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# User Fee Abolition in South Africa: Re-Evaluating the Impact

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# User Fee Abolition in South Africa: 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 the use of public health care facilities for the receipt of curative care for the uninsured. The research also examines potential externalities that could arise from the policy, especially increased demand for curative care in the public sector amongst the insured. Regression discontinuity estimates, which control for the underlying relationship between age and receipt of curative care, point to a statistically insignificant policy impact amongst uninsured children and a statistically significant positive impact amongst insured children. In other words, the policy did not appear to improve access to healthcare, at least curative health care, for children who should have benefitted from the policy.

**Keywords:** Free Health Care, Regression Discontinuity

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# 1 Introduction

In 1994, the newly elected president of South Africa, Nelson Mandela, announced the beginning of 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 health care 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 abolition of user fees would be expected to increase the demand for healthcare. 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 1, 2 and 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 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 et al. (2001) extend the analysis to include both the 1994 and 1996 user fee changes, but continue only

[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.

to make use of registration data from the aforementioned mobile clinic. Unfortunately, their analyses are contradictory. 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.

User fee abolition's reach even included oral health. Bayat & Cleaton-Jones (2003), acknowleding this feature, examine dental clinic attendance in Soweto, following the 1996 policy change. Their study offers more insight than those previously discussed, since they have data for nine state-funded clinics (where user fees were abolished), as well as one pay-clinic (where user fees were unaffected). In the state 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 impacted casual patient visits - 54% increase at the state clinics and 7% increase at the pay clinic. Unsurprisingly, they find that patient/operator ratios were affected by the policy change, having worsened afterwards. However, as with the previous studies, it is not possible to interpret their results with respect to the entire population, given the small size of the sample.

Walker & Gilson (2004), suggest something similar to Bayat & Cleaton-Jones (2003). 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, nurses felt that the implementation process was poorly handled, which fits into a broader set of research related to implementation. Although Walker & Gilson's (2004) nurses also felt the policy positively impacted the poor, their is no evidence-base supporting the nurses' perceptions.

As noted above, the few studies avaialble are rather limited, as they focus on very small samples, and generally do not control for other variables that could impact the observed outcome. In the following analysis, some of those concerns are addressed. This research considers a nationally representative sample of children, rather than a sample of one to ten clinics. The focus of the analysis, however, is on health care provider choice for

children who were ill. In other words, the research examines curative care, rather than preventative care; thus, results may not be directly comparable to previous research. In addition expanding the representativeness of the sample, the research improves on the identification strategies previously applied in the literature. Specifically, the analysis makes use of regression discontinuity (RD) to infer the policy impact of free public health care, although only for curative care.

The impact of the policy is considered for receipt of health care in the public sector, for uninsured children, which should encompass the direct impacts of the policy. In addition, the analysis is extended to examine potential unintended consequences, such as substitution between the private and public sector, especially amongst those with access to health insurance. Although the results are consistent with a positive policy impact, in the sense that public health care demand is higher for those with access to free public services, that positive impact is statistically insignificant for the uninsured, but statistically significant for the insured. From these results, one can conclude that the policy did not, on average, increase access to public health care facilities. Instead, the abolition of user fees in public clinics improved access amongst children who were insured, and, therefore, were not meant to benefit from the policy. 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.

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 health care delivery, a

number of countries on the continent imposed user fees for public health services. However, experience with those fees was not particularly positive.[3] 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, such as that by Ridde & Morestin (2011) and Lagarde & Palmer (2008).

As there have been a number countries, wherein policy has changed, there is ample research to consider. However, as was the case with South Africa, much of the research is unable to capture policy effects, due to either data limitations or methodological shortcomings. Mwabu & Wang'ombe's (1997) Kenya study, for example, is based only on records from four health facilities, even though the data was collected over a lengthy period of time and covered four user fee regimes: (i) initially, there were no fees, (ii) fees were instated, (iii) fees were suspended and (iv) fees were reinstated. They find limited response to fee changes in the analysis, possibly because of the limited set of facilities.

In more recent research, data is generally available from a larger swathe of the relevant country. In Uganda, Nabyonga, Desmet, Karamagi et al. (2005), were able to collect data from six health ditricts in Uganda. In the year following implementation, they find a near 25% in the public facility usage; the increase is nearly 44% in referral centers. Importantly, they uncover an approximately 8% increase within the private-not-for-profit sector, where there were no fee changes. Although it might have been possible to consider difference-in-difference (DD) estimates of the true impact, they did not do so. However, a back of the envelope DD calculation of the effect suggests that the net effect ranged from 17% to 36%.

