Did Primary Health Care User Fee Abolition Matter? Reconsidering South Africa's Experience*

Anna S. Brink† and Steven F. Koch‡

Abstract. South Africa waived user fees for primary healthcare in 1994 and, again, in 1996. The first waiver focused on young children, elderly adults, pregnant women and nursing mothers, while the 1996 reform waived fees for the remainder of the population, subject to means tests. We take advantage of household survey information to examine the impact of the policy on a subset of the reform-eligible population. Although it was expected that public healthcare facility usage would have increased post-reform, no statistically significant evidence supported such a claim. Therefore, our results are consistent with some very recent research examining the 1994 reform, but are generally at odds with the general impression in the literature that user fee abolition matters, when it comes to alleviating inequities in access to healthcare.

Keywords: User Fees, Differences-in-Differences, Multinomial Logit

1. Introduction

President Mandela, of the newly elected democratic government of South Africa, announced that primary healthcare services would be provided without charge at all state facilities. The official policy, enacted June 1, 1994, was for children under the age of six years, pregnant and nursing mothers, and the elderly, as long as they were not currently members of a medical scheme (Wilkinson *et al.* 1997; Leatt *et al.* 2006; South African Government 1994). Additional abolition measures ensued, shortly thereafter. Following an announcement on April 1, 1996, all other South Africans, with a few exceptions (members of a medical scheme, and those living in a household

---

* The authors would like to thank seminar participants at the University of Pretoria and the Eastern Mediterranean University, as well as Olufunke Alaba for their comments on earlier drafts of this research. We would also like to thank Economic Research Southern Africa for partial support of this research. All remaining errors are the sole responsibility of the authors.
† MCOM, Department of Economics, University of Pretoria, Private Bag X20, Hatfield 0028, South Africa, a.brink101@gmail.com.
‡ Corresponding Author: Professor and Head of Economics, Department of Economics, University of Pretoria, Private Bag X20, Hatfield 0028, South Africa, steve.koch@up.ac.za.

earning more than R100 000 per year) were allowed access to primary healthcare at public clinics without user charges (McCoy and Khosa 1996; Leatt *et al.* 2006).

The preceding policies have received attention in the literature, although from limited sources of information. According to some, utilization of public health services increased following the 1994 abolition (McCoy and Khosa 1996; Wilkinson *et al.* 1997; Schneider *et al.* 1997; Schneider and Gilson 1999). More recent research, however, is less supportive. With respect to the 1996 reform, however, even the early evidence varies. Both increases (Wilkinson *et al.* 2001; Bayat and Cleaton-Jones 2003) and decreases (Schneider and Gilson 1999; Wilkinson *et al.* 2001) in utilization have been observed.

A number of other African countries have also abolished public healthcare user fees, and much of the available research finds increases in healthcare utilization to be associated with these policies, as well. Increased utilization has been found in Uganda (Nabyonga *et al.* 2005; Burnham *et al.* 2004), Zambia (Masiye *et al.* 2008) and Kenya (Mbugua *et al.* 1995; Mwabu *et al.* 1995). However, reviews of the user fee literature suggest that there are serious problems with these earlier studies, as they tend to focus on only a small set of clinics, are not representative, and do not include a proper control group for comparison (Lagarde and Palmer 2008; Ridde and Morestin 2011).

The preceding reviews have led to a re-examination of policy impacts in both South Africa (Koch 2012; Koch and Racine 2013) and Zambia (Chama-Chiliba and Koch 2014), amongst others. In each of the previous cases, which make use of more recent developments in program evaluation, the re-examination has uncovered minimal effects. However, the 1996 reform has not yet been re-evaluated; thus, it remains an open question: has the 1996 reform offered more benefits than the 1994 reform? In an effort to address this question, we extend the analysis of Koch (2012) and Koch and Racine (2013), who examine only the 1994 reform. Our data includes information collected before and after the 1996 user fee abolition. Since the information is available for both a control group and a treatment group, the analysis hinges upon an application of difference-in-difference (DD) methods. The control group is young children, who had access to free healthcare before the 1996 reform, as well as after. This control group is compared to a set of older children, who did not have access to free public healthcare until after the 1996 policy announcement. Given that the 1996 policy focuses on those without access to a medical scheme (health insurance), and on those living in households with less than R100 000 (1USD=4.35ZAR on April 1, 1996) in earnings, our treatment and control groups are similarly demarcated. The data is taken from the 1995 and 1996 October Household Surveys (Statistics South Africa 1995, 1996) and were combined in an effort to consider healthcare-seeking behavior for children, focusing on facility choice.

Unfortunately, the results find little, if any, meaningful effect. Neither sick nor injured children, at least those who became eligible for free primary care, were more likely to make use of public healthcare facilities. Therefore, the results suggest that extending free healthcare to a wider section of the more vulnerable members of society had no statistically significant impact on the use of that healthcare.

2. Background

Preceding 1994, South African health policy was racially stratified; four race groups were classified and segregated, and each had its own health department with varying levels of resourcing. Healthcare expenditure in fiscal year 1992 was R122 per capita in the poorest districts, but R437 per capita in the richest districts (McIntyre *et al.* 1995). Unsurprisingly, the result was racially stratified health outcomes and healthcare utilization. In the ten years between 1988 and 1998, infant mortality rates were 11.4 and 53.6 for whites and for Africans in non-urban settings, respectively (Medical Research Council *et al.* 1999).

The new government inherited a fractured health system in need of restructuring. In addition to the racially segregated health departments, there was a system of user charges, although the system was not designed as a full cost recovery system. During the 1992 fiscal year, shortly before fees were abolished, fees covered only 4.5% of recurrent expenditure in South Africa (McIntyre 1994), a figure more or less in line with the 5% observed in other African countries (Creese *et al.* 1990). In 1994, free health care, covering all services and all children under the age of six, unless they were covered by a medical scheme, was made available at state health facilities, including hospitals, community health centers, clinics, mobile clinics and hospitals, where at least half the expenditure is subsidized by the state (South African

Government 1994). The 1996 extension was announced in a budget speech, and, despite limiting coverage to clinics and those from relatively poorer households, was unexpected (McCoy and Khosa 1996; Leatt *et al.* 2006).

