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THE ABOLITION OF USER FEES AND THE DEMAND FOR HEALTH CARE: RE-EVALUATING THE IMPACT

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ABSTRACT. The impact of the abolition of user fees in South Africa, a policy implemented in 1994 for children under the age of six and the elderly, as well as pregnant and nursing mothers, is examined via regression discontinuity. The analysis focuses on provider choice decisions for curative care treatment, but also examines potential externalities that could arise from the policy. As a result of the policy, curative care demand in the public sector is found to increase by approximately 7%; however, the demand for curative care in the private sector is found to decrease by nearly the same amount, suggesting that the policy led to provider choice substitution. The analysis further supports the hypothesis that the health of young children improved marginally.

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1. Introduction

In 1994, the newly elected president of South Africa, Nelson Mandela, announced a wide-ranging health care policy that included free access to primary health care for young children and the elderly, as well as pregnant and nursing mothers; free primary health care was extended to everyone in 1996. The policy announcement, affecting delivery in the public sector, was strongly influenced by the Declaration of Alma-Ata, made in September 1978.[1] Underpinned by the tenets contained in the declaration, the change in policy was expected to improve access to health care for the South African population and help alleviate health inequalities.

Economically, the effect of health care user fees, the creation of additional costs to accessing health care, would be expected to decrease the use of health care services. Similarly, the abolition of user fees would be expected to have the opposite effect to the imposition of user fees. Fuchs (1968), for example, argues that health demand, like the demand for other goods, is determined by willingness and ability to pay, rather than desire, want or need. Grossman (1999) furthers this notion by modelling health demand from the desire to maximize utility, where health is assumed to affect both utility and the constraints, such that health demand is a derived demand, having the expected properties.[2]

Given economic theory, it is not surprising that the limited literature examining user fee abolition in South Africa, post 1994, has generally uncovered increased demand. McCoy & Khosa (1996) find rather large average changes in clinic attendance records in the twelve months following the change; see Figures C.1, C.2 and C.3. However, that analysis focuses primarily upon the pregnant and nursing mothers component of the policy change, and is limited to a small number of

[1]The declaration proclaims that health is a fundamental human right, and that to achieve the "promotion and protection of the health of the people", primary health care - "essential health care based on practical, scientifically sound and socially acceptable methods and technology made universally accessible to individuals" - should provide "promotive, preventative, curative and rehabilitative services".

[2]Economic theory would also predict that the increases in access costs would negatively impact welfare, although if health care demand is price inelastic, user fees would have minimal welfare consequences, Heller (1982). However, Gertler, Locay & Sanderson (1987) find higher price responsiveness amongst the poor, suggesting that user fess are regressive.

clinics. Wilkinson, Sach & Abdool Karim (1997) examine attendance patterns at only one mobile clinic in Hlabisa health district in KwaZulu-Natal. They find only minor changes in under-6 usage patterns; however, the small sample limits its usefulness. Wilkinson, Gouws, Sach & Abdool Karim (2001) extend the analysis to include both the 1994 and 1996 user fee changes, but continue only to make use of registration data from the aforementioned mobile clinic. Their data was separated into antenatal care and curative care, as well as under-6 immunization and growth monitoring, the latter two of which had always been freely available. Their analysis includes both separate regressions before and after the policy changes and separate intercepts for the policy change. Unfortunately, the results of the two analyses contradict each other. The first implies an antenatal care (attendance) increase of about 44%, an increase in under-6 care of about 9%, and an increase in curative care of about 49%. For new registrations, the increases are: 23% for antenatal care, 9% for under-6 care and 59% for curative care. However, the second analysis suggests total attendance decreases on the order of 14%, 9% and 42%, respectively, while new registrations fall by 8%, 20% and 23%, respectively. The limited nature of the sample and the lack of consistency in their results raises doubts regarding the impact of the policy, as well as their strategy for empirical identification.

Not only did the policy change affect access for physical health, it also affected access for oral health. Bayat & Cleaton-Jones (2003), acknowleding this feature, examine dental clinic attendance in Soweto, following the 1996 policy change. An improvement in their study, relative to those previously discussed, is that they have monthly attendance records for nine state-funded clinics, as well as one pay-clinic, the latter of which was not affected by policy. In the nine clinics, attendance increased substantially (50%), while the increase at the pay clinic was smaller (11%), suggesting a net increase, or policy impact, of 39%. In a further breakdown, they suggest that the policy change strongly impacted casual patient visits - 54% increase at the nine clinics and 7% increase at the pay clinic. Unsurprisingly, they find that patient/operator ratios were affected by the policy change, having worsened immediately following the change. However, as with the previous studies, it is not possible to extend the analysis to the population, given the small size of the sample.

Walker & Gilson (2004), similar to the secondary analysis in Bayat & Cleaton-Jones (2003), provide a slightly different take on the policy impact by examining externalities, especially nurse perceptions of the quality of health care that was available following the 1996 policy change. Their retrospective survey, conducted with nurses that had been working during the time of the change, provides strong evidence that these health care professionals felt that they were no longer able to fulfill their professional functions, that their patient load increased and that they did not have enough time for consultations. Relatedly, they find that the nurses felt that the implementation process was poorly handled, which fits into a broader set of research related to implementation.[3] Although helpful in describing some of the external effects associated with the policy, the focus on only clinic level externalities is rather limited.

As noted above, a few studies have examined the impact of the user fee policies in South Africa. However, those studies are rather limited, as they focus on very small samples, and generally fail to consider other issues that could impact the observed outcome. In the following analysis, some of those concerns are addressed. The following analysis considers a nationally representative sample of children, focussing mostly on health care provider choice for those children who were either ill or injured. The analysis makes use of regression discontinuity (RD) to infer the policy impact of free public health care, although only for curative care.[4] The impact of the policy is considered for public health care demand, as well as the demand for any health care, which should encompass the direct impacts of the policy. In addition, the analysis is extended to consider potential unintended consequences, such as substitution between the private and public sector, through the examination of private health care demand. Similarly, substitution away from medical aids

[3] A recent issue of *Health Policy and Planning* addresses many of these concerns in more detail.

[4] Unfortunately, the analysis is limited to a sharp RD design, due to the poor performance of an instrument that might have been expected to provide traction within the implementation of a fuzzy design.

for young children is also considered, as are potential effects on health, via an examination of reported illness and injury. Although the results are consistent with a positive policy impact, in the sense that public health care demand is higher, because of free public health care services, that positive impact is estimated to be offset by a reduction in the demand for private health care services; an ill or injured child between the age of five and six is 6-7% more likely to be treated at a public facility, but 5-7% less likely to be treated at a private facility. Although one of the reasons for introducing the policy was to increase the availability of health care for the most vulnerable, such that more children would be treated, the policy is not found to have been effective. As further evidence of the inefficacy of the policy in meeting the previous goal, the results provide evidence that children under the age of six are less likely to be ill or injured. Despite the fact that one might expect more children under the age of six to be treated at a public health facility, and, therefore, be reported as ill or injured, the anslysis, instead, suggests that fewer children are being reported as ill or injured, as a result of the policy. However, this reduction could also be interpreted in a positive light; fewer reported illnesses could have arisen, due to increased preventative care, which cannot be addressed in the analysis.

The remainder of the paper follows a standard structure. Section 2 provides a more detailed examination of research in Africa related to user fee implementation and abolition. The RD methodology is outlined in Section 3. The data for the analysis is described in detail in Section 4, while the empirical results are presented in Section 5. Section 6 concludes.

2. Review of the Literature

The imposition and abolition of health care user fees has been a feature of public health care delivery in Africa for a number of decades. Spurred on by the goal of raising additional funds for health budgets and increasing the efficiency of delivery, a number of countries on the continent imposed user fees for public health services.

However, experience with those fees was not particularly positive.[5] A number of African countries have, since, reversed policy, abolishing their user fee programs, and the literature examining these policies and policy changes has spawned a number of reviews.

Ridde & Morestin (2011) review 20 articles addressing the abolition of user fees in Uganda, Ghana, South Africa, Kenya and Madagascar, noting that the majority of the articles were based on either attendance registers or interviews of key informants, although a few made use of household level information (Mwabu, Mwanzia & Liambila (1995), Deininger & Mpuga (2005), Xu, Evans, Kadama, Nabyonga, Ogwang Ogwai, Nabukhonzo & Mylena Aguilar (2006) and Penfold, Harrison, Bell & Fitzmaurice (2007)). Lagarde & Palmer (2008) provide a different sort of review of the literature. In their review, which included an interrupted time-series analysis, they argue that the published literature leaves much to be desired. With respect to South Africa, they are skeptical of national conclusions based on data from such a small set of clinics or hospitals, and they worry whether or not there were concurrent important changes that were also likely to have affected the impact.[6] Accessing more representative data, while attempting to control potential confounding factors, is crucial to identifying the true impact of the user fee abolition policy.

2.1. **Facilities Level Analysis.** In one of the earliest papers, Mwabu & Wang'ombe (1997) examine Kenya, featuring a cyclical user fee policy: there were no fees, then there were fees, the fees were suspended and then they were reinstated. Their analysis was based on records from four health facilities in a particular disctrict, although the data was collected for a lengthy period of time to cover all policy

[5]In Kenya, user fees were associated with a 27% decrease in utilization at provincial hospitals, a 46% decrease at district hospitals and a 33% decrease at health centres, (Willis & Leighton (1995)). In Zambia, outpatient attendance fell by 35% (Blas & Limbambala (2001)). In Ghana, a 40% decrease in outpatient attendance was observed (Biritwum (1994)).