Similar results, in terms of percentage increases, were uncovered by Burnham, Pariyo, Galwango et al. (2004), who base their analysis on an even large Ugandan sample of 78 primary health clinics in 10 health districts; their data is primarily captured from attendance registers and health practitioners. They find a similarly large increase in new visits as well as under-5 visits, 53.3% and 27.7%, respectively. For child immunizations, the increase was 17.2%; a 25.3% increase in antenatal visits was observed. Much like

[3] 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)).

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 et al. (2008) examine utilization, with national data from Zambia, and quality of care, with a retrospective perceptions survey. 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. Increases in rural staff workloads are also uncovered. Surprisingly, they also find evidence of decreases in urban areas, suggesting district level substitution. They also find little evidence of service quality deterioration, despite the increased workload.

Although the majority of clinic-based studies, especially those covering a wide swathe of a country, user fee abolishment has been associated with large increases in facility usage. However, depending upon the study, those increases are often shown to negatively impact staff and service quality. In addition, there is evidence that user fee abolition (within the public sector) affects behaviour beyond the public sector. What is generally missing from these studies, though, is information from the client, who makes the decision to make use of a health facility. In some cases, it has been possible to match clients and health facilities to offer a glimpse into both facility level outcomes and household outcomes.

Both Xu, Evans, Kadama et al. (2006) and Penfold, Harrison, Bell et al. (2007) focus primarily on equity impacts at the household level, which has not yet been considered in South Africa. Xu et al.'s (2006) Ugandan study criticizes before and after studies, clinic-based studies fall within this genre, for ignoring other potentially important explanatory variables. Their contribution is to include a policy dummy along with a number of additional controls in a multinomial regression setting. They find that catastrophic expenditure was reduced for the non-poor, but not for the poor. They also find evidence of increased receipt of treatment at all facilities (public, private and other facilities). When combined, these results suggest that unintended policy consequences also

arose. However, the unintended consequences could also have arisen from the inability to capture an appropriate counterfactual.

Penfold et al. (2007), on the other hand, find that the abolition of user fees for child delivery in Ghana 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. At the household level, Asante, Chikwama, Daniels et al. (2007) find that the elimination of delivery fees reduced the costs of delivery by between 8% and 22%, depending on type of delivery. Furthermore, the cost reduction, led to a 12% reduction in the proportion of households facing catastrophic health care expenditures, as a result of childbirth.

Deininger & Mpuga (2005) is rather unique in considering both facility-level information and household information. In their facility level analysis, increases in health facility usage range from about 18.5% for under-fives, to 31% for all. Furthermore, there was a 26% increase in referrals. Simple services for children showed a large increase: a 38% increase in child weighing and a 61% for vitamin A supplementation. Also, they find 12% increase for antenatal care and 34% increase for post-natal care. However, both of these services were free before, as well, suggesting that the underlying utilization impact may not have been properly identified. If that is the case, their household level analysis, on rationing and reported illness, may also be incorrectly identified. Although there are some concerns over the true effect, they find an 8% and 11.5% reduction in rationing for adults and children, respectively. For reported illness, they find a 4.4% decrease in the propensity of children to fall sick, but virtually no effect on adults. Finally, they consider potential savings to the household, resulting from the policy, finding that there are large benefits, net of foregone user fees for the government, and, therefore, the policy has a pro-poor bias.

Although the literature suggests large and beneficial impacts are to be had, following the reduction in health care access/use charges, Lagarde & Palmer's (2008) review, is skeptical. 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, the focus of this analysis, they are especially concerned by willingness to imply

national conclusions based on data from such a small set of clinics or hospitals, and they worry about possible concurrent changes that were also likely to have affected the impact.[4] Accessing more representative data, while attempting to control for potential confounding factors, is, therefore, crucial to identifying the true impact of the user fee abolition policy.

## 3 Methodology

The user fee policy change announced in 1994 was designed for a myriad of different groups; however, the following analysis will focus only on the demand for curative care services for children under the age of six. 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. Since the component of the policy examined here has an age threshold, the analysis follows a Regression Discontinuity Design.

Due to the elimination of public facility user fees, and the implementation rule limiting the policy to those without access to health insurance, the policy is expected to increase the proportion of young uninsured ill children, treated by the public health care sector. In terms of the data, ill children can either be treated at a public facility, a private facility or not at all. However, in this analysis, only public facilities are considered.[5] Although the focus is on the uninsured, and their use of public health care, in their time of need, the analysis is extended to consider one unintended consequence that could arise from the policy.