In addition to the changes in user charges, the South African government unlocked resources for the health of its citizens. Between fiscal years 1992 and 1998, public resources devoted to the public health sector grew between 1% and 1.5% per year in real terms (Thomas *et al.* 2000). The fact that increased resources were devoted to the public health sector is important to the analysis for two reasons. The first is that it shows the South African government's commitment to improved health, raising the expectation of some policy success. The second is that it underscores the importance of the DD approach, described below, and the need for observing the treatment and control groups over time.

3. Methodology

The methodology follows the DD estimator (Card and Krueger 1993; Bertrand *et al.* 2004). Unlike in standard DD, based on linear regression, this analysis is subsumed in a multinomial logit regression. The employed structure is dictated by the analysis of healthcare provider choice, and there are four healthcare choice outcomes: no treatment, treatment at a public clinic, treatment at a non-clinic public health facility, and treatment at a private facility. Estimation via multinomial regression does assume that choices are independent of irrelevant alternatives; thus, for example, the probability of choosing public clinic care relative to private care is assumed to be independent of non-treatment. Although not reported in the analysis tables, we find that this assumption does not appear to be driving the results.

In terms of the policy, since user fees were abolished at public clinics, an increase in public clinic usage is hypothesized. Indirectly, it is also hypothesized that there will be a significant reduction in children not receiving any healthcare, when sick or injured, while the effects at private facilities and non-clinic public facilities are likely to be the reverse, due to substitution effects. Although we have an assumption regarding the substitution patterns, the true patterns are an empirical matter, and, thus, we allow the data to speak, in this regard.

DD analysis is prefaced on fixed-effects panel data estimation, although it can be undertaken with either panel data or repeated cross-section data; the latter is used here. Within DD, it is assumed that there are at least two groups, and that those two groups can be observed over at least two different points in time; for this analysis, there are exactly two groups and two points in time. The two groups will be referred to as younger (control) children and older (treated) children, because the policy affected these groups differently. The younger children, those under the age of six, are the control group, due to fact that these children had access to free primary health care at public clinics in both 1994 and 1996. On the other hand, children aged six years and older are placed in a treatment group, because these children were not included in the 1994 policy announcement, but were included in the 1996 announcement.

To set the stage for understanding DD, we relate it to randomized controlled trials (RCTs). In an ideal RCT, the treatment and control groups would be identical before treatment, while everything outside of the RCT is assumed to remain constant, such that any difference following treatment can be ascribed directly to the treatment. However, outside of the laboratory, it is difficult to keep everything constant. Intuitively, in an ideal setting, DD removes differences between groups over time (Angrist and Pischke, 2009), such that any remaining difference can be ascribed to the treatment.

The operationalization is accomplished through the use of dummy indicator variables. As noted above, the analysis is prefaced on fixed-effect panel analysis. One indicator is required to separate the younger (control) and older (treatment) children, another is used to separate the before and after samples, while a third is used to separate the treated older children in the post-policy sample from all other subgroups; this last indicator variable underpins the DD estimate, and is referred to as the DD indicator. In addition to the indicator variables, a series of control variables are also included in the analysis, to account for the possibility that the initial treatment and control groups are not identical and to control for other aspects of healthcare utilization decisions.

Although one would be tempted to consider only the before and after treatment group difference, that would ignore other changes, for example, the amount of public health care financing available to the public system. Changes in user fee policies, as well as changes in government funding foci could have affected financing, and thus healthcare utilization. However, comparing before and after across the two groups i.e. taking the difference between the differences, removes the bias that would arise from ignoring an important explanatory variable (Bertrand *et al.*, 2004). In DD, the first set of differences is taken at the group level, and these describe the changes within the groups over time. The second difference is taken between groups, after the group differences have been calculated; thus, the second difference measures the relative change between the treatment group and the control group following policy reform. That relative change measures the effect of the policy.

In this analysis, unfortunately, the pre-1996 sample does not have the same characteristics as the post-1996 sample. Therefore, we undertake two further analyses, to see if our results are affected by this sample difference. In the first additional step, as implied above, we considered the differences-in-differences of the controls variables, because identification in DD requires common covariate trends. In other words, it might not matter if the samples are different. For example, as is the case here, the 1996 sample is poorer – see Table A.1 – than the 1995 sample. Importantly, though, old children are not systematically poorer than young children (or vice versa) in 1996 than in 1995. Given that the control variables in the data are found to have common trends, we are not particularly worried about the underlying mean differences identified in the sample. However, for further surety, we took a second step. In the second additional step, we account for the asymmetry through the inclusion of separate time trends amongst the covariates that are found to have significant mean differences across the sample period. As there are only two time periods, variable-specific time trends are merely interactions terms based on a time period indicator crossed with each of the (significant) control variables. In other words, we control for differences in the samples by identifying observations as occurring in one or the other time-period.

Finally, as there are multiple healthcare options from which to choose, policy effects are likely to be substitutable. Benefits are expected to accrue with respect to “public clinic treatment”, and, therefore, some other option is not used. However, the pattern might be complicated, depending upon whether the policy made treatment more likely. Specifically, once an individual is ill, they can choose not to receive any healthcare, they can choose to take advantage of a public clinic, another part of the public sector or the private sector. Thus, reducing the cost of public clinic access could reduce the probability that individuals seek no treatment, or it could reduce the probability of treatment from private providers or non-clinic public providers. Given the multiple options and the required substitutability of options, the DD representation we estimate is set within a multinomial logit framework. However, the parameter estimates from a multinomial regression are not informative about the size of the effect, only the direction. Therefore, effect sizes are calculated from the marginal effect of the DD indicator on each facility choice. The marginal effect of the DD indicator is the estimate of the change in the probability of choosing one of the four facilities.

4. The data

4.1 Data source

Data for the analysis was sourced from two South African October Household Surveys (OHS), one from 1995 and another from 1996. The first of these surveys is the most recent one available preceding the 1996 reform, while the second occurs only a few months after user fees were abolished for all. Data also exists for 1993, 1994, 1997 and 1998. Unfortunately, in 1993 and 1994, the surveys did not properly capture the public-private divide in health care facility use; the 1997 data did not include most of the health questionnaire responses, while the 1998 data could be deemed too many years after the reform. However, it should be noted that earlier analysis did consider 1998; the results from that survey were not all that different from what is reported below, when it comes to the impact of policy on public clinic use.

