4.1}$$

Since the probability of being health is one minus the probability of being sick, this reduces to:

$$EU_0 = (1 - p_2 - p_3)U(H_1) + p_2U(H_2) + p_3U(H_3) \tag{4.2}$$

To capture the salient features of cervical cancer screening, let us assume that $i = 2$ corresponds to having a pre-cancerous lesion and $i = 3$ corresponds to having cervical cancer. The point of screening is to find and treat women with pre-cancerous lesions which may later develop into cervical cancer. If that transition probability is r, then H_2 is really a weighted probability of H_1 and H_3, so that (2) can be rewritten as:

$$EU_0 = (1 - p_2 - p_3)U(H_1) + p_2[rU(H_3) + (1 - r)U(H_1)] + p_3U(H_3) \tag{4.3}$$

If the woman undergoes screening and the test reveals that the woman is ill, she can undergo treatment costing C. We do not use a subscript because $C_1 = 0$, and in this experiment, the cost of treatment for a pre-cancerous lesion is included in the cost of screening, c, so that C_2 is also zero, C therefore refers to the cost of treatment for cervical cancer. Let us assume that treatment is always effective and restores the woman to a level of health, H^* where $H_3 < H^* < H_1$. Let us also assume that if the screening test reveals that the woman has a pre-cancerous lesion, she will receive treatment, and post-treatment, will never develop cervical cancer. In other words, treatment reduces r to 0. The expected utility (EU_1) if the woman chooses to undergo screening is therefore given by the following expression:

$$EU_1 = (1-p_2-p_3)U(H_1-c)+p_2[U(H_1-c)]+p_3[\alpha U(H^*-c-C)+(1-\alpha)U(H_3-c)] \tag{4.4}$$

This reduces to:

$$EU_1 = (1 - p_3)U(H_1 - c) + p_3[\alpha U(H^* - c - C) + (1 - \alpha)U(H_3 - c)] \tag{4.5}$$

In this environment, average incomes are low,[11] formal credit markets are almost non-existent, and there is no health insurance. Cancer treatment which will require multiple visits to the hospital (for initial evaluation, treatment and follow-up) is also only available at the teaching hospital, more than four hours away. There is therefore a distinct possibility that a woman diagnosed with cancer will be unable to pay for treatment. We denote this probability in the model by α. One can therefore think about α as an affordability parameter.

Clearly, the woman will only demand screening if $EU_1 > EU_0$. The expected value of screening, EV is therefore given by: $EV = EU_1 - EU_0 > 0$. If we expand and gather like terms, we obtain the following expression:

$$\begin{aligned} EV &= p_3[\alpha U(H^* - c - C) + (1-\alpha)U(H_3 - c) - U(H_3)] - \\ &\quad p_2[rU(H_3) + (1-r)U(H_1)] + \\ &\quad (1-p_3)U(H_1 - c) - (1 - p_2 - p_3)U(H_1) > 0 \end{aligned} \tag{4.6}$$

From (6), we see that if the woman believes that she is healthy ($p_2 = p_3 = 0$), she will not demand screening because $U(H_1 - c) - U(H_1) < 0$. Differentiating (5) with respect to our parameters of interest, c and C, we find that:

$$\frac{\partial EV}{\partial c} < 0 \quad \text{and} \quad \frac{\partial EV}{\partial C} < 0.$$

This suggests that varying the cost of both screening and treatment will have an impact on the demand for cervical screening.

4.4 Program Description

This study was implemented in Akoko North-West local government, one of nineteen local governments in Ondo State, South-west Nigeria.[12] The population of this local government is approximately 200,000 (National Population Commission, 2006). We chose to implement this study here primarily because the infrastructure for a cervical screening program already existed. An attempt to implement a population-based screening program was begun in 2006 as a collaborative effort between the

[11] They also vary over time. Only about 31% of the women in this sample who describe themselves as employed are in regular wage employment.

[12] A local government area is similar in style to a US county.

local government, a community-based NGO and physicians at the regional teaching hospital. The NGO provided funding which was used to create awareness about the disease and to train community health workers in visual inspection techniques for cervical screening (see Appendix A). In theory, screening was then supposed to be available at the primary health care clinics in each community within the local government at a subsidized price which covered not just screening, but also treatment of any pre-cancerous lesions using cryotherapy (again see Appendix A). In reality, the program never really attained wide coverage, and in all, only about a couple of hundred women were ever screened (most of them around the time when the program was launched). However the framework still existed and so we provided funding so that the program could again be offered. For logistical reasons, we offered the program in one community. Screening was provided through the local primary health care center.

To create awareness about the program, we sought audience with the local traditional ruler and his council of chiefs, we also engaged the services of a town-crier and recruited field workers who went door-to-door to tell women about the screening program. Between November 2008 and February 2009, we carried out an enumeration exercise to create a sampling frame, and 960 households were drawn to participate in this study. The remainder of the study was carried out between March 2009 and August 2009.

4.5 The Experiment

Each household selected to participate in this study was visited by a trained female interviewer who conducted an interview with all eligible women in the household. Eligibility was defined based on gender (female), age (18-64) and medical history (no previous hysterectomy). We also excluded all women who had ever been screened before. Once the interview was completed, the interviewer spent some time educating each woman about cervical cancer and the benefits of cervical screening (based on a cervical cancer resource guide/fact sheet which was later given to the woman) and then administered two experimental interventions:

4.5.1 Screening Price

Within each household, after the interviews were completed, the interviewer gathered all eligible women in the household and informed them that screening was going to be provided at a subsidized price but that the exact price each household would pay would be determined by playing a game. The women were then shown a scratch card with three covered circles numbered 1-3 and told that each covered circle concealed a price. The price schedule consisted of three prices: N0, N50 and N100.[13] The rules of the game were simple: one woman would choose one of the numbered circles and the price underneath the covered circle would be the price all the women in the household would pay. The women were then asked to choose among themselves someone to play the game on behalf of all the women in the household.[14] The game concluded by asking the elected woman to scratch off the sticker and the resulting price was announced by the interviewer. A dated, signed price voucher in that amount was then issued to each eligible woman in the household. Each voucher was person-specific – it contained the name and unique ID of each eligible woman and could only be used by that woman.

We went to a great deal of effort to ensure that the experiment worked as planned. First of all, the price vouchers were very distinctive and could not be duplicated without considerable effort.[15] Secondly, the prices assigned to each numbered circle were varied randomly so that neither the interviewer nor the respondent could know which numbered circle concealed what price. Thirdly, the scratch cards were individually numbered and had to be returned at the end of each working day to the field supervisor who then cross-checked to make sure only one circle had been scratched off and that the others had not been tampered with. That our randomization strategy was successful is evident from Figure 1.

We hypothesized that women receiving higher prices would be less likely to take-up the screening program.

4.5.2 Cancer Treatment Subsidy

During the interview, each woman received detailed information about the benefits of cervical screening in terms of reducing the risk of ever developing the disease. They were told that the price of screening included treatment of any pre-cancerous lesions. They were also told that there was a very small risk of being diagnosed with invasive cancer.[16] If that diagnosis was made, they would be referred to the teaching hospital for further treatment and as is normal in Nigeria, would be responsible for the cost

[13] The currency in Nigeria is known as the Naira (N). $1 ≈ N150.

[14] If there was only one eligible woman in the household, then she was chosen by default.

[15] This is actually an understatement because the technology to duplicate the vouchers was not available in the community.

[16] The interviewers were instructed to tell the women that the prevalence was between 3 and 4 per 10,000 women.

of their treatment. They were then offered an opportunity to receive a subsidy for cancer treatment (if it became necessary) by playing another game. The subsidy would cover the cost of treatment at the teaching hospital up to a maximum of N100,000 and would only be paid out in the (unlikely) event that invasive cancer was diagnosed. The game was similar to the first one but with only two outcomes.[17] At the conclusion of the game, the women were informed whether they were eligible to receive the subsidy or not.