As noted above, user fees were abolished for the very young, *if* they were not covered by health insurance; in South Africa, health insurance is offered by medical aid schemes. In practice, medical aid coverage is private information to the caregiver, and, therefore,

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

[5] In related research, we examine Regression Discontinuity Designs in a multinomial setting, using both parametric and nonparametric methods, and it is applied to these data. Similarly, as there was also a follow-up user fee abolition in 1996, which was applied to a different group, an analysis of the 1996 extension is considered, elsewhere.

it may not be truthfully revealed by the caregiver. Given that possibility, user fee abolition may have, unexpectantly, led to an increase in public facility usage amongst young children with access to medical aids. We investigate both the expected and the potential unintended policy impacts via RD on separate sample, the uninsured and the insured.

In order to specify the RD design, we define a binary indicator of policy access, $D$, which is determined by an age threshold; age is denoted by $a$, and the threshold, six years, in this analysis, is denoted by $a_0$. Therefore, $D = 1[a < a_0]$. Further, denote use of a public health facility, if the child is eligible for free public health care, as $Y_1 \in \{0, 1\}$, while $Y_0 \in \{0, 1\}$ is the use of public health care if the child does not qualify for free health care. In the analysis, children either are or are not treated in a public healthcare facility, when they are ill. 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 $\tau$, 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 one or the other, $Y = DY_1 + (1 - D)Y_0 = Y_0 + D(Y_1 - Y_0)$. Defining $\beta_0 = Y_0$, along with the previously defined $\tau$, and adding a stochastic component yields an incomplete, but helpful, regression formulation $Y_i = \beta_0 + \tau D_i + \nu_i$.

Assuming that the correlation between policy access and unobserved determinants of health facility choice is continuous across the age threshold, as in (1), the treatment effect can be uncovered.

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

The constant correlation assumption outlined in (1) allows for the correlation to be differenced from the regression, such that the treatment effect is identified as the difference between outcomes for the treatment and control groups, as in (2).

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

The regression model considered, therefore, is the following, and it includes controls

for age, as implied by (2).

$$Y_i = \tau D_i + g(\tilde{a}_i) + D_i h(\tilde{a}_i) + X_i\beta + \nu_i \tag{3}$$

In (3), $Y_i$ is binary indicator of treatment at a public facility for ill child $i$, $\tilde{a}_i = a_i - 6$ is the age of the child net of the threshold, because children under one year in age are recorded as zero years old. $D_i$ is the treatment indicator, $X_i$ is a set of control variables and $\nu_i$ is an error term. The treatment effect to be estimated is $\tau$. Due to the dependence of the policy on age, linear functions of age on either side of the threshold, represented by $g$ and $h$ are included in the regression. Rather than considering other polynomials, we allowed the data to suggest polynomials, via nonparametric regression following Li & Racine (2007). Importantly, nonparametric analysis of the underlying relationship supports a linear specification – see Figures 4 and 5 – therefore, the analysis only follows a linear specification. Finally, 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. However, the underlying representation of $g$ and $h$ are linear, due to the descriptive analysis, desribed below.

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, and is measured in years. Although it is discrete, birthdate data in days, weeks and months is also available. Unfortunately, there are too few observations to take advantage of the days or weeks data, and, therefore, the RD results focus on the months and years data. The summary analysis also suggests that there may be too few observations in the insured subsample to allow for anything other than a year-centric analysis.

# 4 The Data

Data for the analysis was sourced from the South African October Household Survey (OHS) of 1995.[6] 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. 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 aid. 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 14 and under who have been reported as ill 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 household head, which was merged back into the child data set. Finally, specific birthdates of children, which are only available for children born to mothers resident in the household, were also merged into the data. This last source of information was the only way to capture age more finely than in years. Notably, although the sample is affected by the resident mother requirement, the analysis is not significantly altered if we do not try to merge exact birthdates to the children, focussing instead on age in

[6] Although data from both 1994 and 1996 is also available, 1994 data does not include information on public healthcare usage, while 1996 was assumed to be confounded by a further change in the policy that extended free primary care within the public sector to a larger group.

years as the running variable.

For the analysis, a binary indicator for whether or not care for the ill child was sought in a public facility is the outcome of interest. However, for comparison purposes, we also created binary indicators for whether or not the ill child received treatment in a private facility, and whether or not the ill child received any treatment at all. Arithmetic means and standard errors for each of these outcomes are described in Table 1 for four different subsets. These subsets are: young children (under the age of six) and uninsured, old uninsured children (between the age of six and 14), young insured children and old insured children. As expected the insured are more likely to be treated in a private facility than a public facility, while the opposite is true for the uninsured. Furthermore, the uninsured children are more likely not to receive any care, when ill. With respect to the policy, there is some evidence that the use of public facilities is higher for younger children compared to older children; however, that difference is larger for the insured than the uninsured.