Each year, the OHS obtains responses from a stratified random sample of individuals in the population, and these can be weighted to match the demographic profile of the country. However, weights are not used in the analysis, since they are meant to reflect the population make-up, and not illness make-up. Stratification is based on province, magisterial district, urban or rural locale and population group, and is set to match the census frames. However, as noted in the methodology, these surveys are different in a variety of respects, which are dealt with in the analysis.

The main purpose of the OHS surveys was to collect information from households and individuals across the nine provinces of South Africa, focusing specifically on socioeconomic development and labor market issues. Although the surveys collect detailed information related to socio-demographics and employment, only limited information related to health and healthcare behavior is available. Health data includes injuries or illnesses in the 30 days prior to the interview, whether or not the individual had access to a medical aid scheme, whether or not care was sought for the illness or injury, and the ownership of the facility (public or private), at which, care was sought.

As the policy reform was meant to cover only those without access to health insurance, and living in households with less than R100 000 in earnings, information on these two variables were needed to limit the sample to an appropriate sample. Furthermore, given that the initial 1994 policy reform only affected children up to the age of six, this analysis focuses only on children near that age, which was chosen to be up to the age of 11. After limiting the sample, the 1995 sample included 1803 children, while the 1996 sample contained 1163 children. Although not reported here, a previous analysis with children up to the age of 18 was also undertaken, and it led to similar results (Brink 2012). The analysis focuses on the treatment and location, thereof, received by an ill or injured child: (i) an ill or injured child receiving no treatment, (ii) an ill or injured child receiving treatment at a public clinic, (iii) an ill or injured child receiving treatment at a non-clinic public facility, or (iv) an ill or injured child receiving treatment at a private facility.

4.2 Control variables

The empirical literature on health facility choice suggests the inclusion of a number of different types of variables, such as price, travel time, illness characteristics, sex, age, education, income, household size, ethnicity and migrant status (Mariko 2003; Pronyk *et al.* 2001; Moïsi *et al.* 2010). The South African empirical literature suggests a number of similar, as well as different, measures: income either in quintiles or in logs, asset measures, race, location, time to facility, insurance access, measures of facility quality, household size, number of dependents, head's education, head's gender, head's age and age squared, the proportion of working to non-working individuals, measures of illness and facility level information, where available (Havemann and van der Berg 2003; Grobler and Stuart 2007; Alaba and Koch 2009). One weakness of our data, unfortunately, is that it does not contain information on either illness condition or quality of care, expected or actual.

Although the OHS data is limited in two important dimensions, quality of care and depth of illness, the data is rich in a number of other dimensions. In terms of demographics, and in line with the previously discussed literature, we include controls for location, ethnicity, and whether or not either of the child's parents is alive. Although it is not possible to be certain whether the child lives with either of their parents, given the structure of the surveys, whether or not either of the child’s parents is alive is used as a proxy.

Household resources that can be mobilized for the care of the sick or injured child are included, as well. As with the previously outlined literature, these measures include: a proxy for income, home ownership, whether or not children in the household went hungry during the past year, due to lack of food. Other resources, household services, also form part of the control variable set, including: tap water, flush toilets, landline or cellular telephones, and refuse that is regularly collected by a local authority.

A number of other variables are included to control for potential opportunity costs of time. We include the distance of the household, in minutes, from the health facility they would most likely use in the event of a need for healthcare; distance is self-reported. Although the distance does not capture resources, it does provide a proxy for the resource use that would be required to treat a sick or injured child. We also include household structure via categorical variables related to the number of children under the age of six (either 0, 1 or more), as well as the number of adults in the household (either 1, 2 or more). We include two additional measures of adult availability, the proportion of employed adults in the household and the proportion of non-labor force participant adults in the household. We also include the proportion of adults in the household that are union members to control for the ability of employed household members to get time away from work, if a child needs additional attention related to healthcare-seeking activities.

In addition to the above controls, education has been shown to be positively associated with the demand for health, theoretically (Grossman 1999) and empirically (Grobler and Stuart 2007). For that reason, the proportion of adults having completed matric (high school) is expected to be associated with an increase in health-seeking behavior, while the proportion of adults without any schooling is expected to reduce it. Along similar lines, the willingness to invest in health is also affected by preferences for health (Grossman 1999), and, therefore, we include the proportion of adults in the household who are smokers to control for health preferences. Although measures of self-reported health or exercise would likely be a better measure, they are not available in the survey.

The most complex data development process was related to the calculation of earnings. In many surveys, including those undertaken in South Africa, it is not uncommon for earnings, either in business or in work, to not be recorded (Simkins 2004). Although a number of options are available, and we considered a number of them, we finally settled on household expenditure. For the 1995 OHS, it is possible to merge the 1995 Income and Expenditure Survey (Leibbrandt *et al.* 2005); thus, it is possible to recover household expenditure. For 1996, on the other hand, it is necessary to trust the self-reported household expenditure information. Importantly, the expenditure information was more complete than the income information, and, since our focus in this analysis is not on the income effect, the measurement effect on the estimated parameters is not as worrisome.

5. Results

5.1 Data summary

The data is summarized in two tables, in Table 1 and Table A.1, separated by survey year. Table 1 outlines the simple differences in the outcome variables over the survey years. Although no other controls are included, the estimates reported in Table 1 are not particularly encouraging, since they suggest increases in non-treatment, and, at best, limited effects of policy on public clinic use. The robustness of these results will be discussed, below.