We hypothesized that women selected to receive the treatment subsidy would be more likely to take up the screening program.

4.6 Data and results

We have data on approximately 1100 women from 912 households.[18] Summary statistics are in Table 4.2. The mean age is 36.3 years, 68 percent of the women are married and the average length of schooling is nine years. About 70 percent describe themselves as employed – 22 percent have regular wage employment, 36 percent are self-employed (most have a small scale business), and 9 percent report self-employment in agriculture. In terms of ethnicity and religion, this is a relatively homogenous sample. The average household size is five and on average there are two bedrooms for sleeping. About a quarter of households report owning a motor vehicle, 41 percent own a refrigerator and nearly 60 percent of households report that at least one member of the household has a bank account. The vast majority of households have electricity (90%) and 23 percent of households describe their main source of drinking water as pipe-borne water. For the most part, this appears to be a fairly healthy population; more than 90 percent of the women describe their health as good or better, and only 8 percent report being in poor health. 47 percent of women describe the local government hospital as their usual source of care; about 14 percent usually seek care at one of the two primary health care centers (including the one where this study is sited). A non-trivial fraction of women (17%) report having infrequent contact with the health system; their preferred source of care is a nearby drug dispensary or a traditional healer (1.2 percent).

Overall, about 35 percent of women drew a zero price (these women are the control group), 33 percent drew a price of N50 and about 32 percent drew the highest price

[17]We again used a scratch card but this time with only two covered circles only identified as A and B.

[18]The vast majority of women agreed to participate in the study. Less than 1% of women refused to participate.

of N100. In Figure 4.1 and Figure 4.2, we graph the distribution of price and the conditional subsidy. In neither case can we reject the null of equality of proportions across treatment and control groups. Though we designed the experiment such that both treatments would be orthogonal, in Figure 4.3 we test for the equality of the price distribution across the treatment (Subsidy = Yes) and control groups (Subsidy = No). A Kolmogorov-Smirnov test fails to reject the null of equality of distributions; the p-value is 0.6.

Next we test whether our randomization strategy was successful. If price was distributed randomly, then it should be entirely unrelated to either individual or household characteristics. In Table 4.3, we regress each of our treatments on a variety of individual and household characteristics. For the most part we see that our randomization worked; none of the covariates are statistically significant in the price regression (Column 1). As for the subsidy, women receiving the subsidy appeared to be slightly younger and more likely to come from a smaller household. They are also more likely to report using a flush toilet. In all subsequent models, we include these variables as control variables. Reassuringly, none of the other variables appear to be correlated with receipt of the subsidy.

4.6.1 Did the interventions have an impact?

We begin by looking at the impact of price. We can estimate the magnitude of the impact by taking a simple difference of means. The take-up of screening in the control group (Price = 0) was 19.5 percent. Going from zero to N50 reduced the take-up rate by nearly 4 percentage points, to 15.8 percent, and at the highest price of N100, the take-up rate was 12.3 percent. Women selected to receive the subsidy were much more likely to take-up the screening program. Relative to the take-up rate of 13.8 percent in the control group, participation among women selected to receive the subsidy was about 4.2 percentage points higher. The impact of price does not appear to be different across the subsidy treatment and control groups (see Figure 4.4). Later on, we formally test for interaction effects between both interventions.

One advantage of a randomized experiment is that, when successful, it limits the need for high-powered econometrics in order to estimate treatment effects. Because random assignment solves the selection problem, a simple difference of means suffices to estimate the average treatment effect of the intervention. In Table 4.4 column 1, we run what is essentially the OLS analogue of a difference of means; we regress take-up of the screening program on *Treatment 1* (price) and *Treatment 2* (subsidy) in a model with no covariates. Because both treatments are orthogonal, we can include both of them in the same model and still identify their separate impacts.[19] Not surprisingly, the results from column 1 are essentially identical to the difference of means. The results suggest that a N10 increase in price reduced take-up by about 0.7 percentage points while women in the subsidy group were about 4 percentage points more likely to participate in the screening program.

In columns 2-6, we report results from variants of the following basic model:

$$Takeup_{ij} = \beta_0 + \beta_1 Price_j + \beta_2 Subsidy_j + X_{ij} + \epsilon_{ij}$$

Where X is a vector of control variables that include individual and household characteristics.

In column 2 we present results from a parsimonious model in which we include just a basic set of demographic variables while in column 3 we include an extensive set of control variables including interviewer fixed effects. In the interest of space we do not report coefficients on all the variables included in the model, many of which were statistically insignificant (detailed results are available from the author on request). In column 4 we estimate a probit model and report marginal effects[20] and in column 5 we estimate a linear model with household random effects. Reported standard errors are robust to intra-household correlation. Across all specifications, the results are nearly identical to those obtained from a simple difference of means. The estimated impacts are robust to including more (or less) covariates or even any at all. They are also robust to a variety of different specifications. This again suggests that our randomization was successful. In column 6, we formally test for interaction between both price and the cancer treatment subsidy. We find no evidence of an interaction effect, which is consistent with the evidence from Figure 4.4.

Overall, the results are remarkably consistent: a N10 increase in the price of screening reduced take-up by between 0.7 and 0.8 percentage points; and selection to receive the cancer treatment subsidy, should it prove necessary, increased the take-up rate by about 4 percentage points – a nearly 30 percent increase in the take-up rate of the screening program.

Another consistent finding that holds across specifications is that older women were significantly more likely to participate in the screening program. The effect appears to be diminishing with age though the higher order term is statistically insignificant. A 5 year increase in age is predicted to increase participation in the

[19]This also reduces the residual variance.

[20]Marginal effects are estimated at the mean.

program by about 2 percentage points. This at first glance might seem puzzling because economic theory would seem to suggest the opposite. The Grossman model of health capital accumulation for example predicts that health investments should decline with age because of the shorter payoff period (Grossman, 1972). Kenkel (1994) discusses the countervailing effect of health risk increasing with age which should increase consumption of health care. There are at least two other explanations for this finding. (1) Older women may face lower opportunity costs. They are less likely to have younger children who need to be looked after while the woman visits the health care center for screening. From our data, they are also more likely to report being self-employed which suggests that they probably have more flexibility in terms of arranging their schedule. (2) There are psychological costs associated with cervical screening because of the intimate nature of the vaginal examination that is required. Older women may face lower psychological costs for example because of prior experience with childbirth.

One problem with the strategy we employed is that the interviewers were not blinded to the price received by each household. Even though they did not know *ex ante* what the price would be, once it was revealed to the household, it was also revealed to the interviewer. It is therefore possible that the interviewers attempted to compensate in cases where the household drew a higher price. If the interviewers realized that women receiving a higher price would be less likely to come for screening, they might have tried extra hard to persuade them to participate. To address this concern, we look at the average time spent on each interview. If the interviewers worked extra hard to persuade women who had an "unlucky" draw, then we would expect that they would spend more time on average with women receiving a higher price than with women receiving a lower price.

In Table 4.5, we regress interview time on price, with price entered as a continuous variable (column1). We do not find a significant correlation between price received and time spent on the interview. However, when we specify price as a binary variable such that we are comparing women who received a positive price to women who drew the free price, we find statistically significant evidence that the interviewers spent on average two minutes extra with women receiving a positive price (column 2). When we specify price as a categorical variable, we find the largest impact for women drawing N50 (column 3). On average, compared to women drawing the zero price, interviews with women drawing N50 lasted three minutes longer. This results persists even after controlling for a broad range of individual and household characteristics. This is a rather interesting finding and is consistent with a model in which interviewers allocate effort (proxied by time) in such a way as to maximize total turnout for screening. The interviewers should rationally allocate their effort (subject to some constraint)[21] such that the marginal product of effort is equal across all three price groups. If the women receiving the lowest price are more likely to come for screening anyway, then the interviewers should rationally allocate more effort to women drawing a positive price. One might expect that the marginal return to effort would be largest amongst the middle group because more women in that group are likely to be on the margin of participating, which would explain why the interviewers would spend more time on average with this group.