[6]For example, Benatar (1997) highlights the construction of almost 100 new primary care clinics that were opened by the end of 1996.

changes. Their results point to inelastic health care demand, and very limited effects, estimated via price response non-linearities, associated with changes in the user fee environment.

Nabyonga, Desmet, Karamagi, Kadama, Omaswa & Walker (2005), on the other hand, were able to collect data from six health ditricts in Uganda. In the year following implementation, they find increased usage of about 25% in the public facilities and 44% in the referral centers; in both places, fees were removed. However, in the private-not-for-profit sector, where there were no fee changes, increases were only about 7.9% in that first year. Although it might have been possible to use the different sets of facilties to consider difference-in-difference (DD) estimates of the true impact, that was not done in the analysis. However, a back of the envelope calculation suggests that the net effect ranged from 17% to 36%.

Similar results, in terms of percentage increases, were uncovered by Burnham, Pariyo, Galwango & Wabwire-Mangen (2004), who base their analysis on data from 78 primary health clinics in 10 health districts in Uganda, including attendance registers and health practitioners. They find a large increase in new visits as well as under-5 visits, 53.3% and 27.7%, respectively. For chlid immunizations, the increase was 17.2%; a 25.3% increase in antenatal visits was observed. Much like Walker & Gilson's (2004) South African analysis, Burnham et al. (2004) find some disgruntlement amongst the staff, while management committees perceived reductions in essential drug availability, reduced support for ancilllary clinc staff, and lower staff morale.

In what could be the most representative of clinic-based studies, Masiye, Chitah, Chanda & Simeo (2008) examine utilization, with national data, and quality of care, with a retrospective perceptions survey. Their analysis focusses on Zambia. They find that utilization increased by about 50% in rural districts, which they show is also associated with the deprivation of the districts in question. Similar rural increases in staff workloads are uncovered. Surprisingly, they also find evidence of decreases in urban areas, suggesting disctrict level substitution. However, they find no evidence of changes for under-5s. Although retrospective quality surveys may be suspect, they find little evidence of service quality deterioration, except in drug availability, despite the increased workload.

2.2. **Household Level Analysis.** Another paper is Deininger & Mpuga (2005) who make use of HH level surveys, rather than just district or clinic level data. There are two primary analyses, one at the facility level (panel data looking for breaks) and the other at the household level (suggestive of DD, although the change affected everyone, so the control is not eniterly clear). Their household level focus is on rationing, although they also consider illness, i.e., two potential side benefits of the policy. At the facility-level, increases range from about 18.5% for under fives, to 31% for all. Further, there was a 26% increase in referrals. Simple services for children showed a large increase, 38% increase in child weighing and a 61% for vitamin A supplementation. Also, 12% increase for antenatal care and 34% increase for post-natal care (but these were free before?) For rationing, they find an 8% and 11.5% reduction in rationing for adults and children, respectively, as a marginal effect. For illness, they find a 4.4% decrease in the propensity of children to fall sick, but virtually no effect on adults. Finally, they try to estimate the costs and benefits of the programs, to get at potential savings to the household, findind that there are large benefits net of lost user fees and that there is a pro-poor bias in savings to households.

Xu et al. (2006), in a slight twist, examine utilization and catastrophic expenditures in Uganda. Importantly, they criticize the before and after studies for ignoring other potentially important explanatory variables. They are one of the few to use retrospective data at the household level to control for potential biases. The method applied is a simple difference estimate buried within a Multinomial Logit model (MNL) model, i.e., RD within MNL. They find that catastrophic expenditure was reduced for the nont-poor, but not for the poor. They also find evidnce of increased treatment at all facilities (public, private and other facilities) suggesting - possibly - that the policy improved efficiency in the system or that the analysis failed to pick up on other potential things that happened at the same time (the private effect should not have also been positive?). Unfortunately, they do not provide marginal effects, so it is difficult to get a comparison with previous or even the following analysis.

Ghana eliminated user fees for care associated with child delivery in September of 2003. Penfold et al. (2007) find that the policy decreased traditional birth attendance by approximately 12%, although it is not clear if any other policies were in place in the country to try to improve skilled birth attendance rates. The savings associated with the elimination of delivery fees were considered by Asante, Chikwama, Daniels & Armar-Klemesu (2007). They find that overall costs associated with delivery were reduced by the policy by between 8% and 22%, depending on type of delivery, associated with a 12% reduction in the proportion of households that faced catastrophic health care expenditures, as a result of child birth.

3. Methodology

Although the user fee policy change announced in 1994 had a number of components, the following analysis will focus only on the demand for curative care services for children under the age of six, as the data available does not make it possible to consider preventative care, antenatal care or effects related to nursing mothers. Gupta & Dasgupta (2002), amongst others, note that provider choice decisions are primarily related to curative care, which is the focus of this analysis. As the policy has an age threshold, the analysis will be based on the application of RD.[7] Furthermore, as the age variable in the data is not continuous, the RD analysis will take into account the discrete nature of the running variable.

Due to the reduction in public health care facility user fees, the policy is expected to increase the proportion of young children seeking health care in the public sector. In terms of the data, ill or injured children can either be treated at a public facility, a private facility or not at all. Therefore, the increase in demand at public facilities could be driven by the treatment of children that might not have been treated at all;

[7] In a companion paper, the 1994 and 1996 policy changes are examined within a DD context.

it could also be driven by the treatment of children that would normally have been treated in the private sector. Separating these effects is an important component of the analysis.

However, other impacts, either positive or negative externatlities, could also have arisen from the policy. For example, the policy could have affected medical aid scheme incentives, and, therefore, medical aid scheme coverage. Similarly, the policy may have influenced reported illness. Reduced health care prices could have led to more illnesses and injuries being reported. On the other hand, reduced prices could have led to increases in preventative care, improved health and reduced illnesses and injuries.

For the analysis, we consider a binary indicator of treatment status, D, which is determined by an age threshold, where age is denoted by a, and the threshold, six in this analysis, is denoted by a_0. Therefore, $D = 1[a < a_0]$. Denoting the potential outcome of the child, if the child is eligible for free public health care, as Y_1, while Y_0 is the potential outcome if the child does not qualify for free health care. Defining $\tau \equiv E[Y_1 - Y_0 | a = a_0]$, as the average treatment effect. The goal of an RD analysis is to estimate τ, which is the effect of free health care at the age cut-off. However, it is not possible to observe both potential outcomes for each child. Instead, we observe $Y = DY_1 + (1 - D)Y_0$.

Assuming that the endogeneity is continuous across the age threshold, as in (1), the treatment effect can be uncovered.

$$\lim_{\Delta \uparrow 0} E[D_i \nu_{ji} | X_i, a_i = a_0 + \Delta] - \lim_{\Delta \downarrow 0} E[D_i \nu_{ji} | X_i, a_i = a_0 + \Delta] = 0 \tag{1}$$

The assumption outlined in (1) assumes the endogeneity can be differenced out of the regression, such that the treatment effect is identified as the difference between outcomes for the treatment and control groups, as in (2).

$$\tau_j = \lim_{\Delta \uparrow 0} E[Y_{ij} | X_i, a_i = a_0 + \Delta] - \lim_{\Delta \downarrow 0} E[Y_{ij} | X_i, a_i = a_0 + \Delta] \tag{2}$$

The majority of RD examples, such as Hahn, Todd & van der Klaauw (2001), Lee (2008) and Card, Dobkin & Maestas (2008) use data that is nearly continuous with respect to the running variable. However, in this design, as with the design considered by Duflo (2003), the running variable, age, is discrete. Therefore, we apply methods discussed in Lee & Lemieux (2010). Specifically, we allow for cluster effects, based on age, in the analysis. We also consider a variety of different bandwidths in the analysis, although within the discrete running variable context.

In terms of the demand for curative health care, there are, essentially, three mutually exclusive binary care options: no care, private care and public care. Although the analysis could be conducted within a multinomial setting, using, for example, multinomial logit or probit, separate OLS regressions are, instead used, i.e., the analysis is based on separate linear probability models.[8]

The regression model considered, therefore, is the following.

$$Y_{ij} = \tau_j D_i + g\left(\tilde{a}_i\right) + D_i h\left(\tilde{a}_i\right) + X_i \beta_j + \nu_{ij} \tag{3}$$

In (3), Y_{ij} is the outcome j of interest for child i, $\tilde{a}_i = a_i - 5$ is the age of the child net of the threshold,[9] D_i is the treatment indicator, X_i is a set of control variables and ν_{ij} is a heteroskedastic error term that is assumed to be clustered by age. The treatment effect to be estimated is τ_j. Due to the dependence of the policy on age, polynomial functions of age, represented by g and h are included in the regression. This regression is applied to a series of subsamples of differing age bandwidths, providing an indication of the consistency of the estimate across different groups, discussed more fully, below. The bandwidths include differing numbers of age groups within the regression, and, therefore, the degree of the polymomials represented by g and h (first, second or third) differ.[10]

[8]The previous literature related to provider choice in South Africa has, for the most part, made use of multinomial regression. However, that research focuses on determinants, rather than causal effects. See, for example, Haveman & van der Berg (2003), Burger & Grobler (2007) and Grobler & Stuart (2007).