Table 1. Descriptive Analysis of Outcome Variables

Outcomes	1995	1998
notreat	0.234	0.318***
	(0.01)	(0.02)
pubclin	0.277	0.320**
	(0.01)	(0.02)
pubothr	0.295***	0.182
	(0.01)	(0.01)
private	0.194	0.180
	(0.01)	(0.01)
Observations	1,938	1,168

Source: Author's calculations.
Clustered by Household Standard Errors in parentheses. *** p <0.01, ** p <0.05

Table A.1, on the other hand, describes the data in the two samples. As has been noted, there are a number of differences between the children over the two sample periods, and, thus a more careful analysis was also undertaken. To summarize, the data in 1996 is decidedly less urban, and contains fewer Asians and coloured children, but more black children. In 1996, compared to 1995, fewer children are living in the Eastern Cape and KwaZulu Natal, while more children are living in Gauteng and Limpopo provinces. More households in 1996 have one child under the age of six, while fewer have no children under the age of six. Households in 1996 spent less, but are more likely to own their home, than in 1995. However, children in those households were less likely to go hungry at some point during 1995 than 1996. In terms of the adult proportional variables, there is a switch between NLFP and employment, with the NLFP proportion higher in 1996 than 1995, while the opposite is true for the employment proportion; there is also some evidence of an increase in the proportion of adults having completed matric in 1996. Despite these statistically significant differences across sample years, there is little in the way of differential trends. In other words, the reductions or increases observed across the samples are not dependent on the age groups used in the analysis. Thus, we are confident that the results reported below are an appropriate representation of the average policy effect.

5.2 DD estimates

Two sets of multinomial regressions were estimated. In the first, the variables described in the data section were used to control for potential confounding factors that could influence the choice of healthcare facility. The multinomial logit estimates for this regression can be found in Table B.1. In the second analysis, trends in the control variables were also included. The multinomial logit regression estimates for the second analysis can be found in Table B.2.

However, the primary focus of the analysis is the impact of the 1996 policy reform, which was estimated as the marginal effect of the DD indicator for each healthcare utilization outcome in the multinomial logit regression. The marginal effect was calculated as the difference in the probability for all of the alternatives in the model. The effect is the probability of using that alternative post-reform, if in the post-reform eligible subgroup, net of the probability of using that alternative pre-reform. As the probabilities are predicted from nonlinear functions, it is important to set the control variables to a particular level. We do so by considering the median household; in South Africa, the median household is relatively poor, its young children were eligible for the 1994 reform, and, therefore, any of that household's older children would be covered by the 1996 reform. With respect to the results reported, regardless of whether or not control variable trends were incorporated, they are disappointing. User fee abolition had no statistically significant effect on the healthcare facility choice decisions made for ill and injured children.

Table 2. DD Estimates for Multinomial Logit Regression Models with and without Year Interaction Terms

	Wiithout Year Interactions			With Year Interactions		
Outcome	Model 2	Model 3	Model 4	Model 2	Model 3	Model 4
No Treatment	-0.060**	-0.063**	-0.063**	-0.056	-0.065*	-0.055
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Public Clinic	0.015	0.016	0.018	-0.020	-0.019	-0.029
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Public Other	0.039	0.043	0.038	0.015	0.012	0.016
	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)
Private	0.006	0.004	0.007	0.061	0.072	0.068
	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)	(0.05)
Log-Likelihood	-4073.0	-4043.7	-4014.1	-4021.9	-3978.5	-3935.6
Observations	3106	3106	3106	3106	3106	3106

Source: Author's calculations of Difference-in-Difference Marginal Effects at the Mean of the Data. Robust standard errors in parenthesis, clustered by household. ** p<0.05, * p<0.1.

In summary, the policy has had no positive effect on overall treatment. Overall, treatment has not increased; in fact the sign suggests more children are not receiving children. Public clinic use has not risen significantly, although the sign is as expected. Similarly, private facility use is lower, even if not statistically significantly so.

One worry that arises in the multinomial regression setting is that the independence of irrelevant alternatives assumption could be violated. A violation of the assumption suggests that the results could be dependent upon the set of alternatives within the model structure. Therefore, we considered a few other specifications with a different set of alternatives: (i) public clinics vs. all others, (ii) public facilities vs. all others, (iii) public clinics vs. non-treatment and (iv) public facilities vs. non-treatment. There are no substantive differences in the conclusions, and, thus, we are comfortable assuming that independent alternatives are irrelevant, at least in this setting (results available from authors upon request).

5.3 Discussion

The DD analysis points to a statistically insignificant policy effect, when comparing no treatment, public clinic usage, other public facility usage and private facility usage. Since user fees were cut for primary care in the public sector, which is primarily delivered through the clinic structure, we expected a positive effect on public clinic usage, and a reduction somewhere else, to offset the increase. Our results generally disagree with the findings previously reported in the user fee literature, especially the early literature outside of South Africa; however, the literature on the effects of curative care utilization following user fee changes inside and outside of South Africa is not extensive. In South Africa, for example, only a few studies are available. Curative care is found to increase amongst children under the age of six, following the 1994 policy change, and there was an observed increase in adult curative care services over the 1992-1998 period (Wilkinson *et al.* 2001; Lagarde and Palmer 2008). Recent analysis, making use of more representative data related to the 1994 policy reform finds increases unlikely to exceed 5% (Koch 2012; Koch and Racine 2013), a figure much lower than the 44.7%, 77.3% and 300% implied by others (Wilkinson *et al.* 1997; Bayat and Cleaton-Jones 2003; McCoy and Khosa 1996).

A few studies are available in other African countries, as well. In Madascar, curative care visits increased by 17% (Fafchamps and Minten 2007). In Uganda, studies have found 28% increases in under–five visits and 53% increases in new visits (Burnham *et al.* 2004). In the same country, others have observed 25% increases at public facilities and 44% increases at referral centers (Nabyonga *et al.* 2005), as well as an 18% increase in care for under fives and a 26% increase in care for all (Deininger and Mpuga 2004). In a related study, informal care increased by 10%, public service care increased by nearly 11%, while non-use increased by 16%, which suggests that user fee abolition effects in Uganda were not as pronounced as was found in previous studies (Deininger and Mpuga 2004; Xu *et al.* 2006; McPake *et al.* 2011). Finally, in Zambia, increases in rural district utilization have been found nearer to 50% (Masiye *et al.* 2008), although this effect does not carry through to child delivery services (Chama-Chiliba and Koch 2014).