In addition to the outcome variables, a number of different control variables are included in the regression. These control variables are meant to detect heterogeneity in socioeconomic circumstances, as well as demographic and family circumstances that are also likely to be related to healthcare-seeking behaviour. We create dummy variables for the sex and population group of the child, along with binary indicators for province of dwelling and whether or not it is in an urban location. With respect to household structure, we develop categorical indicators of the number of members of the household and the number of adults in the household. We control for whether or not the ill child is the son or daughter of the household head, a grandchild of the household head or not directly related to the household head. In addition to relationship, we include categorical indicators of the household head's educational achievement, labour force status and a binary indicator of the sex of the household head. In terms of the accessibility of healthcare, we include categorical indicators of the location of the dwelling to the nearest medical facility, specifically, the medical facility that would most likely be used, if medical care was needed, along with the level of household income.

A summary of the control variables is provided in Appendix A, see Table A.1. A

number of features in the data are readily observable. Clearly, the insured and uninsured samples are different. There are household structure differences and population group differences, as well as household head education and labour force status differences. Within insurance status, however, the differences are not nearly so pronounced.

# 5 Results

The focus of the analysis is on differences in healthcare-seeking behaviour, especially in the public sector, that can be attributed to user fee abolition. As described above, the analysis follows a regression discontinuity design. Results are presented for the uninsured, who should be expected to benefit from the policy, as well as the insured, who are not supposed to benefit the from policy, given its design. Within each subgroup, the RD results are presented for differing bandwidths, or age ranges, to see whether or not the results are consistent. Unfortunately, there are power concerns, at least in the case of insured children aged from four years up to seven years, as there are only about 150 observations; therefore, statististical conclusions are difficult to draw for this subsample and subgroup. Other than the power concerns, however, the quantitiative results are fairly similar within subgroup, although there are differences across the subbroups. Furthermore, although a discontinuity can be observed in the preliminary analysis, and does, in fact, underpin the reported results, once additional controls are included in the regressions, the underlying discontinuity can no longer be identified in the residuals. In other words, the chosen regression polynomial in age, is an appropriate polynomial.

## 5.1 Uninsured Children

As noted earlier, user fees were abolished for many in South Africa, although this analysis focuses on children. Since user fees were meant only to be abolished for children under the age of six, who did not have access to health insurance, the initial analysis considers only uninsured children. We begin the analysis, illustratively, and then turn to regression results. However, the illustration speaks volumes, with respect to the regression results.

Figure 4 contains three panels, each containing similar information. Panel 4a presents simple estimates of the impact of user fee abolition for the uninsured children, based on

ages measured in years, panel 4b does the same for ages measured in months, while the relationship between age in weeks and public sector usage are illustrated in panel 4c. Within each figure are the predictions from both a linear and nonparametric regression of receipt of public health care by ill uninsured children, and that regression is applied to all of the observations on either side of the threshold.[7] When illustrating the fit, however, we limit the number of points in the figure. In addition to model fits, 95% confidence intervals for each model prediction is also included. Finally, each figure contains a simple mean estimate, by age, either in years, months or weeks, of the proportion of uninsured ill children receiving health care from a public facility. The only conclusion to be derived from the each of the panels in Figure 4 is that even though access to public facilities was improved, through the reduction in user fees, uninsured children were not any more likely to receive treatment in a public sector health facility, regardless of their eligibility for free care.

The impact of user fee abolition on young children, as illustrated in Figure 4, is borne out in the empirical analysis, as well. Those empirical results are available in Tables 2 and 3. Each table includes three sets of estimates. In each set, only the estimate of the policy indicator is presented, and it is presented without controls (other than the running variable); to the initial analysis, child controls are added, then location controls are included, and, finally, household controls are added. The running variable, upon which Table 2 is founded, is age in years. On the other hand, age in months is the running variable underpinning Table 3. In terms of the three sets of analyses included in each table, they represent different age bandwidths. In panels 2a and 3a all children through the age of 14 are included. Each subsequent panel is based on a smaller age group that is generally closer to the policy's age threshold. Panels 2b and 3b include only children aged three to ten, while the last panels, 2c and 3c, include results based only on children between the ages of four and seven, i.e., only two years above or below the threshold.

Two results are revealed in Tables 2 and 3. Firstly, the policy indicator is statistically insignificant, and, therefore, we conclude that caregivers of uninsured children were not

[7] The nonparametric regression makes use of gaussian second order kernel for age, and the bandwidth is selected via least squares cross-validation. For more information, see Li & Racine (2007).

enticed by free primary healthcare. Uninsured young children were not more likely to be treated in public facilities than uninsured older children. Secondly, the size of the effect, although still statistically insignificant, increases, as more controls are included in the regression discontinuity design. Therefore, we conclude that user fee reductions were more likely to affect caregivers of children close to the threshold than it was caregivers of children farther away from the threshold.