[9]As the running variable is discrete, the smallest age below the threshold is 5.

[10]For example, in a three-year bandwidth subsample, there are only three different ages above and below the threshold. Although it would be possible to include a quadratic function over three

4. The Data

4.1. **Data Source.** Data for the analysis was sourced from the South African October Household Survey (OHS) of 1995.[11] The main purpose of the OHS, Statistics South Africa (1995), was to collect information on households and individuals across the nine provinces of South Africa, and the survey included questions related to dwellings and dwelling services, perceived quality of life, socio-demographic information, employment and unemployment, the informal and formal labour markets, as well as births and deaths in the household. Along with this information, there is a short series of questions related to illness, injury, healthcare-seeking behaviour and access to medical aids. The survey included responses from 121 538 individuals living in 29 700 households. The survey follows a stratified random sampling method, being explicitly stratified by province, magisterial district, urban or rural locale and population group. These enumeration areas were selected systematically based on probabilities proportional to their size, where the size was estimated from the 1991 population census. Within a selected enumeration area, ten households were drawn for interview. Post-stratified weights are available, but are not used in the analysis, due to the fact that the analysis sample is limited to all children aged 11 and under that have been reported ill or injured in the last 30 days. The weights are not calibrated for a subsample of this nature, and, therefore, the weights are not likely to lead to true population estimates.

A series of different sections in the survey cover a variety of different topics; however, it is possible to merge the relevant information to create data at the child level. For this analysis, data for each child was taken from the individual questionnaire, including information on the mother and father, if they are members of the household. Information related to the child's mother and/or father was merged into the child dataset, as was data related to the household.

age points, that model would be overfitted, and, therefore, in the three-year subsample, only first degree polynomials in age are included.

[11]Although data from both 1994 and 1996 is also available, 1994 was deemed too soon after policy implementation, while 1996 was assumed to be confounded by a further change in the policy that extended free primary care within the public sector to all.

For the analysis, six outcome variables are considered: (1) whether care for the ill or injured child was sought in a public facility or (2) private facility or (3) either a public or private facility, (4) whether the child had access to a medical aid (health insurance), and (5) whether the child was reported as ill or injured.[12]

In addition to the outcome variables, we create dummy variables for the sex of the child and the population group of the child, and include the age of the child, which serves as the running variable in the analysis. Household level controls include the size of the household and the number of rooms per capita in the dwelling. We also include dummy variables for household services, such as in-house tap water and flush toilets, as well as dummies for urban and provincial locales. Finally, dummy variables are included to represent both the time and the distance the dwelling sits from the health facility usually attended, if a household member seeks medical care. For mothers and fathers, we capture their education, whether or not they have a medical aid, whether or not they are employed or unemployed, their monthly earnings, whether or not they refused to provide earnings as a specific amount and whether or not they are part of the household;[13] in the last case, no information on earnings, education, employment or medical aid access is available.

4.2. **Data Summary.** A summary of the data is provided in Appendix A. In Table A.1, the variable, its definition, as well as its mean and standard error in each of two subsamples is presented. Tests of whether or not those means differ by subsample are also listed in the table. Importantly, as a first pass, the differences in means provide some prelimary evidence with respect to the impact of the free primary health care policy. Approximately 9% more children under the age of 6 are treated

[12]Whether or not payment was made for the delivery of health care, if it was sought, was also examined. Unfortunately, the payment variable is based on a question that is not specific enough. In other words, it is not known if a payment was made for the visit, for any transportation related to the visit, for drugs that might have been subscribed or for any other reason. For that reason, it is not possible to use this variable as an instrument for enforcement of the policy, and, therefore, this analysis is relegated to Appendix B.

[13]In the survey, a few hundred of the parents do not report an actual earnings figure. Instead, they offer a range of earnings, as well as whether that range is weekly, monthly or yearly. However, the ranges provided are not completely appropriate for creating estimates of their monthly earnings. Although a wide range of possibilities exist for creating such estimates, no attempt has been made to do so. Furthermore, controlling for interval reporting appears to be appropriate in the analysis.

in public facilities.[14] Furthermore, younger children are approximately 8% more likely to be treated at any facility, although there is no statistical significance in the difference between the probability of treatment in a private facility. Relatedly, older children are approximately 1.5% more likely to have access to a medical aid, while younger children are 3% more likely to be reported as ill or injured.

These tests also provide some evidence related to the similarity of the two subsamples. In a completely random experiment, there should be no difference between the two groups. Although that is true for many of the variables in the analysis, it is not true for all, allowing us to conclude that the samples are not completely balanced, and, therefore, many of the controls should be included in the RD analysis. For example, there is a slightly larger proportion of mixed-race children in the older age group, that is offset by a slightly smaller proportion of black-African children in that group. Furthermore, more of the older children are found in the Western Cape and the Northern Province, but fewer are from the Free State.

Finally, there are a number of differences with respect to maternal and parental controls, which is not that surprising given the nature of the labour market in the country, as well as other realities associated with raising children. We find that younger children's mothers are more educated, more likely to be unemployed, less likely to be employed, less likely to have access to a medical aid, less likely to have recorded their earnings in an interval and less likely to live away from the home. On the other hand, we find that younger children's fathers are more likely to live away from home, less likely to have a medical aid and less likely to report their earnings in an interval. One might also worry that medical aid access is affected by the policy. If there was a discontinuity associated with medical aid access, that would suggest that the inclusion of medical aid access in the analysis would not be appropriate. Fortunately, as can be seen in Figure 4 and the discussion in Section 5.2.2, that is not a concern.

[14]Oolder children are approximately 10% more likely to have their care require payment, although that is not separated by public and private facility usage.

5. Results

The results from the analysis are presented in Tables 1 - 5, and each of those tables are presented in the same fashion. Due to the fact that the running variable is not continuous, the analysis is undertaken using different discrete bandwidths, and the standard errors are further clustered via age. In the top panel of each table, six-year bandwidths are presented; in other words, children aged through five years are compared against children aged from six years through eleven years. In the second panel, five-year bandwidths are used, instead. For this panel, children aged from one to five are compared against children aged six through eleven. In these two analyses, there are enough ages to include a cubic polynomial in age above and below the cutoff. In the third panel, four-year bandwidths are considered, comparing children that are between two and five with children that are between six and ten. Under that, there is only enough variation in the running variable to include a quadratic in age (above and below the threshold) in the regression. In the fourth panel, three-year bandwidths are used, allowing only for a linear function in age above. Finally, in the bottom panel, a one year bandwidth is analyzed; five-year olds are compared to six-year olds. Given either a one-year bandwidth or a two-year bandwidth and a discrete running variable, it is not possible to include age effects in the regression; therefore, only the one-year bandwidth results are presented.[15]

5.1. **Policy Impact: Direct Effects.**

5.1.1. *Public Health Care.* As the primary purpose of the analysis is to estimate the impact of user fee abolition on the use of public health facilities by children under the age of six, the first analysis considered is an esimate of (3) on healthcare-seeking at public facilities. Before considering the regression results, an illustration of the means and a quadratic fit in the running variable is provided; see Figure 1. As can be seen in the illustration, children under the age of six are more likely to use public facilities. We turn to the regression results, below.

[15]With two age values on either side of the cutoff, a linear age effect would be perfectly estimated, and, therefore, overfit.

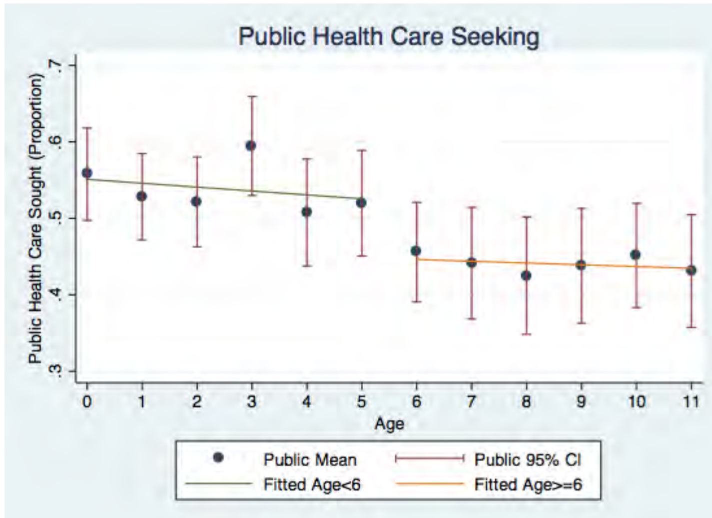

FIGURE 1. Treatment at Public Facilities

The results of the analysis are presented in in Table 1. With the exception of one estimate, the results suggest a positive policy impact; however, those policy impacts are imprecisely estimated, at least for bandwidths in excess of three years. For the most part, the additional controls do not alter the estimation within any of the subsamples, with the exception of the six-year bandwidth subsample, suggesting that the assumption in (1) is quite reasonable for the analysis of public sector provider choice. Furthermore, with the exception of the five-year bandwidth subsample, the estimates are generally increasing and more precise as the subsample bandwidth decreases, the latter of which suggests that the RD estimator subsumed in (2) is more accurate as the comparisons are made closer to the threshold.