There are a number of reasons our results could differ from the rest of the literature. All but a few of the previous studies (Deininger and Mpuga 2004; Xu *et al.* 2006; Koch 2012; Koch and Racine 2013) collected data from healthcare facilities, some from as few as one facility. Furthermore, as pointed out in a detailed review of the literature, the data used in the previous analyses is likely to be unreliable, due to the limited number of facilities, while control groups were lacking. Therefore, the analyses were unlikely to be able to account for a number of changes that occurred around the same time user fees changed (Lagarde and Palmer 2008). A similar concern can be raised with respect to the earlier household level studies (Deininger and Mpuga 2004; Xu *et al.* 2006). Control groups, for comparison purposes, are not available, and, therefore, the increases observed in those studies could reflect broad increases in the country, rather than increases that can be attributed directly to the policy.

Although the sample employed in this analysis is more representative than the samples employed in a number of other studies, data both before and after the policy change contains a large number of observations, and the analysis is based on an empirical model designed to deal with a variety of confounding factors, there are limitations to the analysis. The 1996 policy followed closely on the heels of an earlier policy abolishing user fees in the public healthcare sector for young children, nursing and pregnant women and the elderly, which was implemented in 1994. It is possible that the time horizon was too short to uncover an effect. It is also the case that the two samples used for the analysis were not as similar as would have been preferred. However, we uncovered no significant differences in sample trends across the treatment and control groups, while adjusting for sample differences, through the inclusion of additional control variables, did not alter the substantive conclusions. Also, it should not be forgotten that policy implementation on the ground does not always match the policy goal. Unfortunately, it is not possible to go back and see how effectively these policies were implemented, or whether the implementation of these policies increased public clinic queues and waiting times. Although there is some evidence of imperfect user fee abolition implementation, there is rather strong evidence that fees were less likely to be paid by those eligible for the 1994 reform (Koch 2012).

Another limitation of the study arises from the lack of clinic-level information for corroboration. It would have been preferable to compare the results reported here to a broader set of clinic-level information. Efforts have been ongoing to uncover clinic-level information, but have been largely unfruitful. Even though it would have been nice to compare individual level results with clinic-level effects, any analysis at the clinic-level would not be able to consider substitution across health facilities, and, therefore, this analysis, despite uncovering no substitution, has something to offer that clinic-level studies do not.

Finally, the policy impact was estimated for a rather narrow band of the population, albeit a band of the population that was targeted for the policy intervention. Given the narrow range of the target population, it cannot be concluded that the policy had no impact, only that there was no statistically identifiable impact on public clinic utilization in the analysis population. Although extending the analysis to other children did not affect the results (Brink 2012), it is possible that the policy had a greater effect on adults. Adults were not included in the analysis, because the treatment and control groups needed to be similar. We felt that the inclusion of adults was not appropriate, because of the decision-making process. Primary caregivers are assumed to make healthcare decisions for very young children, who are in the control group. Therefore, it was preferred that primary caregivers also made the decisions for the treatment group. Since adults typically make their own healthcare choices, adults are not an empirically relevant treatment group.

6. Conclusion

South Africa has availed public health facilities to more individuals by waiving user fees for primary health care, first in 1994, and again, in 1996. Since the 1994 plan focused on young children and elderly adults, as well as pregnant and nursing mothers, the 1996 change, which waived fees for the remainder of the population, subject to means tests, was examined via DD applied to a subsample of young children. Although the policy provided free primary care to all at public clinics, the results of the analysis do not support the hypothesis that free primary care increased public clinic visits amongst ill and injured children.

Given the lack of empirical support for the hypothesis, coupled with the broad support in the empirical literature for utilization increases associated with the 1994 user fee abolition, further abolishing user fees to more of the population was not an effective option for improving the utilization of public health facilities in South Africa. The result further suggests that barriers to accessing health care for this segment of the population is not user fees; rather, there are other barriers that matter. Therefore, in order to improve the utilization of public health facilities, future research should focus on developing a deeper understanding of healthcare access barriers, and develop policies to address those barriers.

References

Alaba, OA, Koch, SF. 2009. Dynamic health care decisions and child health in South Africa. *Journal of Income Distribution*, 18(3-4): 188-205.

Angrist, JD, Pischke, J-S. (2009) Mostly Harmless Econometrics: An Empiricist's Companion. Princeton University Press, Oxfordshire, UK.

Bayat, A, Cleaton-Jones, P. 2003. Dental clinic attendance in Soweto, South Africa, before and after the introduction of free primary dental services. *Community Dentistry and Oral Epidemiology* **31**(2): 105–110.

Bertrand, M, Duflo, E, Mullainathan, S. 2004. How much should we trust difference-in-difference estimates? *Quarterly Journal of Economics* **119**(1)**:** 249-275.

Brink, AS. 2012. User Fee Abolition and its Effect on the Demand for Healthcare in

South Africa. *University of Pretoria MCOM*.

Burnham, GM, Pariyo, G, Galwango, E, Wabwire-Mangen, F. 2004. Discontinuation of cost sharing in Uganda, *Bulletin of the World Health Organization* 82(3): 187–195.

Card, D, Krueger, AB. 1993. Minimum wages and employment: A case study of the fast food industry in New Jersey and Pennsylvania. *American Economic Review* **84:** 772–793.

Chama-Chiliba, CM, Koch, SF. 2013. Assessing regional variations in the effect of the removal of user fees on institutional deliveries in rural Zambia. *Economic Resarch Southern Africa Working Paper 427*.

Creese, AL. 1990. User-charges for health care: a review of recent experience. World Health Organisation.

Deininger, K, Mpuga, P. 2005. Economic and welfare impact of the abolition of user fees: evidence from Uganda. *Journal of African Economies* **14**(1): 55–91.

Fafchamps M, Minten B. 2007. Public service provision, user fees, and political turmoil. *Journal of African Economies* **16**: 485–518.

Grobler, C, Stuart, IC. 2007. Health care provider choice. *South African Journal of Economics* **75**(2): 327–350.

Grossman, M. 1999. The human capital model and the demand for health. *NBER Working Paper 7078*.

Havemann, R, van der Berg, S. 2003. The demand for health care in South Africa. *Journal of Studies in Economics and Econometrics* **27**(3): 1–27.