The next question is, does this affect our experimental estimates? In Table 4.6, we repeat the analysis in Table 4.4 column 3 but include interview time as a covariate. The experimental impacts are exactly the same and even though the sign on interview time is in the expected direction, it is statistically insignificant. We conclude therefore that while the interviewers may have spent more time trying to persuade women with an "unlucky" draw to participate in the screening program, this does not appear to have made a difference.

4.7 Other considerations

4.7.1 Non-linearities in price

Because we have three price points, we can test for non-linear impacts of price. The evidence on this is mixed in the literature. Kremer and Miguel (2007) find that conditional on being charged a positive price, take-up of deworming medication does not vary with price while Cohen and Dupas (2007) show that the effect of price on take-up of insecticide-treated bed nets among pregnant women is increasing in price. In Table 4.7, we enter price as a categorical variable (N50 is the omitted price category). Overall we find evidence more in keeping with Kremer and Miguel (2007) than with Cohen and Dupas (2007). More of the impact appears to be between N0 and N50. Going from zero to N50 reduces take-up of screening by about 5 percentage points (statistically significant at $p < 0.1$) while going from N50 to N100 reduces take-up by about 3 percentage points. However we cannot reject the null that both coefficients are equal (F-statistic = 0.19). It is likely that in practice, the impact of price is going to be product and/or service-specific.

[21] The constraint might be completing a given number of interviews per day.

4.7.2 Is price a targeting mechanism?

To explore this question, we constructed a measure of subjective risk. Our reasoning here is that women who perceive themselves as having the most need for screening should be women who perceive themselves as having a higher risk of developing cervical cancer. To measure this subjective risk, we showed women a physical scale and asked them to mark on the scale where they thought they fell in terms of *their* probability of ever developing cervical cancer. This type of scale is used in the Health and Retirement Study and has been shown to have good construct validity (see Hurd and McGarry, 1995). The scale ranged from 0-100 with 0 representing *absolutely no chance* of ever developing cervical cancer and 100 signifying absolute certainty that they would develop cervical cancer. One can think of this kind of scale as representing the individual's perception of her subjective risk probability. We rescaled the variable to range between 0 and 1. The distribution of this risk measure in our sample is shown in Figure 4.5. The mean was 0.19.[22]

We divide the risk probabilities into quartiles and in Table 4.8 we present the distribution of price for each risk quartile conditional on take-up of screening. We find that conditional on take-up, 34% of women in the highest risk quartile were willing to pay the highest price of N100 compared to 18% of women in the lowest risk quartile. These results certainly seem suggestive but we cannot reject the null that the distribution is the same. An important caveat though is that we have really small sample sizes. When we include this risk measure in the regression model, the coefficient is positive suggesting that women with higher subjective risk were more likely to participate in screening, but it is statistically insignificant. The interaction with price is also statistically insignificant. Overall we find no evidence that women with the highest subjective risk have the highest willingness to pay.

We chose to use a subjective measure rather than an objective measure of risk for several reasons: first only about 9 percent of women screened (less than 2% of the total sample) had a positive test result and because the number is so small, we cannot use a positive test result as a measure of risk. In addition, pre-cancerous cervical lesions are for the most part asymptomatic, meaning that the woman does not feel "sick" and so arguably the most salient measure of perceived risk is the woman's self-reported probability. We find a similar pattern of results when we use

[22]The age-standardized lifetime risk of developing cervical cancer is approximately 5% for Brazil and Peru (see Sasieni and Adams (1999). It is probably higher in Nigeria. This suggests that while women may be overestimating their risk of developing cervical cancer, they are not not wildly overestimating it.

a different measure of risk - the number of lifetime sexual partners per woman.[23]

4.7.3 Heterogenous treatment effects

There is growing recognition in the health economics literature of the importance of heterogeneity in treatment effects (see Heckman et al., 1997; Bitler et al., 2005, for discussions). The experimental impacts we have estimated in this paper represent average treatment effects but it is likely that both treatments (price and the subsidy) will have different impacts for different groups of individuals. In this section we consider, to quote Marianne Bitler, "what mean impacts might be missing". We explicitly consider heterogeneity along several potentially important dimensions - age, schooling, health status, prior knowledge about cervical screening, and household wealth. We interact both price and subsidy with continuous measures of age and years of schooling and with discrete variables for self-reported health status, prior knowledge of cervical screening and a proxy for household wealth. To measure health status, we asked women to assess their health on a 5-point scale from 1 (Excellent) to 5 (Poor). In the regressions which follow, we collapse this variable and compare those reporting fair health or worse to those reporting good health or better.

To assess prior knowledge about cervical screening, we asked women whether they had heard about cervical screening before the start of the program. This is a binary variable equal to one if the answer was yes. Lastly we proxy for household wealth using a measure of household self-reported income. We asked women to estimate total household income from all sources (the interviewer was asked to enumerate various sources of income including remittances from abroad) and to limit measurement error we asked the women to simply say whether it fell within a given range. As we show in Table 4.9, Table 4.10 and Table 4.11 this correlates in the expected way with commonly used measures of wealth including possession of durable assets and other characteristics of the household and should therefore be a reasonable proxy. We collapse this measure into three categories: households reporting incomes <N10000, households reporting incomes between N10000 and N40000 and households reporting incomes >N40000. Results are in Table 4.12. Overall, the only statistically significant finding is that women from the richest households have lower price elasticities. At a given price, women belonging to the richest households were about 0.2 percentage points more likely to participate in the screening program than women from the poorest households.

[23]There is only very small variation in this measure and so the woman's self-assessed subjective risk is the preferred measure.

4.8 Discussion

We find not surprisingly that higher prices reduce take-up of the screening program but we do not find *per se* a discontinuity at zero. We note however that though we do not find evidence of a drastic decline in take-up when we go from zero to a small positive price (at least over the range of price in the data), like Holla and Kremer (2009), we argue this might be a bit misleading. If we assume that the impact of price is linear over a substantial portion of the price distribution, and over the range of price in this study it appears to be linear, then our estimates imply that increasing the price to about N300 would reduce demand for screening to essentially zero. This is still only a fraction of the true cost of providing cervical cancer screening.[24] Estimates from the supplementary appendix of Goldie et al. (2005) for Kenya, which one assumes is probably not too different from Nigeria, suggest that the cost of providing screening using VIA is around $30 (in 2000 international dollars). This includes only cost of labor and materials and ignores all other costs (for example the cost of treating complications arising from treatment). All else equal, it suggests that subsidies are likely going to be needed if wide coverage is an important policy goal.

The experimental impacts we find are both qualitatively and quantitatively important. A N10 increase in price reduces take-up of cervical screening by between 0.7 and 0.8 percentage points. Going from a price of zero to a price of N50 reduces participation by about 4 percentage points - a roughly 20 percent decline. Our estimates imply a price elasticity around -0.2. This not surprisingly is lower than the price elasticity of -0.6 reported in Ashraf et al. (2007). As we argued earlier, there is likely to be greater substitutability for a water disinfectant product compared to cervical cancer screening. Unlike Cohen and Dupas (2007), over the price range observed in this study, we do not find take-up declining at an increasing rate; instead, our findings can be interpreted as being more in keeping with Kremer and Miguel (2007) who find that conditional on the price being positive, higher prices do not have a significant impact on take-up. In keeping with the rest of the literature we find, at best, only very weak evidence that price serves to allocate the health good to those with the most need.