With respect to the principles of RD, where the impact is estimated at the limit of the age cutoff, the preferred estimate is the one-year bandwidth estimate directly comparing children closest to the threshold, suggesting that the policy led to a 6.4% to 7.3% increase in the probability that a sick or injured child would receive health care services from the public sector. Although increasing the bandwidth affected the estimates, the results support the economic argument that lower prices yield increased demand. The change in the estimates across bandwidths does suggest that, even though age effects were included in the analysis, the inclusion of a polynomial in age did not completely address the relationship between the outcome variable and age.[16]

5.1.2. *Any Health Care.* In addition to examining the impact of policy on public health facility usage, a further examination was undertaken to examine the impact of the policy on treatment overall. For this analysis, the outcome variable is a dummy measuring whether or not the ill or injured child received any treatment, whether at a public or a private health facility. An illustration of the mean proportion treated at any facility, along with a quadratic function in age is provided in Figure 2, which shows that the difference in treatment at the threshold was, at most, minor.

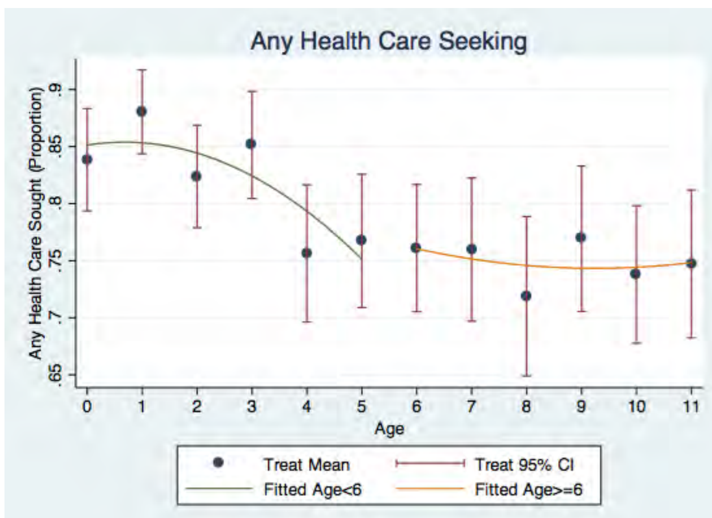

FIGURE 2. Treatment at any Facility

The empirical results for the analysis are available in Table 2. Importantly, the regression results are not as consistent across subsamples as was the case in the public healthcare-seeking analysis, which is not necessarily suprising, given Figure 2. The policy either decreased the probability that any child under the age of six

[16]If date of birth were available in the data, the relationships could have been investigated more formally. Unfortunately, the data that is available provides age in years at the time of the interview; in other words, it is not possible to undertake a more general nonparametric analysis of the relationship between the running variable and the outcome variable.

sought health care at any facility by up to 6.8% for the five-year bandwidth using only age controls or increased the probability up to 1.2% in the six-year bandwidth where all controls are included; however all of the estimates in these two subsamples are imprecise. For the most part, this second analysis suggests that, at best, the observed increase in public facility usage, discussed above, was at the expense of private facilities, which we investigate further, below. At the worst, the policy led to a decrease in health care seeking for those who were covered. In terms of the RD assumptions, the regression results in the last panel of Table 2 are the most appropriate, and these suggest small, and mostly insignificant policy effects.

5.2. **Policy Impact: Externalities.** In an attempt to further investigate the policy impact, an additional series of regressions were considered. In the first, we examine the effect of the policy on private health care use. In the second, we consider the effect on medical aid access for children, and in the third, we examine whether or not illnesses are more or less likely to be reported.

5.2.1. *Private Health Care.* In first of the externality analyses, private health care use, rather than public health care or any treatment, is the outcome variable of interest. Given that the policy does not provide free primary care to children in private facilities, one would expect no effect of the policy on the use of private facilities. However, one might also expect to see private health care use fall, if there is some substitution between the public and private sectors, as implied by discussion related to any health care. Substitution could be driven by the belief that it makes sense to take children to public facilities, where health care is free, rather than pay for it at private facilities. An illustration of that possibility is provided in Figure 3, which clearly shows that a discontinuity at the threshold is visible, with fewer treated children using private facilities than their control counterparts. Since there was no policy impact on the use of treatment in general, the decrease in the proportion of sick and injured children using private facilities implies that the estimated increase in the priobability of public facility was driven by substitution between private and public facilities.

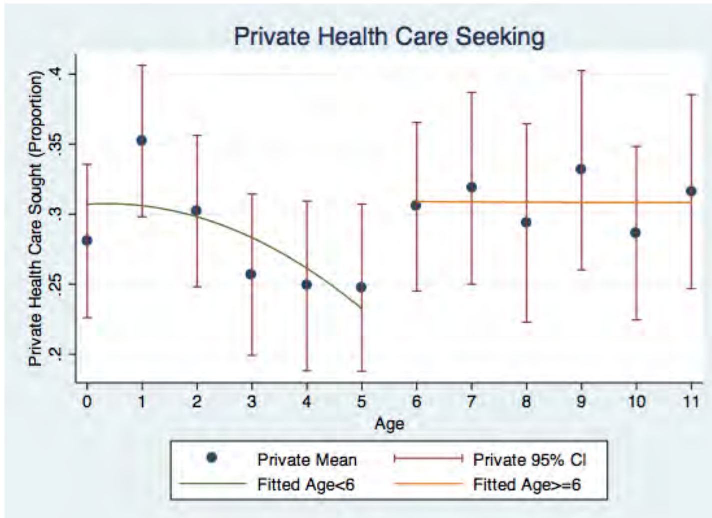

Figure 3. Treatment at Private Facilities

Empirically, the implications from Figure 3 are generally supported. The results from the analysis are available in Table 3. As expected, given the preceding discussion, young children with access to free public health care are much less likely to be taken to private healthcare facilities, and the effect is rather large, ranging from an imprecise 1.9% for the six-year bandwidth and only age controls to a significan 9.6% for the five-year bandwidth and only age controls. Within subsamples, the estimates are quite similar, regardless of which controls are included. In other words, the RD assumption of constant endogeneity across the threshold appears to hold. Furthermore, the results are rather consistent across the subsamples, suggesting that the limit in (2), at least in the case of private health care use, is nearly independent of the size of the subsample, as measured by Δ. With respect to the specific assumptions of RD, once again, the final panel of the table presents the preferred estimates: a sick or injured child was between 5.8% and 7.2% less likely to seek treatment at a private facility, because health care was freely available in the public sector. Economically, it appears that free public health care led to substitution away from the private sector. In other words, the increases associated with public health care use have been offset by reductions in the use of private health care facilities.

5.2.2. *Medical Aid Access.* Although the policy has had a positive impact on the use of public facilities, the analysis was able to identify at least one externality that arose: private health care use was estimated to have fallen, such that the impact on overall health care use was estimated to be rather small or even negative. In addition to this possible externality, other externalities could have arisen. Since private health care use was shown to decrease, and public health care is free, it might also be the case that parents of young children could have decided that they could save money by not insuring their young children. Similarly, medical aid providers could have decided to offer fewer health care benefits for children in an effort to keep their costs down. If either of those two possibilities were true, one would expect a discontinuity at the age cutoff to be associated with medical aid access. We investigate this possibility by examining the relationship between the policy variable and children's medical aid coverage for all children under the age of 12. Figure 4 illustrates the analysis. Since the data in Figure 4, do not suggest a discontinuity, the policy did not appear to have an effect on Medical Aid access.

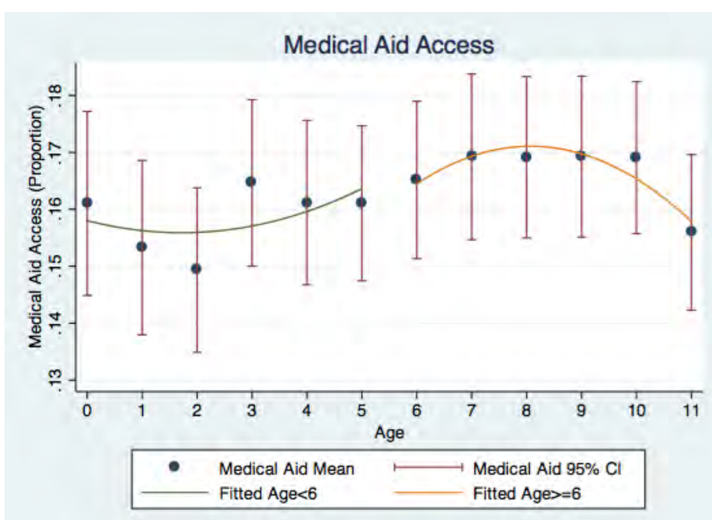

FIGURE 4. Medical Aid Access

The empirical results, reported in Table 4, confirm that medical aid access amongst children under the age of six was not affected by the policy; the estimates range from -1.9% for the six-year bandwith including age, individual and household controls to 0.9% for the five-year bandwidth including age and individual controls. Given that medical aid access is likely to be driven by the employment status of parents, as well as their own medical aid access, controlling for parental employment and medical aid access, as is done in the final two columns of the table is expected to provide the best estimates of the policy effect. Furthermore, taking into account the RD assumptions, which is most appropriate in the final panel of Table 4, suggests that the policy reduced medical aid coverage for young children by, at most 0.7%; however, after including maternal and paternal controls, that reduction falls to 0.1% or even zero.