Koch, SF. 2012. The abolition of user fees and the demand for health care: re-evaluating the impact. *Economic Research Southern Africa Working Paper 307.*

Koch, SF, Racine, JS. 2013. Healthcare facility choice and user fee abolition: Regression discontinuity in a multinomial choice setting. *Economic Research Southern Africa Working Paper No. 373*.

Lagarde M, Palmer N. 2008. The impact of user fees on health service utilization in low- and middle-income countries: how strong is the evidence? *Bulletin of the World Health Organization* **86**: 839–848.

Leatt, A, Shung-King, M, Monson, J. 2006. Healing inequalities: The free health care policy, in J. Monson, K. Hall, C. Smith and M. Shung-King, eds, South African Child Gauge, Children’s Institute, University of Cape Town, pp. 51–

56.

Leibbrandt, M, Levinsohn, J, McCrary, J. 2005. Incomes in South Africa since the fall of Apartheid. *NBER Working Paper No 11384*.

Mariko, M. 2003. Quality of care and the demand for health services in Bamako, Mali: the specific roles of structural, process and outcome components. *Social Science & Medicine* **56**: 1183–1196.

Masiye, F, Chitah, BM, Chanda, P, Simeo, F. 2008. Removal of user fees at primary health care facilities in Zambia: a study of the effects on utilization and quality of care. *Regional Network for Equity in Health in east and southern Africa Discussion Paper 57*.

Mbugua JK, Bloom GH, Segall MM. 1995. Impact of user charges on vulnerable groups: the case of Kibwezi in rural Kenya. *Social Science& Medicine* **41**: 829–835.

McCoy, D, Khosa, S. 1996. Free health care policies, in D Harrison, P Barron and J Edwards, eds, South African Health Review 1996, Health Systems Trust and Henry J. Kaiser Family Foundation, pp. 157–165.

McIntyre, D. 1994. User fees for public sector health services in South Africa: revenue generating potential and equity implications, *Health Economics Unit Working Paper No. 6*, University of Cape Town.

McIntyre D, Bloom G, Doherty J, Brijlal P. 1995. *Health expenditure and finance in South Africa.* Durban: The Health Systems Trust and the World Bank.

McPake, B, Brikci, N, Cometto, G, Schmidt, A, Araujo, E. 2011. Removing user fees: learning from international experience to support the process. *Health Policy and Planning* **26**: ii104-ii117.

Medical Research Council, MACRO International, Department of Health. 1999. *South Africa Demographic and Health Survey 1998.* Pretoria: Department of Health.

Moïsi, JC, Gatakaa, H, Noor, AM, *et al.* 2010. Geographic access to care is not a determinant of child mortality in a rural Kenyan setting with high health facility density. *BMC Public Health* **10**(142), pp. 9.

Mwabu, G, Mwanzia, J, Liambila, W. 1995. User charges in government health facilities in Kenya: effect on attendance and revenue. *Health Policy and Planning* **10**(2): 164–170.

Nabyonga, J, Desmet, M, Karamagi, H *et al.* 2005. Abolition of cost-sharing is propoor: evidence from Uganda. *Health Policy and Planning* **20:** 100–108.

Pronyk, PM, Makhubele, MB, Hargreaves, JR *et al*. 2001. Assessing health seeking behaviour among tuberculosis patients in rural South Africa. *The International Journal of Tuberculosis and Lung Disease* **4**(7): 619–627.

Ridde, V, Morestin, F. 2011. A scoping review of the literature on the abolition of user fees in health care services in Africa. *Health Policy and Planning* **26**(1): 1–11.

Schneider H, Gilson L. 1999. The impact of free maternal healthcare in South Africa. In: Berer M, Sundari Ravindran, TK (eds). Safe Motherhood Initiatives: Critical Issues. Oxford: Blackwell Science for Reproductive Health Matters, pp. 93–101.

Schneider H, Kaka Z, Jina S *et al*. 1997. A survey to assess the impact of free primary healthcare on the Soweto Community Health Centres. Technical Report. Johannesburg: Centre for Health Policy, University of Witwatersrand.

Simkins, C. 2004. What happened to the distribution of income in South Africa between 1995 and 2001? Unpublished Manuscript. Johannesburg: University of the Witwatersrand, Department of Economics.

South African Government. 1994. Free health services for pregnant women and children under the age of 6 years. Government Notice 657, Government Gazette 15817.

Statistics South Africa. 1995. October Household Survey (South Afica), 1995. Statistics South Africa (Producer) and South African Data Archive (Distributor).

Statistics South Africa. 1996. October Household Survey (South Afica), 1996. Statistics South Africa (Producer) and South African Data Archive (Distributor).

Thomas, S, Muirhead, D, Doherty, J, Muheki, C. 2000. Public Sector Financing, in A Ntuli, ed, South African Health Review 2000, Health Systems Trust, pp. 127–148.

Wilkinson, D, Gouws, E, Sach, M, Abdool Karim, S. 2001. Effect of removing user fees on attendance for curative and preventive primary health care services in rural South Africa. *Bulletin of the World Health Organization* **79**(7): 665–671.

Wilkinson, D, Sach, M, Abdool Karim, S. 1997. Examination of attendance patterns before and after introduction of South Africa's policy of free health care for children aged under 6 years and pregnant women. *British Medical Journal* **314**(7085): 940–941.

Xu K, Evans DB, Kadama P *et al*. 2006. Understanding the impact of eliminating user fees: Utilization and catastrophic health expenditures in Uganda. *Social Science & Medicine* **62**: 866–876.