Another important contribution this paper makes is by testing the hypothesis that concerns about affordability of treatment contribute to the low take-up of screening. Even though the probability of being diagnosed with cervical cancer is very small,

[24] Where screening is available in Nigeria, it costs up to N5000, though presumably the true cost of providing the service is much less.

previous work has shown that human beings tend to overestimate the likelihood of occurrence of improbable events (see an overview of some of this literature in McFadden, 1999). Consistent with this, we find that women in our sample, on average, appear to overestimate their probability of developing cervical cancer. This is not a new finding (see **?**).

We find that the take-up rate is about 4 percentage points higher among women who are offered a conditional treatment subsidy - to be paid out only in the event that they are diagnosed with invasive cancer. From this we conclude that even though screening itself reduces the risk of developing cervical cancer, there is evidence that concerns about affordability of treatment in the unlikely event of being diagnosed with cervical cancer contribute to low take-up of cervical screening.

One concern we had about the subsidy was that for it to work, it had to be credible. Women had to believe that if diagnosed with the disease they would actually receive the subsidy, because it was essentially a promise. We were concerned that in this environment where people are generally distrustful of promises – especially for a program associated with the government – that we would not find an effect simply because women were skeptical. As the results show, our fears were unfounded. It is important to note though that our estimates can be interpreted as a lower bound on the true effect if at least some women did not participate in screening because they did not believe they would receive the subsidy.

4.9 Conclusion

Lack of generalizability is often mentioned as one problem with field experiments. Many of the field experiments in developing countries are small-scale and regional and critics often argue that results from these experiments may not apply to other places and at other times (see for example Rodrik, 2008). Duflo (2006) in response to this critique makes a compelling case for replication of field experiments in different settings (see also Imbens, 2009). In this study we explicitly link our work to previously done work in other countries and compare/contrast our findings. Overall, we find results that are broadly consistent with the work of others in this field. We also make an important contribution by estimating the impact of a conditional cancer treatment subsidy. An important takeaway from this paper is that even though price has an important effect on take-up of screening, clearly it is not the whole story, because even among women who received a zero price, the take-up rate was still only 20 percent. It is likely that distance and travel costs still remained a significant barrier and contributed to the low participation rates. We did not attempt to influence those costs except indirectly by reducing the total cost of screening. Another reason that is often cited as contributing to low rates of screening is cultural factors. We attempted to take those into account by recruiting only female interviewers and female nurses to conduct the screening. We acknowledge though that there might be other dimensions along which cultural factors might come into play. We hope that in future work we will be able to tackle some of these alternative explanations.