### 5.2 Insured Children

Although the primary purpose of the analysis was to examine the causal impact of user fee abolition on the receipt of curative care in the public sector amongst ill, but uninsured, young children, a secondary analysis is also considered. Since user fees were meant only to be abolished for children under the age of six, who did not have access to health insurance, those without health insurance should not have been affected by the policy. As before, we begin the analysis, illustratively, and then turn to regression results, and, also as before, the illustrations are generally supported by the regression.

Panels 5a, 5b and 5c of panel 5 illustrates estimates of the impact of user fee abolition on insured children. The initial illustration is over ages in years, followed by ages in months, and ages in weeks, respectively. Each figure contains linear and nonparametric model predictions, prediction confidence intervals and mean estimates of the receipt of public health care, by age; the regressions are estimated on either side of the threshold. With the exception of the nonparametric regression over age in months, the illustrated results suggest that a linear representation of the age polynomial on either side of the threshold is a reasonable representation. For the young children, see panel 5b, the nonparametric regression is clearly overfitting the data, and, therefore, only linear polynomials will be considered for these regressions. Although Figures 4 and 5 contain the same type of information, different conclusions are to be derived from each figure. For the insured, there is consistent evidence that access to free health care from the public sector was affecting decisions.

As seen in Figure 5, proportionally more young insured children under the age of six were receiving health care from the public sector than insured children at least six

years old, a result we were unable to uncover for the uninsured. Would that effect remain, following the inclusion of additional controls? We consider that via estimation of (3) applied to ill, but insured, children. The regression results of the policy impact on insured children are outlined in Tables 4 and 5. More often than not, the impact is found to be statistically significant, as well as large.

As before, the analysis is split according to the discreteness of the running variable, either in years or in months, in Tables 4 and 5, respectively. Within each table, the results are further split by bandwidth. First, all children through the age of 14 are considered. Second, the age range, or bandwidth, is narrowed to children aged between three and ten years old. Third, the bandwidth is further narrowed to those at least four up to the age of seven. Unfortunately, as we only started with about 700 uninsured ill children, statistical power in the final subgroup is quite limited.

Despite the limits in statistical power, the results implied in Figure 5 hold, for the most part. The average policy impact across insured children ranges from a low (and statistically insignificant) of 5.1% to a high (and statistically significant) of 23.3%. The majority of the estimates of the impact fall between 15% and 19%. Even though policy should not have affected the insured, the results suggest that it did, and it did so by a large margin.

### 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 statistically significant increase in the probability that an ill insured child will receive health care at a public facility, and that increase is around 17%; however, there is no evidence that the same policy had any impact on uninsured ill children, who should have benefitted from the policy.

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 nearer to 50%, whereas our results point to increases of only about a third, and only in

a subgroup that should not have been affected. 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 only amongst ill 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 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. Therefore, another question to consider is whether or not the policy impact is likely to be overstated or understated, as a result of the simplifying assumptions implied by our RD approach. In order to address this question, we first consider whether or not the RD approach appears to be valid. Specifically, one might wonder whether or not some of the variables in the analysis are also correlated with the policy. If so, the policy estimates could be tainted by bias.

The performance of the RD model is analyzed in Figures 6 and 7. As with the illustrations in Figures 4 and 5, we illustrate linear and nonparametric fits of the data, as well as average values of the data, by age group. The difference, however, is that the data being considered is not public health usage; rather, it is the residual from the regression. Specifically, we consider the residuals from the regressions with all children and all controls. For the uninsured, where more obserations are available, both the linear and nonparametric fit of the residuals suggest that residuals are zero on both sides of the threshold – see Figures 6a and 7a – suggesting that there is no information in the data that is specific to a particular age group. In other words, the RD approach is reasonable for this subgroup.

For the insured subsample; however, more caution is needed, since the nonparametric fits suggest some nonlinearities around zero. The most egregious example of the nonlinearity is observed in Figure 7b, which is representative of the overfitting that is possible in nonparametric analysis, and was also observed in Figure 5b. In Figure 6b

there is some evidence of curvature in the residuals, although it remains minimial and is focussed around zero. Importantly, though, the linear residual fits do suggest reasonable performance on both sides of the threshold.