Table A.1. Descriptive Statistics of Control Variables

VARIABLES	1995	1998	VARIABLES	1995	1998
Survey Year	0.000	1.000	Father Alive	0.881	0.914***
	(0.00)	(0.00)	(=1 if father is known to still be alive)	(0.01)	(0.01)
Treated Group	0.429	0.404	Mother Alive	0.973	0.986*
(=1 if child is at least six years old)	(0.01)	(0.01)	(=1 if mother is known to still be alive)	(0.00)	(0.00)
DD Indicator	0.000	0.404	Distance to Medical Facility: 15-30 mins	0.336	0.343
(=1 if age >=6 & year=1998)	(0.00)	(0.01)	(=1 if household lives 15-30 minutes away)	(0.01)	(0.02)
Urban	0.503***	0.440	Distance to Medical Facility: 30-60 mins	0.195	0.277***
(=1 if lives in urban locale)	(0.01)	(0.02)	(=1 if household lives 30-60 minutes away)	(0.01)	(0.02)
African	0.788	0.867***	Distance to Medical Facility: >60 mins	0.157	0.169
(=1 if black african)	(0.01)	(0.01)	(=1 if household lives at least 60 minutes away)	(0.01)	(0.01)
White	0.030	0.025	Positive Reported Income	0.221	0.590***
(=1 if white)	(0.00)	(0.01)	(=1 if household has reported earnings)	(0.01)	(0.02)
Asian	0.040***	0.021	Own Home	0.468	0.807***
(=1 if Asian)	(0.01)	(0.01)	(=1 if household lives in a home they own)	(0.01)	(0.01)
Coloured	0.141***	0.086	Access to Tap Water	0.585	0.759***
(=1 if mixed race)	(0.01)	(0.01)	(=1 if have tap water in or near house)	(0.01)	(0.02)
No Children <6	0.204	0.188	Access to Flush Toilet	0.405**	0.360
(=1 if no children under 6 in household)	(0.01)	(0.01)	(=1 if have a flush toilet in or near house)	(0.01)	(0.02)
One Child <6	0.424	0.485***	Share Toilet	0.138	0.165**
(=1 if one child under 6 in household)	(0.01)	(0.02)	(=1 if have to share toilet with neighbors)	(0.01)	(0.01)
Two or more <6	0.373**	0.327	Access to Phone	0.170	0.166
(=1 if two children under 6 in household)	(0.01)	(0.02)	(=1 if own or rent phone)	(0.01)	(0.01)
One Adult	0.109	0.152***	Refuse Regularly Collected	0.460***	0.394
(=1 if only one adult in household)	(0.01)	(0.01)	(=1 if refuse is regularly collected)	(0.01)	(0.02)
Two Adults	0.401	0.419***	Log Income	2.058	4.823***
(=1 if two adults are in the household)	(0.01)	(0.02)	(= natural log of reported earnings + 1)	(0.11)	(0.15)

Two + Adults	0.489	0.430
(=1 if more than two adults in household)	(0.01)	(0.02)
Western Cape	0.084**	0.059
(=1 if lives in Western Cape)	(0.01)	(0.01)
Eastern Cape	0.208***	0.130
(=1 if lives in Eastern Cape)	(0.01)	(0.01)
Northern Cape	0.046	0.057
(=1 if lives in Northern Cape)	(0.01)	(0.01)
Free State	0.077	0.066
(=1 if lives in Free State)	(0.01)	(0.01)
KwaZulu-Natal	0.288***	0.216
(=1 if lives in KwaZulu-Natal)	(0.01)	(0.01)
Northwest	0.101	0.111
(=1 if lives in Northwest Province)	(0.01)	(0.01)
Gauteng	0.089	0.081
(=1 if lives in Gauteng Province)	(0.01)	(0.01)
Mpumalanga	0.079	0.132***
(=1 if lives in Mpumalanga Province)	(0.01)	(0.01)
Limpopo	0.027	0.148***
(=1 if lives in Limpopo Province)	(0.00)	(0.01)
Log Income Squared	19.361	41.358***
(= squared value of log income)	(1.08)	(1.44)
Prop Adults in Union	0.040	0.043***
(= proportion of adults belong to labour union)	(0.00)	(0.00)
Prop Adults NLFP	0.231	0.288
(= proportion adults not participating in work)	(0.00)	(0.01)
Prop Adults Employed	0.366***	0.158
(= proportion of adults that are employed)	(0.01)	(0.01)
Prop Adults Matric	0.073*	0.066
(= proportion of adults with matric certificate)	(0.00)	(0.00)
Prop Adults No School	0.312	0.384***
(= proportion of adults without any schooling)	(0.01)	(0.01)
Children Hungry (past year)	0.572***	0.437
(=1 if children ever went hungry in past year)	(0.01)	(0.02)
Prop Adult Smokers	0.129***	0.101
(= proportion of adults that are smokers)	(0.00)	(0.00)
Observations	1,938	1,168

Source: 1995 and 1998 OHS. Standard errors in parenthesis, clustered by household. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table B.1. Multinomial Regression Results without Year Interactions