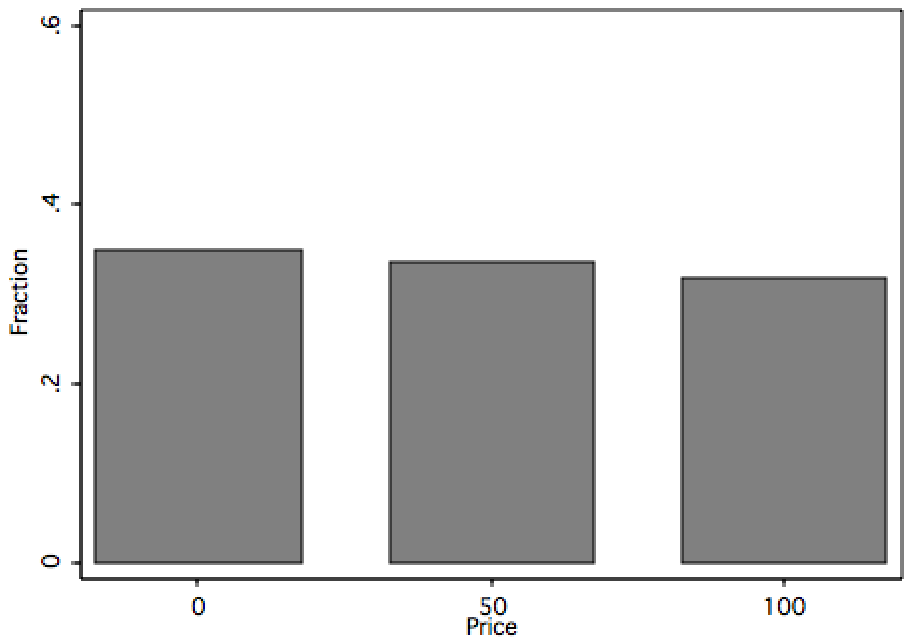

Figure 4.1: Price Distribution

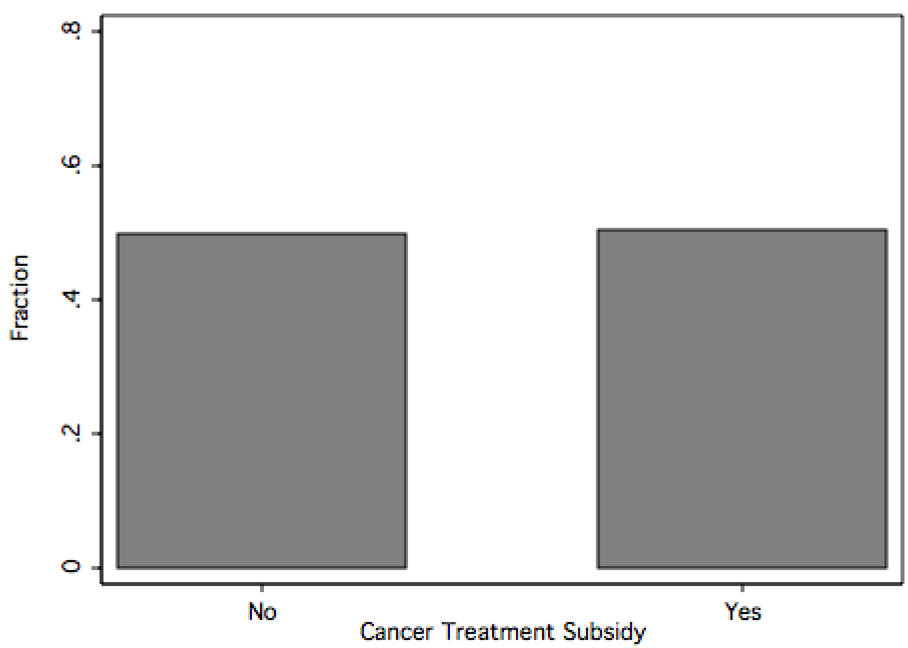

Figure 4.2: Distribution of Subsidy

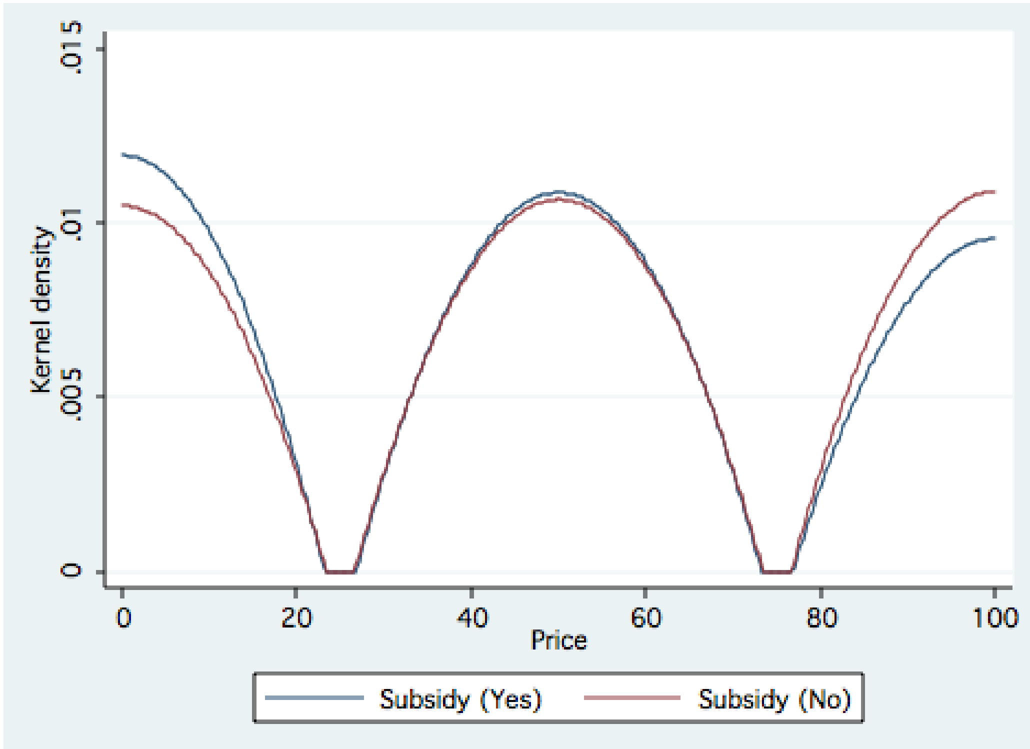

Figure 4.3: Kernel Density

Table 4.1: Joint distribution

	No	Yes	
Free	175	200	375
N50	178	182	360
N100	182	160	342
	535	542	1077

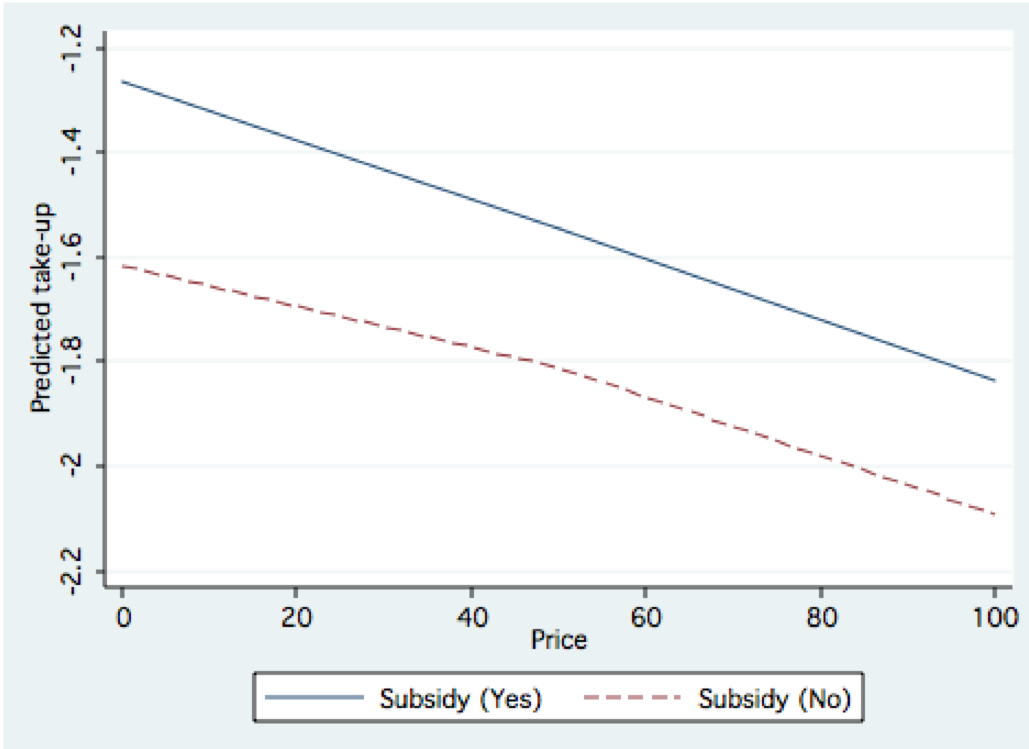

Figure 4.4: Was there an interaction effect?

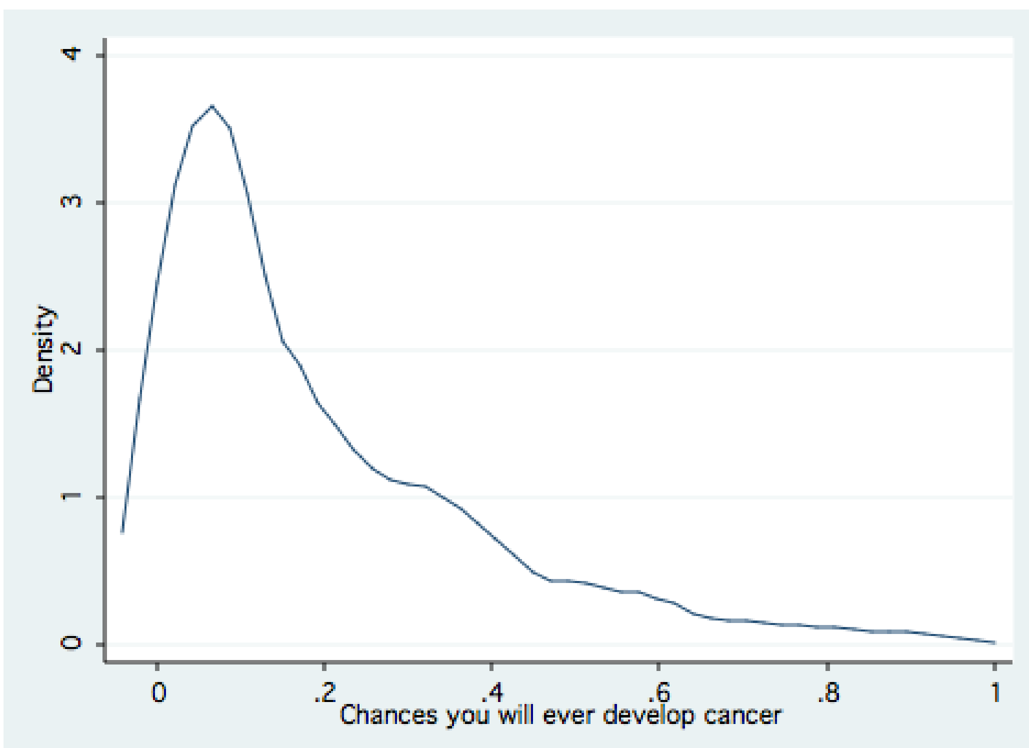

Figure 4.5: Distribution of subjective risk probability

Table 4.2: Summary Statistics

variable	**mean**	**sd**
Age	36.3	12.7
Married	0.68	0.47
Years of schooling	9.1	5.1
Employed	0.70	0.46
Prior knowledge of cervical screening	0.34	0.47
Spouse age (if married)	42.3	21.8
Religion (Christian=1)	0.90	0.31
Ever smoked	0.01	0.08
Self-reported health status (Excellent=1)	0.31	0.46
Health care visits over the last 12 months	1.6	3.2
Ever pregnant	0.81	0.39
Household size	5.1	2.0
Number of bedrooms	2.2	1.3
Main source of drinking water (pipe-borne water)	0.23	0.42
Main source of cooking fuel (firewood)	0.64	0.48
Percentage of Households with bednet	0.28	0.45
Percentage of Households that use flush toilet	0.19	0.40
Percentage of Households with Television	0.81	0.39

Table 4.3: Was randomization successful?