Although there are some issues in the residuals analysis, especially amongst the insured, there are no obvious concerns arising out of the uninsured subsample. Therefore, we remain comfortable stating that user fee abolition neither statistically significantly nor noticeably increase the average usage of public health care facilities amongst uninsured children. One additional worry that arises in an analysis of this nature is the assumption that the RD was sharp. Even though the preceding analysis suggests that any fuzziness was likely not too important, one would be willing to believe that some six-year olds and some seven-year olds could have received free health care in the public sector. Identity documents, for example, 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. If so, the results would be underestimated. However, children are often rather proud of growing-up, and, therefore, it is also reasonable to believe that even if a parent were to lie about the child's age, the child may not be able to continue with the lie. Unfortunately, there is no data allowing us to examine these possibilities. Despite the potential underestimation, 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).

The difference between our results and those reported in the literature bring into question the analysis reported in James, Morris, Keith et al. (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. Since the increase in public healthcare demand that we were able to report was for insured

children, who most likely would have received care in private facilities, if needed, those simulation results could be strongly overstated. Even ignoring statistical significance, our results suggest that the increase is at most 7%, which is about $1/7^{th}$ the average effect reported in the literature. In that case, maybe only 10 000 to 21 000 children would be saved by user fee abolition. Admittedly, neither of those are small numbers, even though they are far less than James et al.'s (2005) research suggests.

# 6 Conclusion

Economic intuition 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, to some degree, our analysis. However, economic behaviour is slightly more complex than that simple intuition suggests. In the case of health care demand for children, some children have access to health insurance and some do not, and that access is likely to shape caregiver decisions. Our results suggest that the abolition of user fees in the public healthcare sector has led to demand substitution for the insured, but has not, on average, affected the uninsured. 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 that are unlikely to be representative of the country, as a whole. In our analysis, we are able to examine decisions made 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.

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 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 et al. (2006). Finally, the analysis focusses on average effects, and, therefore, additional research examining the distribution of effects could be relevant.

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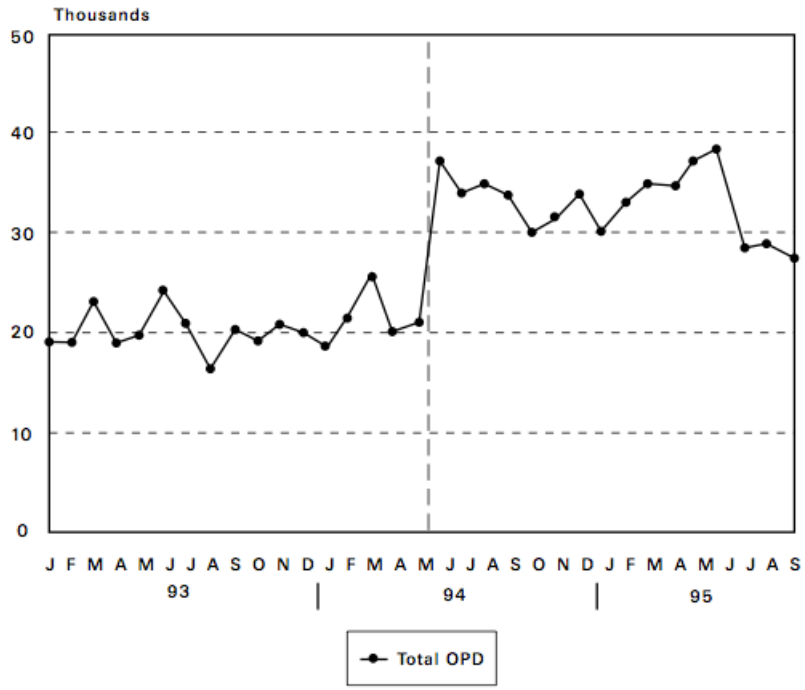