5.2.3. *Reported Illness.* It is possible that other external benefits or costs accrued to young children. For example, free public health care could have led to more reported illness or affected the overall health of children, as a result of increased access to free public health care, such as preventative care. In order to examine this possibility, all children under the age of 12 were included in an analysis, wherein the outcome variable is reported illness or injury. The following illustration, Figure 5 suggests some success in this regard, as fewer young children below, but near, the threshold are reported ill or injured than children above, but near, the threshold.

Empirically, the results implied by Figure 5 are borne out; see Table 5. According to the analysis, a consistent reduction in reported illness and injury is uncovered, and that reduction ranges from 0.9% for the one-year, and, therefore, preferred bandwidth, to 4.0% for the six-year bandwidth. The results are consistent within subsamples, inferring that the RD assumption of constant endogeneity across the threshold is reasonable. Furthermore, the results are quire similar across subsamples. Similar to the analysis for private health care use, the similarity across and within subsamples, suggests that the policy impact is consistently estimated.

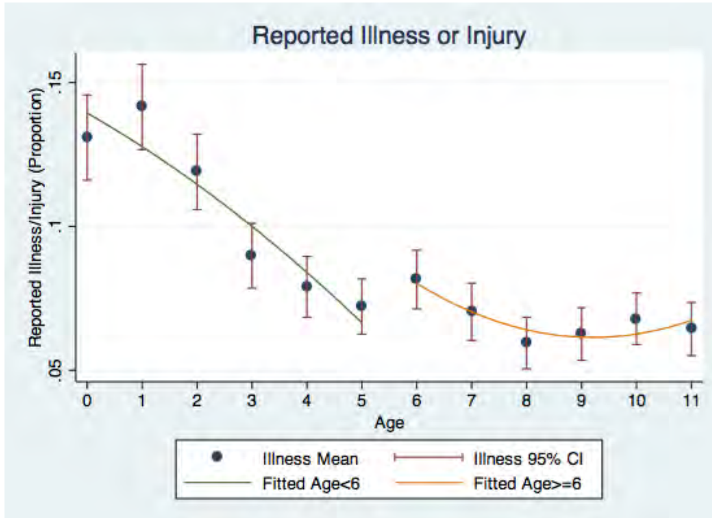

FIGURE 5. Reported Illness or Injury

5.3. **Discussion.** In the preceding analyses, a series of potential impacts related to the availability of free primary care have been considered, based on a sharp RD design. The results point to a significant increase in the probability that an ill or injured child will receive health care at a public facility, and that increase is in the range of 7%; however, there is strong evidence that this increase was offset by a nearly equal decrease in the probability that a sick or injured child would receive health care treatment at a private health care facility. When combined, these two effects are negated, such that the proportion of ill or injred children that receive any health care has not been affected by the policy. Additional analysis is consistent with a a small reduction in reported illness or injury amongst the same children, even though it might have been expected that far more children could have been be reported as ill or injured, if they could be seen for free, but a near zero reduction or increase in medical aid coverage amongst those eligible for free public health care, as well.

The results from this analysis are in sharp contrast to much of the literature that estimates the impacts of user fee reduction policies. With respect to South Africa, the few papers that are available, increases in public health care use are found to be in the range of 50%, whereas our results point to increases of only about 7%. However, since much of the research, including that in South Africa, considers the effect of user fee abolition at the clinic level - see the reviews by Lagarde & Palmer (2008) and Ridde & Morestin (2011) - and do not control for the unintended consequences of the policy, it is not surprsing that the results are different. Furthermore, this analysis considers healthcare-seeking behaviour amongst ill and injured children, which is only a small component of the 1994 user fee abolition policy in South Africa. Specifically, our analysis cannot directly consider preventative healthcare activities, although we do find evidence that is consistent with the policy having a positive impact on the overall health of young children, which could be due to an increase in preventative healthcare for, especially, young children.

Although the analysis is based on a sharp RD design, which is not likely to be perfect, there are no instruments in the data to control for whether or not the policy was enforced, as discussed in Appendix B. Therefore, another question to consider is whether or not the policy is likely to be overstated or understated, as a result of the simplifying assumptions implied by the RD approach. In the case of private health care use, medical aid coverage and reported illness, the results were consistent across subsamples and included regressors, suggesting that the sharp RD assumptions are reasonable for these analyses. However, the results for the other analyses, public health care use and any treatment, were not as consistent across subsamples, although they were generally consistent within subsamples. Realistically, one would be willing to accept that some six-year olds and some seven-year olds were likely to have received free health care in the public sector, as identity documents were not always available for all South Africans at that time, and it might be rather difficult to determine a child's age just by looking at the child. Under those circumstances, a number of six-year olds and seven-year olds could have received free public health care, although they would be recorded in the data as six and seven-year olds, yielding underestimates of the user fee abolition policy impact on public health care seeking behaviour. Despite the likely understated estimates, it is unlikely that correcting for the fuzziness of the design would yield estimates anywhere close to those reported by McCoy & Khosa (1996), Wilkinson et al. (1997), Wilkinson et al. (2001) or Bayat & Cleaton-Jones (2003).[17]

The difference between our results and those reported in the literature bring into question the analsysis of James, Morris, Keith & Taylor (2005). Their epidemiological study examining the potential life-saving effects of user charge abolition, was underpinned by research reporting large increases in health care demand. According to their simulation analysis, between 153 000 and 305 000 children under the age of 5 could be saved through user fee abolition across a number of countries in Africa. Given the 7% increase in public health demand estimated in our analysis, which is $1/7^{th}$ of the 50% estimate used, at least in the case of South Africa, it might be the case that only 22 000 to 44 000 children could be saved. On the other hand, if public health care use replaces private health care use, the improvement in health is likely to be much smaller. For example, our results suggest that there is approximately a 1% to 4% reduction in reported illness amongst the children eligible for free public health care in South Africa, $1/50^{th}$ to $1/12^{th}$ the effect implied by the South African analysis. In that case, only 3 000 to 25 000 children would be saved by user fee abolition. Although not a small number, it is far less than James et al.'s (2005) research suggests.

6. Conclusion

Economic inuition suggests that the abolition of public health care user fees should lead to increased demand for public health care. Most of the research related to user fee policies in Africa supports that intuition, including our analysis. However, economic behaviour is slightly more complex than that simple intuition suggests. In the case of health care demand for children, households face a choice of seeking care in public facilities, private facilities or not at all. Due to the abolition of user fees, public health care has become relatively cheaper, implying demand substitution: it is to be expected that some children will be more likely to receive

[17]Unfortunately, that cannot be determined. See Appendix B.

their health care advice from public health care service providers rather than private service providers. The literature examining user fee abolition policies has not considered the aforementioned general equilibrium effect, focussing, instead, on the increase in utilization at the public clinic level. In the preceding analysis, we are able to consider the policy impacts of free primary health care within a general equilibrium framework, and we find evidence in support of substitution.

In addition to the previous studies' limitations with regards to general equilibrium, their small sample clinic focus, with the exception of Masiye et al. (2008), provide results are that are unlikely to be representative of the country, as a whole. In our analysis, we are able to examine household decisions for South African children, and that data was taken from a representative sample of households. As such, our analysis provides better information with respect to the impact of the 1994 South African user fee abolition policy at a national level.

Finally, household level analysis of user fee policies generally do not control for potential endogeneities associated with healthcare provider choice decisions.[18] Similarly, South African health care provider choice analyses focus on determinants of choice, rather than causal effects on choice. In this analysis, we make use of regression discontinuity to control for potential endogeneities, such as household structure, that might influence the causal effects estimates of free public health care on a number of potential health related decisions, such as provider choice, medical aid access and reported illness.[19]

Although this analysis provides a number of insights into the effect of free primary health care on households, there are a number of additional questions remaining. In particular, the policy may have affected household welfare, and, in fact, part of the reason for the abolition of user fees was to improve household

[18]For example, Duflo (2000), Duflo (2003), Bertrand, Mullainathan & Miller (2003) and Jensen (2004) use RD to consider the effect of pension income on labour market outcomes, child health and remittance substitution, the analysis has been criticized for not accounting for the endogeneity of household formation, Hamoudi & Thomas (2005).

[19]Although not discussed or reported in the analysis, we are also able to control for the endogeneity of medical aid access within the provider choice decision. Our results suggest that medical aid coverage for children under the age of 12 raises the probability of private care by 15%, but has an insignificant affect on the demand public health services.

welfare. Therefore, considering whether or not the policy was pro-poor deserves attention. Furthermore, the policy was enacted in 1994, nearly 20 years ago. Although small policy impacts were identified in this analysis, it is plausible, that the primary benefit of the policy was long-term, rather than immediate. In that regard, considering the effect of the policy on the health of the population, or, possibly, education completion, would shed light on the broader benefits of user fee abolition, providing a better picture than could be simulated by either James et al. (2005) or James, Hanson, McPake, Balabanova, Gwatkin, Hopwood & et al. (2006).