	(1) Price	(2) SE	(1) Subsidy	(2) SE
Age	-.12	(.15)	-.004*	(.0018)
Married	2.2	(3.7)	-.019	(.045)
Schooling	.033	(.35)	-.006	(.0044)
Employed	6.6	4.0	-.058	(.05)
Religion (Christian=1)	-7.5	(4.2)	.063	(.052)
Number of sick days (last 30 days)	.074	(.6)	.014	(.0074)
Have ever been pregnant	-2.6	(5.0)	.10	(.061)
Prior knowledge of cervical screening	-.68	(2.9)	.051	(.036)
Self-reported risk of developing cancer	.046	(.074)	.002	(.0009)
Self-reported income (None)	3.6	(7.3)	.007	(.09)
Self-reported income (< N5000)	-8.3	(5.6)	-.084	(.069)
Self-reported income (N5k–N10k)	.20	(5.4)	-.038	(.067)
Self-reported income (N10k–N20k)	-2.2	(5.6)	-.024	(.069)
Household Size	-1.0	(.72)	-.018*	(.009)
Number of bedrooms	3.3	(5.0)	.032	(.062)
Report owning a bednet	-3.7	(3.0)	.056	(.037)
Report owning a power generator	6.1	(3.6)	-.031	(.045)
Report owning a refrigerator	-.38	(3.0)	-.055	(.037)
Type of toilet (Water closet)	1.5	(4.0)	.17**	(.05)
Main source of cooking fuel (firewood)	.50	(3.2)	.047	(.04)
N	1001		1002	
r2	.048		.048	
F	1.6		1.6	

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

All regressions include interviewer fixed effects

Table 4.4: Main Results

	(1) OLS	(2) OLS	(3) OLS	(4) Probit	(5) Random Effects	(6) OLS
Price	-.00074***	-.0007**	-.00077***	-.0078***	-.00076***	-.00065*
	(.00028)	(.00028)	(.00028)	(.00028)	(.00029)	(.00038)
Subsidy	.038*	.039*	.038*	.037*	.036	.048
	(.023)	(.023)	(.023)	(.023)	(.023)	(.038)
Age		.0043***	.0039***	.0039***	.004***	.0042***
		(.0012)	(.0013)	(.0011)	(.0012)	(.0012)
Married		.016	.023	.021	.022	.018
		(.027)	(.029)	(.026)	(.028)	(.028)
Schooling		.0041	.0037	.0029	.0031	.0034
		(.0027)	(.0033)	(.0027)	(.0031)	(.0032)
Employed		-.045	-.04	-.046	-.047	-.047
		(.031)	(.034)	(.034)	(.034)	(.034)
Religion		.033	.036	.036	.032	.037
		(.037)	(.033)	(.033)	(.034)	(.034)
Price x Subsidy						-.00021
						(.00057)
Basic set of controls	No	Yes				
Includes interviewer fixed effects	No	No	Yes	Yes	Yes	
N	1042	1032	1025	1031	1031	1031
r2	.0099	.028	.047			.045
F	5.2	2.9	2.4			2.6

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.5: Duration of Interview

	(1) Interview	(2) Interview	(3) Interview
Price	.0095		
	(.0099)		
Positive price		1.8**	
		(.85)	
Price = N50			2.6***
			(.97)
Price = N100			.90
			(.99)
age	.07*	.07*	.065
	(.042)	(.042)	(.042)
Schooling	-.018	-.021	-.027
	(.1)	(.1)	(.1)
Married	2.2**	2.3**	2.3**
	(.98)	(.98)	(.98)
Employed	-.28	-.27	-.14
	(1.1)	(1.1)	(1.1)
N	1016	1016	1016
r2	.16	.16	.17
F	7.9	8.1	7.9

All models include an extensive list of controls including interviewer fixed effects

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Omitted price category is N50

Table 4.6: Robustness Check

	(1)
	Take-up
Price	-.00077***
	(.00029)
Subsidy	.04*
	(.023)
Interview time	.0004
	(.00099)
N	1022
r2	.045
F	2.6

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.7: Price is categorical

	(1)
	Take-up
Free	.051*
	(.03)
N100	-.029
	(.028)
N	1016
r2	.045
F	2.3

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Omitted price category is N50

Table 4.8: Risk Quartiles

Risk Quartiles	Free	N50	N100	Total
1st quartile	21	20	9	50
2nd quartile	15	14	11	40
3rd quartile	21	11	8	40
4th quartile	16	11	14	41
Total	73	56	42	171

Pearson chi2(6) = 5.8 Pr = 0.44

Table 4.9: Toilet facility is water closet

	Water closet		
Household's total monthly income	No	Yes	Total
Less than N5000	92.4	7.6	100.0
N5,000-N10,000	90.5	9.5	100.0
N10,000-N20,000	88.6	11.4	100.0
N20,000-N40,000	80.5	19.5	100.0
N40,000-N60,000	66.4	33.6	100.0
More than N60,000	62.1	37.9	100.0
Total	81.4	18.6	100.0

Table 4.10: Does any member of this Household have a bank account?

	Bank account		
Household's total monthly income	No	Yes	Total
Less than N5000	56.6	43.4	100.0
N5,000-N10,000	55.3	44.7	100.0
N10,000-N20,000	49.1	50.9	100.0
N20,000-N40,000	33.3	66.7	100.0
N40,000-N60,000	21.6	78.4	100.0
More than N60,000	16.5	83.5	100.0
Total	41.1	58.9	100.0

Table 4.11: Does any member of this Household own an electric generator?

	Generator		
Household's total monthly income	No	Yes	Total
Less than N5000	88.3	11.7	100.0
N5,000-N10,000	87.2	12.8	100.0
N10,000-N20,000	86.5	13.5	100.0
N20,000-N40,000	85.1	14.9	100.0
N40,000-N60,000	68.1	31.9	100.0
More than N60,000	53.2	46.8	100.0
Total	80.1	19.9	100.0

Table 4.12: Heterogenous treatment effects

	(1) Coefficient	(2) (SE)
price	-.00089	(.0015)
subsidy	-.14	(.12)
pricexage	-1.1e-05	(2.7e-05)
age	.0033	(.0022)
Schooling	.0028	(.006)
Schooling x Price	-3.1e-05	(7.6e-05)
Poor health	.024	(.076)
Poor health x Price	.0007	(.001)
Income2 (N10-N40k)	-.018	(.057)
Income3 (> N40k)	-.11*	(.061)
Income2 x Price	.0006	(.0007)
Income3 x Price	.002***	(.0007)
Aware of screening	.012	(.052)
Aware x Price	7.8e-05	(.0006)
Subsidy x Age	.0027	(.0023)
Subsidy x Schooling	.006	(.0062)
Subsidy x Poor health	-.0079	(.082)
Subsidy x Income2	.036	(.057)
Subsidy x Income3	.0074	(.062)
Subsidy x Aware	.017	(.05)
_cons	-.026	(.12)
N	1040	
r2	.057	
F	1.9	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

CHAPTER V

Conclusion

In this dissertation I have examined some of the factors commonly mentioned as contributing to health professional migration. In my first paper I introduce a new dataset on physician migration from more than 30 sub-Saharan African countries to the US and the UK. I exploit the panel nature of this dataset to derive quantitative estimates of the importance of economic shocks. I find that - in this case at least - the perceived wisdom appears to be right: physician migration does respond to changes in economic conditions. My estimates suggest that a one percentage point decline in lagged growth increases physician out-migration by approximately 0.3 percent. In the IV models, a one percentage point decline in lagged growth increases physician out-migration by between 3.4 and 3.6 percent. Even though I argue that economic shocks affect physician migration directly through earnings, and indirectly by changing expectations about future earnings, I cannot test these hypotheses using the data I have. I therefore leave this for future work.