Figure 1: Paediatric Attendance: Soweto Clinics

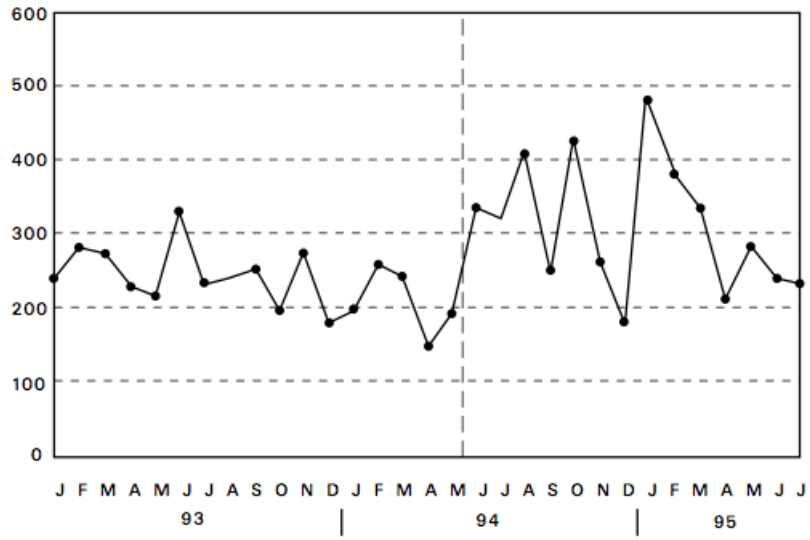



Figure 2: Antenatal Bookings: KwaZulu-Natal Clinics

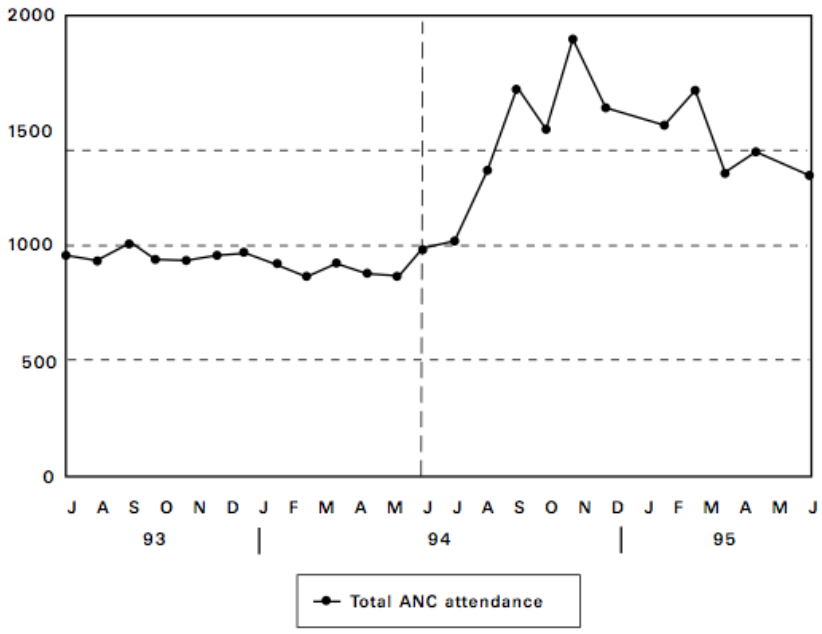
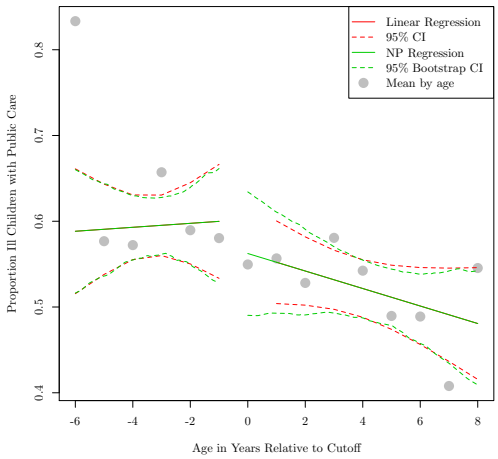


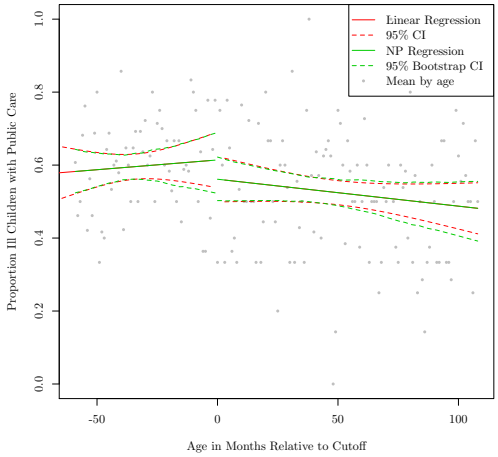

Figure 3: Antenatal Attendance: Goldfields Regional Hospital

Table 1: Health Facility Choice Summary Data

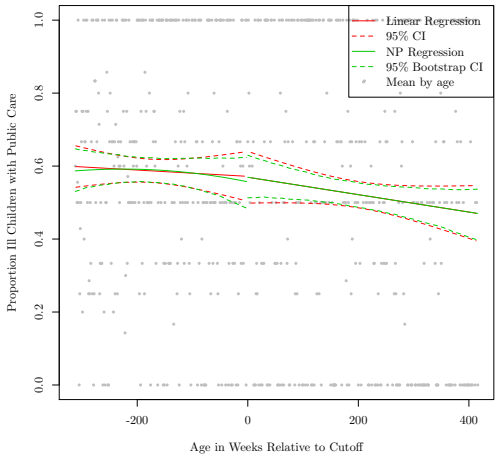
| | U Young | U Old | I Young | I Old |
|---|---|---|---|---|
| Public Care (mean) | 0.594 | 0.525 | 0.355 | 0.229 |
| Public (se) | 0.02 | 0.02 | 0.03 | 0.02 |
| Private Care (mean) | 0.224 | 0.208 | 0.500 | 0.574 |
| Private (se) | 0.01 | 0.01 | 0.03 | 0.02 |
| No Care (mean) | 0.181 | 0.268 | 0.145 | 0.196 |
| No (se) | 0.01 | 0.02 | 0.02 | 0.02 |
| Observations | 794 | 867 | 296 | 397 |