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Table 1. Policy Impact: Public Health Care Use

VARIABLES	Base Controls	+Individual Controls	+Household Controls	+Maternal Controls	+Paternal Controls
Six-year bandwidth. Control Group (Age < 6), Treatment Group (6 ≤ Age ≤ 11)					
policy^e	-0.0103	0.0143	0.0249	0.0274	0.0409
	(0.022)	(0.032)	(0.044)	(0.051)	(0.050)
Observations	2,574	2,574	2,574	2,574	2,574
R^2	0.009	0.088	0.115	0.120	0.124
Five-year bandwidth. Control Group (1 ≤ Age < 6), Treatment Group (6 ≤ Age < 11)					
policy^e	0.0283^d	0.0132	0.0381	0.0456	0.0563
	(0.016)	(0.051)	(0.064)	(0.072)	(0.077)
Observations	2,140	2,140	2,140	2,140	2,140
R^2	0.008	0.089	0.114	0.118	0.124
Four-year bandwidth. Control Group (2 ≤ Age < 6), Treatment Group (6 ≤ Age < 10)					
policy^f	0.0161	0.0230	0.0320	0.0301	0.0378
	(0.018)	(0.033)	(0.036)	(0.040)	(0.040)
Observations	1,633	1,633	1,633	1,633	1,633
R^2	0.010	0.092	0.112	0.119	0.125
Three-year bandwidth. Control Group (3 ≤ Age < 6), Treatment Group (6 ≤ Age < 9)					
policy^g	0.0320	0.0389	0.0452^d	0.0456^d	0.0508^c
	(0.020)	(0.027)	(0.026)	(0.026)	(0.021)
Observations	1,186	1,186	1,186	1,186	1,186
R^2	0.012	0.093	0.114	0.126	0.131
One-year bandwidth. Control Group (Age = 5), Treatment Group (Age = 6)					
policy^h	0.0640^a	0.0731^a	0.0693^b	0.0708^b	0.0668^a
	(0.000)	(0.001)	(0.003)	(0.004)	(0.001)
Observations	428	428	428	428	428
R^2	0.004	0.057	0.090	0.101	0.127

Standard errors, clustered by age, in parentheses: ^{a} p<0.01, ^{b} p<0.05, ^{c} p<0.1, ^{d} p<0.15
Base Controls: policy, ^{e} cubic polynomial in age above and below threshold.
Base Controls (continued): ^{f} quadratic polynomial in age above and below threshold.
Base Controls (continued): ^{g} linear in age above and below threshold.
Base Controls (continued): ^{h} no age controls. Individual: race, gender, medical aid.
Household: province, urban, hh size, hh services, distance and time to medical care.
Maternal/Paternal: education, parent at home, medical aid access, income, un/employment.

Table 2. Policy Impact: Any Healthcare Use

VARIABLES	Base Controls	+Individual Controls	+Household Controls	+Maternal Controls	+Paternal Controls
Six-year bandwidth. Control Group (Age < 6), Treatment Group (6 ≤ Age ≤ 11)					
policy[e]	-0.0301	-0.0342	-0.0064	-0.0011	0.0124
	(0.040)	(0.036)	(0.043)	(0.045)	(0.042)
Observations	2,574	2,574	2,574	2,574	2,574
R^2	0.014	0.020	0.046	0.058	0.064
Five-year bandwidth. Control Group (1 ≤ Age < 6), Treatment Group (6 ≤ Age < 11)					
policy[e]	-0.0680	-0.0649	-0.0440	-0.0407	-0.0339
	(0.067)	(0.065)	(0.080)	(0.086)	(0.084)
Observations	2,140	2,140	2,140	2,140	2,140
R^2	0.014	0.023	0.051	0.062	0.069
Four-year bandwidth. Control Group (2 ≤ Age < 6), Treatment Group (6 ≤ Age < 10)					
policy[f]	-0.0596[d]	-0.0580[d]	-0.0353	-0.0350	-0.0258
	(0.037)	(0.035)	(0.039)	(0.041)	(0.034)
Observations	1,633	1,633	1,633	1,633	1,633
R^2	0.007	0.018	0.053	0.064	0.073
Three-year bandwidth. Control Group (3 ≤ Age < 6), Treatment Group (6 ≤ Age < 9)					
policy[g]	-0.0367	-0.0383[d]	-0.0217	-0.0184	-0.0127
	(0.023)	(0.021)	(0.021)	(0.021)	(0.013)
Observations	1,186	1,186	1,186	1,186	1,186
R^2	0.007	0.021	0.064	0.077	0.087
One-year bandwidth. Control Group (Age = 5), Treatment Group (Age = 6)					
policy[h]	0.0063[a]	0.0015	0.0076	0.0066[b]	0.0002
	(0.000)	(0.007)	(0.004)	(0.000)	(0.002)
Observations	428	428	428	428	428
R^2	0.000	0.025	0.080	0.094	0.117

Standard errors, clustered by age, in parentheses: [a] p<0.01, [b] p<0.05, [c] p<0.1, [d] p<0.15
Base Controls: policy, [e] cubic polynomial in age above and below threshold.
Base Controls (continued): [f] quadratic polynomial in age above and below threshold.
Base Controls (continued): [g] linear in age above and below threshold.
Base Controls (continued): [h] no age controls. Individual: race, gender, medical aid.
Household: province, urban, hh size, hh services, distance and time to medical care.
Maternal/Paternal: education, parent at home, medical aid access, income, un/employment.

Table 3. Policy Externality I: Private Health Care Use

VARIABLES	Base Controls	+Individual Controls	+Household Controls	+Maternal Controls	+Paternal Controls
Six-year bandwidth. Control Group (Age < 6), Treatment Group (6 ≤ Age ≤ 11)					
policy[e]	-0.0197	-0.0485[a]	-0.0313[c]	-0.0285	-0.0285
	(0.033)	(0.015)	(0.017)	(0.021)	(0.026)
Observations	2,574	2,574	2,574	2,574	2,574
R^2	0.004	0.140	0.170	0.177	0.181
Five-year bandwidth. Control Group (1 ≤ Age < 6), Treatment Group (6 ≤ Age < 11)					
policy[e]	-0.0963[d]	-0.0780[a]	-0.0820[a]	-0.0863[a]	-0.0903[a]
	(0.054)	(0.018)	(0.022)	(0.025)	(0.020)
Observations	2,140	2,140	2,140	2,140	2,140
R^2	0.005	0.155	0.183	0.192	0.196
Four-year bandwidth. Control Group (2 ≤ Age < 6), Treatment Group (6 ≤ Age < 10)					
policy[f]	-0.0757[b]	-0.0811[a]	-0.0673[a]	-0.0651[a]	-0.0636[a]
	(0.026)	(0.011)	(0.014)	(0.015)	(0.011)
Observations	1,633	1,633	1,633	1,633	1,633
R^2	0.004	0.160	0.190	0.199	0.203
Three-year bandwidth. Control Group (3 ≤ Age < 6), Treatment Group (6 ≤ Age < 9)					
policy[g]	-0.0687[a]	-0.0772[a]	-0.0669[a]	-0.0641[a]	-0.0636[a]
	(0.010)	(0.008)	(0.011)	(0.012)	(0.011)
Observations	1,186	1,186	1,186	1,186	1,186
R^2	0.004	0.163	0.196	0.211	0.213
One-year bandwidth. Control Group (Age = 5), Treatment Group (Age = 6)					
policy[h]	-0.0578[a]	-0.0716[c]	-0.0616[a]	-0.0642[b]	-0.0666[b]
	(0.000)	(0.008)	(0.001)	(0.004)	(0.001)
Observations	428	428	428	428	428
R^2	0.004	0.165	0.206	0.215	0.234

Standard errors, clustered by age, in parentheses: [a] p<0.01, [b] p<0.05, [c] p<0.1, [d] p<0.15
Base Controls: policy, [e] cubic polynomial in age above and below threshold.
Base Controls (continued): [f] quadratic polynomial in age above and below threshold.
Base Controls (continued): [g] linear in age above and below threshold.
Base Controls (continued): [h] no age controls. Individual: race, gender, medical aid.
Household: province, urban, hh size, hh services, distance and time to medical care.
Maternal/Paternal: education, parent at home, medical aid access, income, un/employment.

Table 4. Policy Externality II: Medical Aid Access

VARIABLES	Base Controls	+Individual Controls	+Household Controls	+Maternal Controls	+Paternal Controls
Six-year bandwidth. Control Group (Age < 6), Treatment Group (6 ≤ Age ≤ 11)					
policy[e]	-0.0071[d]	-0.0152[d]	-0.0187[b]	-0.0043	-0.0012
	(0.004)	(0.010)	(0.007)	(0.005)	(0.004)
Observations	30,667	30,667	30,667	30,667	30,667
R^2	0.000	0.261	0.337	0.799	0.816
Five-year bandwidth. Control Group (1 ≤ Age < 6), Treatment Group (6 ≤ Age < 11)					
policy[e]	0.0038[b]	0.0088[b]	-0.0002	0.0063	0.0056
	(0.001)	(0.004)	(0.001)	(0.007)	(0.005)
Observations	25,974	25,974	25,974	25,974	25,974
R^2	0.000	0.258	0.336	0.798	0.815
Four-year bandwidth. Control Group (2 ≤ Age < 6), Treatment Group (6 ≤ Age < 10)					
policy[f]	-0.0001	-0.0030	-0.0073[a]	0.0025	0.0032
	(0.002)	(0.004)	(0.001)	(0.003)	(0.003)
Observations	20,813	20,813	20,813	20,813	20,813
R^2	0.000	0.255	0.334	0.795	0.813
Three-year bandwidth. Control Group (3 ≤ Age < 6), Treatment Group (6 ≤ Age < 9)					
policy[g]	-0.0032[c]	-0.0057[c]	-0.0090[a]	0.0014	0.0024
	(0.001)	(0.002)	(0.001)	(0.003)	(0.002)
Observations	15,776	15,776	15,776	15,776	15,776
R^2	0.000	0.253	0.334	0.796	0.814
One-year bandwidth. Control Group (Age = 5), Treatment Group (Age = 6)					
policy[h]	-0.0041[a]	-0.0035[b]	-0.0070[b]	-0.0010[c]	0.0003[b]
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
Observations	5,573	5,573	5,573	5,573	5,573
R^2	0.000	0.257	0.326	0.794	0.811

Standard errors, clustered by age, in parentheses: [a] p<0.01, [b] p<0.05, [c] p<0.1, [d] p<0.15

Base Controls: policy, [e] cubic polynomial in age above and below threshold.