In my second paper, I look directly at the impact of doctors' earnings. I exploit a natural experiment in Ghana to derive estimates of the impact of increasing doctors' salaries. I find that the ADHA program which increased salaries of doctors in Ghana by between 75-150 percent appears to have reduced migration of physicians from Ghana. By 2004, six years after the program was introduced, the foreign migrant stock of Ghanaian doctors had reduced by between 10-13 percent depending on the assumptions one is willing to make. The cost-effectiveness of this program is unclear though. The starting budget for the program was around \$1.6 million.[1] If we increase this by 10 percent every year to account for inflation, then by 2004, the government would have spent approximately \$11,384,605. This translates to approximately \$113,846 per physician retained. Admittedly, these are crude estimates and

[1] Later on the budget ballooned because the government expanded the program to include other health professionals.

fail to take into account a number of different things including the spillover effects on other health professionals but if anything, we are more than likely underestimating the cost of the program. It certainly raises questions about the cost-effectiveness of a program to increase doctors' salaries.

The generalizability of our findings is unclear but we do find a similar effect when we take a quick look at a different country, Tanzania, which implemented a similar program. This suggests that our findings are likely to hold up if one evaluates other programs elsewhere. There is however, still a lot of room for more research on this topic. In addition to obtaining estimates of the impact of doctors' earnings, future work can help clarify what aspects of such programs are important or even, whether programs that utilize non-financial incentives will prove to be more cost-effective.

In my third paper, I examine the demand for preventive care. I show that price is clearly an important determinant of take-up but I also show that, at least in the case of cervical cancer screening, the cost of treatment for the cervical cancer, an admittedly unlikely event, also affects take-up of cervical screening. I also make two additional contributions; I test for evidence of a discontinuity at zero (I find none), and investigate whether women with the most need are also those with the highest willingness to pay, which would be consistent with price acting as a screening mechanism. Again I find no evidence of this in the data.

APPENDICES

APPENDIX A

Comparison between CPD (2008) and Okeke (2008)

In order to compare both datasets, the data is truncated in the year 2000 i.e. all migration flows occurring after the year 2000 are excluded. Since the CPD captures emigration as of the year 2000, by restricting the sample to exclude emigration occurring after 2000, it is possible to make both datasets roughly comparable. Despite the differences in definition and methodology, the overall similarities are striking. In the table below I summarize the key differences between both datasets. In Table A.2 migration numbers are compared side by side for each country in the sample and in Figure A.1 and figure A.2 I plot my data against theirs (in logs). If the data were identical then the plot should be a 45-degree line. Notice the strong correlation between both datasets. For the US, both data sets are for all intents and purposes identical, and even for the UK, the degree of correlation is quite strong.

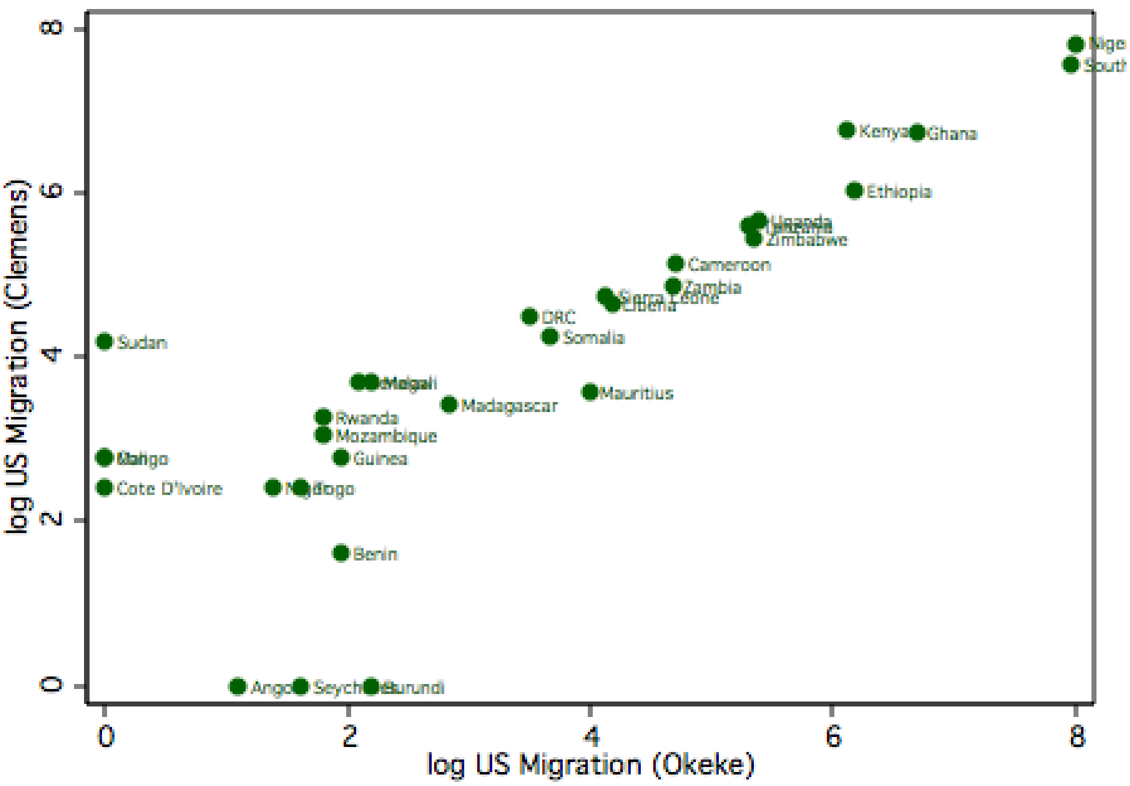

Figure A.1: Correlation (US Migration)

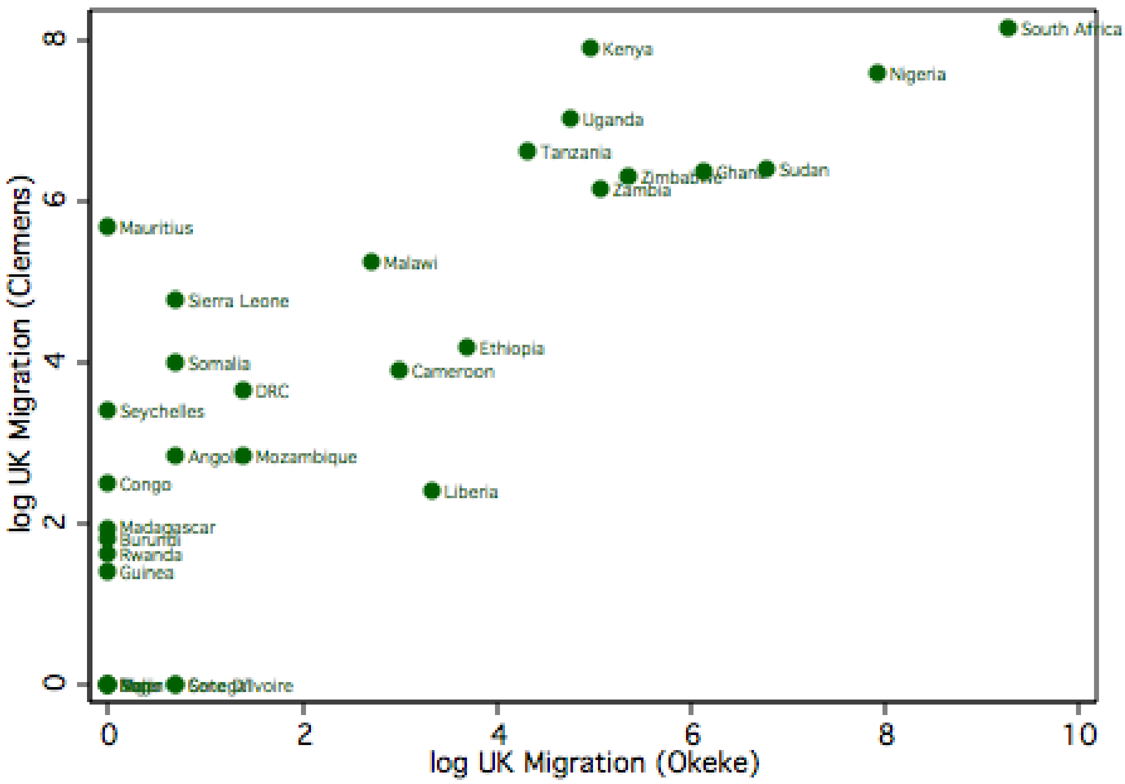

Figure A.2: Correlation (UK Migration)