	Model 2			Model 3			Model 4		
VARIABLES	Pub Clinic	Pub Other	Private	Pub Clinic	Pub Other	Private	Pub Clinic	Pub Other	Private
Survey Year	-0.339**	-0.995***	-0.506***	-0.198	-1.032***	-0.486***	-0.243	-1.013***	-0.371*
	(0.15)	(0.17)	(0.17)	(0.18)	(0.19)	(0.18)	(0.20)	(0.21)	(0.22)
Older Children	-0.671***	-0.671***	-0.665***	-0.674***	-0.674***	-0.658***	-0.705***	-0.692***	-0.639***
	(0.15)	(0.15)	(0.17)	(0.15)	(0.15)	(0.17)	(0.15)	(0.15)	(0.17)
Free Care Older Children	0.296	0.387*	0.275	0.311	0.415*	0.280	0.315	0.394*	0.291
	(0.21)	(0.22)	(0.24)	(0.21)	(0.22)	(0.24)	(0.21)	(0.22)	(0.24)
Free State	-1.054***	-1.078***	0.322	-1.123***	-1.087***	0.318	-1.111***	-1.081***	0.306
	(0.37)	(0.34)	(0.34)	(0.37)	(0.35)	(0.35)	(0.37)	(0.35)	(0.35)
Northwest	0.479	0.002	0.676*	0.503	0.080	0.767**	0.512	0.080	0.751**
	(0.35)	(0.33)	(0.36)	(0.35)	(0.34)	(0.37)	(0.35)	(0.34)	(0.37)
Mpumalanga	0.345	-0.272	0.845**	0.376	-0.215	0.892**	0.383	-0.189	0.867**
	(0.35)	(0.34)	(0.36)	(0.36)	(0.35)	(0.37)	(0.36)	(0.35)	(0.37)
Urban	-0.039	0.190	0.414***	-0.047	0.214	0.149	-0.084	0.189	0.169
	(0.14)	(0.15)	(0.16)	(0.21)	(0.22)	(0.23)	(0.21)	(0.22)	(0.23)
Asian	-1.107**	-0.122	0.461	-0.997**	-0.180	0.314	-0.866*	-0.154	0.309
	(0.45)	(0.34)	(0.33)	(0.46)	(0.35)	(0.34)	(0.47)	(0.35)	(0.35)
White	-1.232**	0.109	0.961***	-0.998*	0.019	0.795**	-0.766	0.040	0.758**
	(0.50)	(0.36)	(0.34)	(0.53)	(0.39)	(0.37)	(0.55)	(0.40)	(0.38)
Two Adults	-0.350*	-0.144	-0.368*	-0.345*	-0.184	-0.379*	-0.312	-0.181	-0.423*
	(0.19)	(0.21)	(0.22)	(0.20)	(0.21)	(0.22)	(0.20)	(0.22)	(0.23)
Two + Adults	-0.509***	-0.522**	-0.692***	-0.458**	-0.552***	-0.688***	-0.395*	-0.529**	-0.689***
	(0.19)	(0.21)	(0.21)	(0.19)	(0.21)	(0.22)	(0.21)	(0.23)	(0.24)
Med Facility: > 60 mins	-0.568***	0.114	-0.372	-0.618***	0.123	-0.321	-0.635***	0.147	-0.305
	(0.20)	(0.21)	(0.23)	(0.21)	(0.21)	(0.23)	(0.21)	(0.21)	(0.23)
Own Home				-0.299**	-0.159	-0.143	-0.291**	-0.148	-0.151
				(0.14)	(0.14)	(0.15)	(0.14)	(0.14)	(0.15)
Toilet Shared				-0.072	0.061	0.337*	-0.070	0.075	0.328*

				(0.18)	(0.18)	(0.19)	(0.18)	(0.18)	(0.19)
Prop Adults Matric							-1.936***	-0.321	0.693
							(0.57)	(0.49)	(0.48)
Observations		3,106			3,106			3,106	

Source: Multinomial logit results from STATA 12. Robust standard errors in parentheses, clustered by household. *** p<0.01, ** p<0.05, * p<0.1
Non-treatment is base category in all models.

Table B.2. Multinomial Regression Results with Year Interactions

	Model 2			Model 3			Model 4		
VARIABLES	Pub Clinic	Pub Other	Private	Pub Clinic	Pub Other	Private	Pub Clinic	Pub Other	Private
Year	0.519	0.005	-2.622*	0.139	-0.088	-3.541**	0.472	0.177	-3.130*
	(1.40)	(1.31)	(1.58)	(1.47)	(1.38)	(1.68)	(1.55)	(1.42)	(1.76)
Group	-0.616***	-0.630***	-0.766***	-0.632***	-0.642***	-0.789***	-0.641***	-0.660***	-0.754***
	(0.15)	(0.16)	(0.19)	(0.15)	(0.16)	(0.19)	(0.16)	(0.16)	(0.19)
Treated	0.154	0.280	0.528*	0.195	0.310	0.617**	0.118	0.281	0.558*
	(0.25)	(0.27)	(0.29)	(0.25)	(0.27)	(0.30)	(0.25)	(0.27)	(0.30)
Free State	-1.063***	-0.647*	0.512	-1.112***	-0.717*	0.563	-1.099***	-0.698*	0.532
	(0.40)	(0.36)	(0.38)	(0.40)	(0.38)	(0.39)	(0.40)	(0.37)	(0.39)
Northwest	0.472	0.442	0.823**	0.471	0.485	0.881**	0.502	0.489	0.869**
	(0.37)	(0.35)	(0.39)	(0.38)	(0.36)	(0.40)	(0.37)	(0.36)	(0.40)
Urban	0.060	0.242	0.355*	0.134	0.343	0.175	0.124	0.345	0.247
	(0.17)	(0.18)	(0.20)	(0.24)	(0.25)	(0.28)	(0.24)	(0.25)	(0.28)
Asian	-1.076*	0.060	0.871**	-0.904	0.045	0.719*	-0.894	0.017	0.665
	(0.57)	(0.40)	(0.40)	(0.58)	(0.41)	(0.41)	(0.60)	(0.41)	(0.42)
White	-1.785***	0.096	1.038**	-1.519**	0.036	0.877*	-1.437**	0.023	0.828
	(0.66)	(0.43)	(0.44)	(0.69)	(0.46)	(0.48)	(0.73)	(0.48)	(0.50)
One Child <6	-0.223	-0.310	-0.453*	-0.221	-0.308	-0.501**	-0.183	-0.268	-0.480*
	(0.23)	(0.22)	(0.25)	(0.23)	(0.22)	(0.25)	(0.25)	(0.25)	(0.27)
Two + Adults	-0.586**	-0.598**	-0.676**	-0.524**	-0.599**	-0.651**	-0.545*	-0.594*	-0.652**
	(0.27)	(0.28)	(0.31)	(0.27)	(0.28)	(0.31)	(0.29)	(0.30)	(0.33)
Med Facility: 30-60 mins	-0.108	0.143	-0.575**	-0.141	0.110	-0.499*	-0.155	0.121	-0.484*
	(0.21)	(0.23)	(0.26)	(0.22)	(0.23)	(0.27)	(0.22)	(0.23)	(0.27)
Med Facility: > 60 mins	-0.608***	0.085	-0.389*	-0.654***	0.100	-0.341	-0.653***	0.131	-0.326
	(0.20)	(0.21)	(0.23)	(0.21)	(0.21)	(0.23)	(0.21)	(0.22)	(0.23)
Own Home				-0.275*	-0.099	-0.238	-0.229	-0.068	-0.254
				(0.16)	(0.16)	(0.18)	(0.16)	(0.16)	(0.18)
Toilet Shared				-0.033	0.092	0.366*	-0.027	0.113	0.355*
				(0.18)	(0.18)	(0.19)	(0.18)	(0.18)	(0.19)
Prop Adults Matric							-1.896***	-0.311	0.704