Base Controls (continued): [f] quadratic polynomial in age above and below threshold.

Base Controls (continued): [g] linear in age above and below threshold.

Base Controls (continued): [h] no age controls. Individual: race, gender, medical aid.

Household: province, urban, hh size, hh services, distance and time to medical care.

Maternal/Paternal: education, parent at home, medical aid access, income, un/employment.

Table 5. Policy Externality III: Reported Illness or Injury

VARIABLES	Base Controls	+Individual Controls	+Household Controls	+Maternal Controls	+Paternal Controls
Six-year bandwidth. Control Group (Age < 6), Treatment Group (6 ≤ Age ≤ 11)					
policy[e]	-0.0398[a]	-0.0390[a]	-0.0398[a]	-0.0401[a]	-0.0404[a]
	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)
Observations	30,667	30,667	30,667	30,667	30,667
R^2	0.008	0.018	0.023	0.024	0.024
Five-year bandwidth. Control Group (1 ≤ Age < 6), Treatment Group (6 ≤ Age < 11)					
policy[e]	-0.0274[a]	-0.0268[a]	-0.0285[a]	-0.0293[a]	-0.0302[a]
	(0.006)	(0.006)	(0.005)	(0.005)	(0.006)
Observations	25,974	25,974	25,974	25,974	25,974
R^2	0.007	0.017	0.023	0.024	0.024
Four-year bandwidth. Control Group (2 ≤ Age < 6), Treatment Group (6 ≤ Age < 10)					
policy[f]	-0.0305[a]	-0.0300[a]	-0.0307[a]	-0.0310[a]	-0.0314[a]
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Observations	20,813	20,813	20,813	20,813	20,813
R^2	0.004	0.014	0.020	0.021	0.021
Three-year bandwidth. Control Group (3 ≤ Age < 6), Treatment Group (6 ≤ Age < 9)					
policy[g]	-0.0209[a]	-0.0204[a]	-0.0210[a]	-0.0211[a]	-0.0214[a]
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	15,776	15,776	15,776	15,776	15,776
R^2	0.001	0.011	0.018	0.018	0.019
One-year bandwidth. Control Group (Age = 5), Treatment Group (Age = 6)					
policy[h]	-0.0094[a]	-0.0088[a]	-0.0094[b]	-0.0094[b]	-0.0099[b]
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Observations	5,573	5,573	5,573	5,573	5,573
R^2	0.000	0.013	0.021	0.023	0.025

Standard errors, clustered by age, in parentheses: [a] p<0.01, [b] p<0.05, [c] p<0.1, [d] p<0.15

Base Controls: policy, [e] cubic polynomial in age above and below threshold.

Base Controls (continued): [f] quadratic polynomial in age above and below threshold.

Base Controls (continued): [g] linear in age above and below threshold.

Base Controls (continued): [h] no age controls. Individual: race, gender, medical aid.

Household: province, urban, hh size, hh services, distance and time to medical care.

Maternal/Paternal: education, parent at home, medical aid access, income, un/employment.

Appendix A. Summary Data

Table A.1. Definitions and Descriptive Statistics of Data

VARS	Definition	age<6 Mean (s.e)	6≤age<12 Mean (s.e.)
	Outcome Variables		
payhlth	=1, if don't pay for care	0.4849	0.5822[a]
		(0.015)	(0.012)
public	= 1, if use public care	0.5367[a]	0.4421
		(0.011)	(0.004)
treat	=1, if treated at public/prvate facility	0.8167[a]	0.7357
		(0.020)	(0.005)
private	= 1, if use private care	0.2799	0.2936
		(0.017)	(0.007)
med_aid	=1, if have medical aid	0.1586	0.1663[c]
		(0.003)	(0.003)
illness	=1, if reported ill	0.1071[a]	0.0725
		(0.003)	(0.002)
	Individual Level Controls		
age	age of child	2.3049	8.4211
		(0.730)	(0.805)
dage	age of child, if older than 5	N/A	8.4211
		-	(0.805)
colour	=1, if mixed race	0.1327	0.1518[d]
		(0.005)	(0.008)
asian	=1, if Asian (Indian)	0.0466	0.0562
		(0.003)	(0.004)
black	=1, if black African	0.6912[a]	0.6409
		(0.007)	(0.017)
male	=1, if male	0.5191	0.5025
		(0.016)	(0.013)
	Household Level Controls		
hh_size	Number of HH members	5.9823	5.9841
		(0.083)	(0.036)
rmpc	Rooms in house per capita	1.3730	1.4176
		(0.025)	(0.006)

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Mean Estimates and Definitions for Analysis Data: Continued

VARS	Definition	age< 6 Mean (s.e)	6≤age<12 Mean (s.e.)
tap_h20	=1, if have tap water	0.6833 (0.009)	0.6737 (0.013)
fltoil	=1, if have flush toilet	0.5381 (0.008)	0.5550 (0.017)
urban	=1, if in urban area	0.5907 (0.012)	0.5965 (0.010)
tmed2	= 1, if facility within 30 mins	0.3160 (0.015)	0.3129 (0.013)
tmed3	=1, if facility within 60 mins	0.1616 (0.009)	0.1586 (0.006)
tmed4	=1, if facility outside 60 mins	0.1347 (0.004)	0.1242 (0.007)
dmed2	= 1, if facility within 1 to 5 km	0.3633 (0.015)	0.3624 (0.013)
dmed3	=1, if facility 5 km or more	0.3975 (0.021)	0.4002 (0.017)
prov_wc	=1 if living in Western Cape	0.0926 (0.008)	0.1166^b (0.009)
prov_ec	=1, if living in Eastern Cape	0.1905 (0.007)	0.1762 (0.011)
prov_nc	=1, if living in Northern Cape	0.0447 (0.004)	0.0471 (0.006)
prov_fs	=1, if living in Free State	0.0907^b (0.007)	0.0663 (0.005)
prov_kz	=1, if living in KwaZulu Natal	0.2733 (0.012)	0.2693 (0.008)
prov_nw	=1, if living in Northwest Province	0.0821 (0.004)	0.1057^b (0.009)
prov_gp	=1, if living in Gauteng	0.1235 (0.005)	0.1141 (0.007)
prov_mp	=1, if living in Mpumalanga	0.0736 (0.005)	0.0772 (0.005)
	Maternal Control Variables		
$momed^e$	mother's education in years	6.2602^a (0.128)	5.5092 (0.113)
$mmed^e$	=1, mother has medical aid	0.2293	0.2668^b

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Mean Estimates and Definitions for Analysis Data: Continued

VARS	Definition	age< 6 Mean (s.e)	6≤age<12 Mean (s.e.)
		(0.010)	(0.013)
munemp[e]	=1, mother is unemployed	0.1176[a]	0.0747
		(0.007)	(0.004)
memp[e]	=1, mother is employed	0.3219	0.3792[a]
		(0.014)	(0.015)
minc[e]	mother's monthy earnings	341.87	357.11
		(19.0)	(16.6)
minc_cd[e]	=1, earnings recorded in intervals	0.0604	0.0805[b]
		(0.007)	(0.006)
mnothome	=1, mother lives away	0.1038	0.1611[a]
		(0.013)	(0.007)
	Paternal Control Variables		
daded[f]	father's education in years	3.9468	3.9295
		(0.072)	(0.138)
fmed[f]	=1, father has medical aid	0.2037	0.2374[b]
		(0.009)	(0.017)
funemp[f]	=1, father is unemployed	0.0243	0.0243
		(0.002)	(0.006)
femp[f]	=1, father is employed	0.4658	0.4824
		(0.006)	(0.017)
finc[f]	father's monthy earnings	821.22	907.52
		(31.7)	(50.8)
finc_cd[f]	=1, earnings recorded in intervals	0.0920	0.1116[c]
		(0.008)	(0.008)
fnothome	=1, father lives away	0.4651[c]	0.4304
		(0.009)	(0.014)
Obs		1522	1192

Standard errors in parentheses; [a]p<0.01, [b]p<0.05, [c]p<0.1, [d]p<0.15
[e] =0 if mother not in house. [f] =0 if father not in house.

Appendix B. Policy Enforcement?