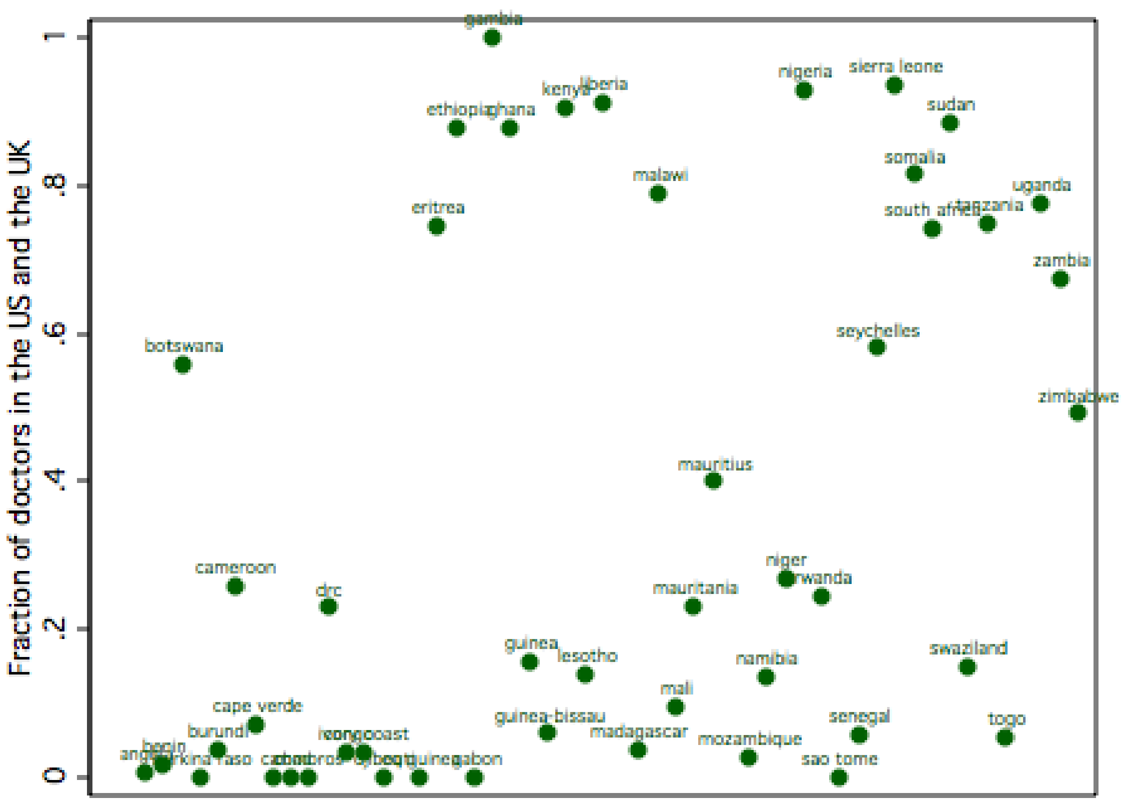

Figure A.3: Fraction of African Physicians abroad (US and UK combined)

Table A.1: Summary of differences

CPD (2008)	**Okeke (2008)**	**Comments**
Includes all African Countries (53 countries)	Includes only sub-Saharan African countries	Excludes all North African countries i.e. Morocco, Libya, Algeria, Egypt, Tunisia
Migrant doctor is defined based on country of birth (does not matter where training was received)	Migrant doctor is defined based on country of medical training i.e. where qualification was received	Excludes 12 countries without a medical school i.e. Botswana, Djibouti, Cape Verde, Comoros, Equatorial Guinea, Eritrea, Gambia, Lesotho, Mauritania, Namibia, Sao Tome and Swaziland
Captures migration to nine receiving countries	Captures migration to two receiving countries - the US and the UK	Excludes countries with zero migration to the US and the UK. Excludes 5 more countries; Burkina Faso, Chad, Central African Republic, Gabon and Guinea-Bissau

Table A.2: Comparison of both datasets

country	**US(1)**	**US(2)**	**UK(1)**	**UK(2)**
Angola	2	0	1	16
Benin	6	4	0	0
Burundi	8	0	0	5
Cameroon	110	170	19	49
Congo	0	15	0	11
Cote D'Ivoire	0	10	1	0
DRC	32	90	3	37
Ethiopia	482	420	39	65
Ghana	801	850	460	590
Guinea	6	15	0	3
Kenya	450	865	143	2733
Liberia	64	105	27	10
Madagascar	16	30	0	6
Malawi	8	40	14	191
Mali	0	15	0	0
Mauritius	53	35	0	294
Mozambique	5	20	3	16
Niger	3	10	0	0
Nigeria	2991	2510	2754	1997
Rwanda	5	25	0	4
Senegal	7	40	1	0
Seychelles	4	0	0	29
Sierra Leone	61	115	1	118
Somalia	38	70	1	53
South Africa	2855	1950	10707	3509
Sudan	0	65	892	606
Tanzania	202	270	74	743
Togo	4	10	0	0
Uganda	217	290	117	1136
Zambia	106	130	160	465
Zimbabwe	210	235	213	553
Total	8746	8404	15630	13239

Source: Okeke dataset (1) and Clemens dataset (2)

APPENDIX B

Data Sources for Chapter III

Table B.1: OECD countries and data sources

Country	Data source	Definition	Available data
Australia	Australian Bureau of Census	Country of birth	1991, 1996, 2001
Austria	Statistik Austria	Country of birth	1991, 2001
Belgium	Institut National de Statistiques	Country of birth	1991, 2001
Canada	Canadian Medical Association	Country of qualification	1994-2004
Denmark	Statistics Denmark	Country of birth	2004, 2005
France	French Medical Association	Country of qualification	1991-2004
Germany	German Medical Association	Country of citizenship	1991-2004
Ireland	Central Statistical Office	Country of birth	1991, 2002
Italy	Instituto nazionale di statistica	Country of citizenship	1991
New Zealand	New Zealand Medical Association	Country of qualification	1991, 2004
Norway	Norway Medical Association	Country of qualification	2004
Portugal	Ordem dos medicos - Lisboa	Country of citizenship	2002, 2003
Sweden	Statistics Sweden	Country of birth	1991, 2003
Switzerland	Office Federal de la Statistique	Country of citizenship	1991, 2000
UK	General Medical Council	Country of qualification	1991-2004
USA	American Medical Association	Country of qualification	1991-2004

Source: Reproduced from Table 1 in Docquier and Bhargava (2007)

APPENDIX C

Visual Inspection with Acetic Acid

Visual Inspection with acetic acid (VIA) is an alternative method of cervical cancer screening which is rapidly gaining traction in developing countries. It has similar sensitivity to the more well-known Pap smear but is much less resource intensive. It involves a visual examination of the cervix and application of 3-5% acetic acid solution which turns areas with cervical dysplasia white, making them more visible. One advantage of this method is that one can adopt a screen and treat approach where women receive screening and treatment (of precancerous lesions) during the same visit. This method of screening is effective; it is safe and it is more affordable for women in poorer countries. There are several methods for treatment of pre-cancerous lesions: (1) Methods that destroy abnormal tissue e.g. cryotherapy (80-90% effective), cold coagulation, laser vaporization. (2) Methods that remove abnormal tissue e.g. cone biopsy and LEEP (loop electrosurgical excision procedure).

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