In a typical RD analysis, one would also like to examine the possibility that the policy is not perfectly enforced. At first glance, the data appears to make that possible. The data that we consider includes a question regarding whether or not the health care service that was sought required any payment. However, the wording in the question does not clarify whether or not there were any costs associated with the health service, such as transport, additional drugs or lost wages, as opposed to a payment directly realted to the use of a facility. If the question was interpreted to refer only to direct payment for use of the facility, which, should be zero for those eligible, the response to the question would give information with respect to the enforcement of the policy. If not, the responses would give very little indication of policy enforcement. More importantly, if the question was not interpreted in the way we would have liked, the responses to the question are not appropriate for use within a fuzzy RD design that could potentially reveal, even if after the fact, a local average treatment effect. An illustration of that concern is available in Figure B.1. Although young children's public health care service is, on average, less likely to have required payment than older children's, public health care payments for ill and injured five-year olds and six-year olds are extremely similar.[B.1]

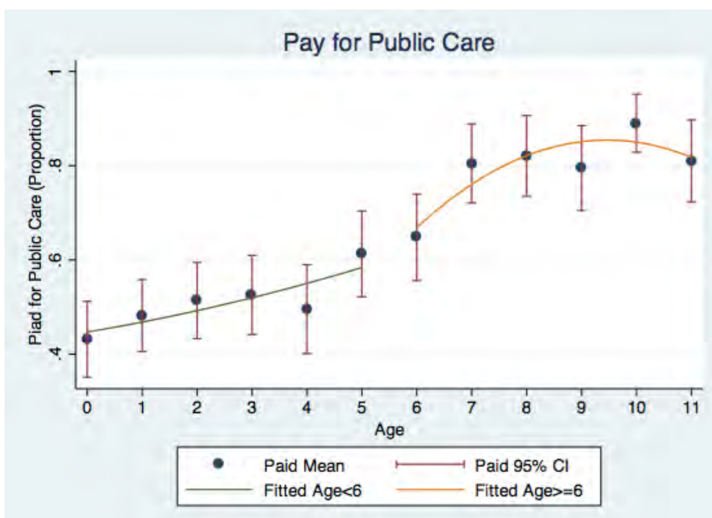

FIGURE B.1. Payment for Public Health Care Services

The regression results in Table B.1 concur with Figure B.1, in that consistent evidence of a payment reduction is uncovered. Although there is some evidence

[B.1]For this analysis, the subsample was limited to children that sought care at public facilities.

that those who were eligible for free care were less likely to pay for their visit to the public facility, especially in the smaller subsamples, that evidence is not convincing as the results vary largely and switch signs across subsamples, although they are reasonably similar within subsamples.[B.2]

[B.2]Although not reported in the table, public care for ill and injured children was more likely to require payment, when the child lived farther away from the health facility. In other words, it can generally be inferred that the question was not clear enough to separate direct health care payments from any additional side payments that might have been required to seek health care at the public facility in question.

TABLE B.1. Policy Enforcement: Paying for Public Care

VARIABLES	Base Controls	+Individual Controls	+Household Controls	+Maternal Controls	+Paternal Controls
Six-year bandwidth. Control Group (Age < 6), Treatment Group (6 ≤ Age ≤ 11)					
policy[e]	0.1291	0.1275	0.1099	0.1019	0.1020
	(0.091)	(0.090)	(0.092)	(0.096)	(0.081)
Observations	1,344	1,344	1,344	1,344	1,344
R^2	0.097	0.104	0.135	0.138	0.144
Five-year bandwidth. Control Group (1 ≤ Age < 6), Treatment Group (6 ≤ Age < 11)					
policy[e]	0.4075[a]	0.4074[a]	0.3662[a]	0.3685[a]	0.3490[a]
	(0.015)	(0.016)	(0.039)	(0.041)	(0.031)
Observations	1,119	1,119	1,119	1,119	1,119
R^2	0.090	0.093	0.131	0.135	0.139
Four-year bandwidth. Control Group (2 ≤ Age < 6), Treatment Group (6 ≤ Age < 10)					
policy[f]	0.1823[a]	0.1761[a]	0.1514[b]	0.1537[b]	0.1482[a]
	(0.029)	(0.040)	(0.049)	(0.050)	(0.042)
Observations	853	853	853	853	853
R^2	0.064	0.074	0.110	0.116	0.118
Three-year bandwidth. Control Group (3 ≤ Age < 6), Treatment Group (6 ≤ Age < 9)					
policy[g]	0.0101	-0.0059	-0.0211	-0.0146	-0.0178
	(0.047)	(0.048)	(0.047)	(0.045)	(0.039)
Observations	627	627	627	627	627
R^2	0.057	0.075	0.109	0.121	0.124
One-year bandwidth. Control Group (Age = 5), Treatment Group (Age = 6)					
policy[h]	-0.0350[a]	-0.0567[c]	-0.0562[d]	-0.0540	-0.0724
	(0.000)	(0.007)	(0.011)	(0.020)	(0.028)
Observations	216	216	216	216	216
R^2	0.001	0.042	0.132	0.148	0.178

Standard errors, clustered by age, in parentheses: [a] p<0.01, [b] p<0.05, [c] p<0.1, [d] p<0.15
Base Controls: policy, [e] cubic polynomial in age above and below threshold.
Base Controls (continued): [f] quadratic polynomial in age above and below threshold.
Base Controls (continued): [g] linear in age above and below threshold.
Base Controls (continued): [h] no age controls. Individual: race, gender, medical aid.
Household: province, urban, hh size, hh services, distance and time to medical care.
Maternal/Paternal: education, parent at home, medical aid access, income, un/employment.

Appendix C. Figures from McCoy & Khosa (1996)

Below, analsys from McCoy & Khosa (1996) are presented. The months in the figures are based on English. From left to right, the months are January, February, March, April, May, June, July, August, September, October, November and December. The list is repeated for each of the three years in the following figures.

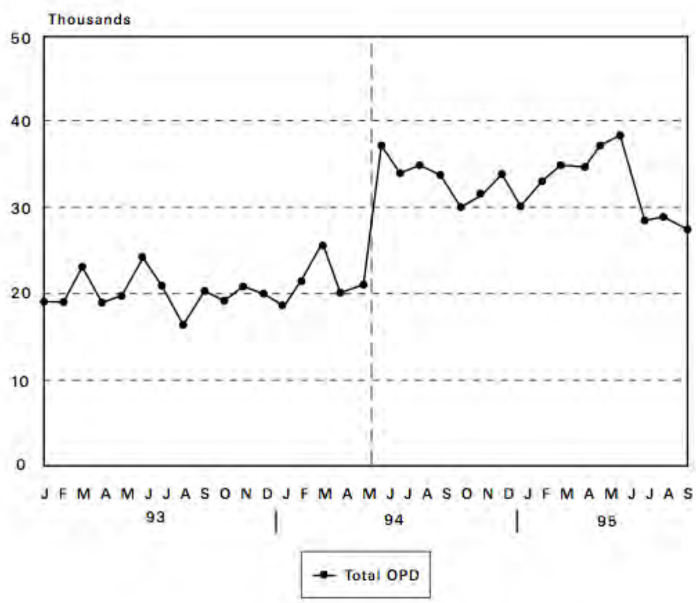

Figure C.1. Paediatric Attendance: Soweto Clinics

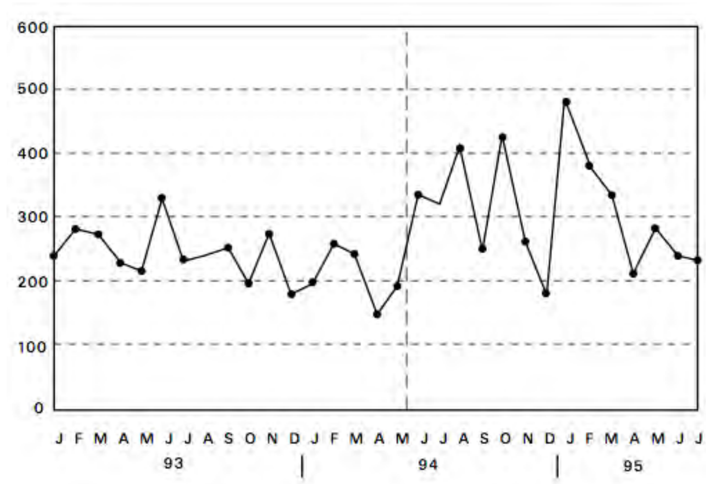

Figure C.2. Antenatal Bookings: KwaZulu-Natal Clinics

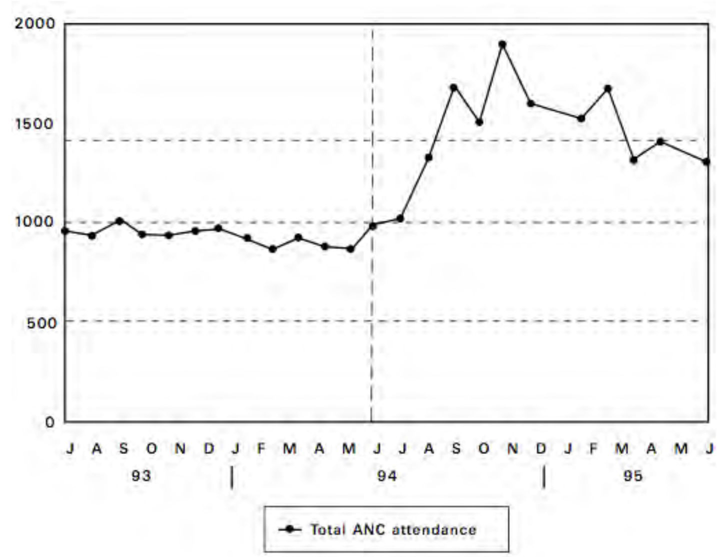

Figure C.3. Antenatal Attendance: Goldfields Regional Hospital

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