(a) Estimates over Years




(b) Estimates over Months




(c) Estimates over Weeks

Figure 4: Proportion of Uninsured Ill Children Receiving Public Care

Table 2: Public Care ATE amongst the Uninsured: Age Measured in Years

(a) ATE, Uninsured Children Aged 0-14 Years

| | Estimate | Std. Error | t value | Pr(>\|t\|) |
|---|---|---|---|---|
| No Controls | 0.040 | 0.054 | 0.737 | 0.461 |
| Child Controls | 0.037 | 0.054 | 0.685 | 0.494 |
| Locale Controls | 0.052 | 0.053 | 0.975 | 0.330 |
| Household Controls | 0.051 | 0.054 | 0.954 | 0.340 |

(b) ATE, Uninsured Children Aged 3-10 Years

| | Estimate | Std. Error | t value | Pr(>\|t\|) |
|---|---|---|---|---|
| No Controls | -0.016 | 0.079 | -0.203 | 0.839 |
| Child Controls | -0.018 | 0.079 | -0.235 | 0.814 |
| Locale Controls | 0.005 | 0.079 | 0.063 | 0.950 |
| Household Controls | 0.004 | 0.080 | 0.049 | 0.961 |

(c) ATE, Uninsured Children Aged 4-7 Years

| | Estimate | Std. Error | t value | Pr(>\|t\|) |
|---|---|---|---|---|
| No Controls | 0.030 | 0.101 | 0.295 | 0.768 |
| Child Controls | 0.028 | 0.102 | 0.275 | 0.783 |
| Locale Controls | 0.065 | 0.102 | 0.642 | 0.521 |
| Household Controls | 0.068 | 0.103 | 0.656 | 0.512 |

Table 3: Public Care ATE amongst the Uninsured: Age Measured in Months

(a) ATE, Uninsured Children Aged 0-168 Months

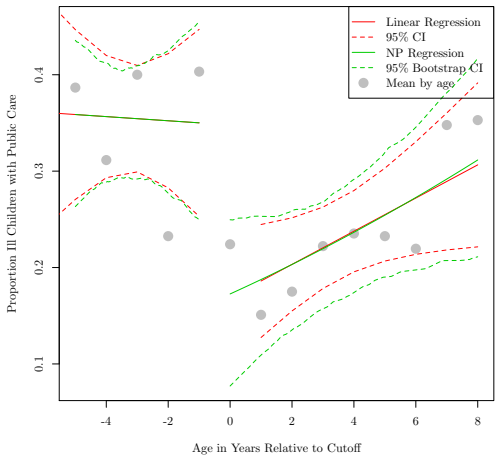
| | Estimate | Std. Error | t value | Pr(>\|t\|) |
|---|---|---|---|---|
| No Controls | 0.053 | 0.050 | 1.049 | 0.295 |
| Child Controls | 0.049 | 0.050 | 0.975 | 0.330 |
| Locale Controls | 0.064 | 0.050 | 1.279 | 0.201 |
| Household Controls | 0.064 | 0.050 | 1.289 | 0.198 |

(b) ATE, Uninsured Children Aged 36-120 Months

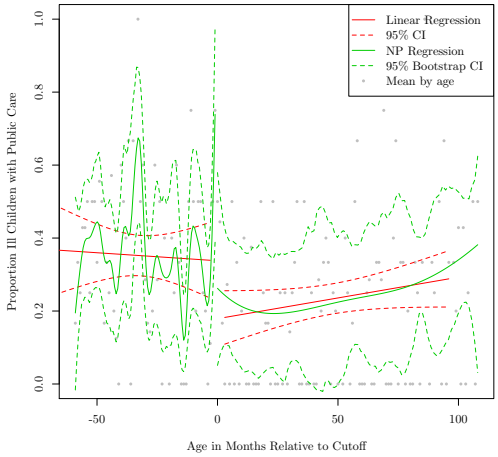
| | Estimate | Std. Error | t value | Pr(>\|t\|) |
|---|---|---|---|---|
| No Controls | 0.014 | 0.061 | 0.231 | 0.817 |
| Child Controls | 0.010 | 0.061 | 0.162 | 0.871 |
| Locale Controls | 0.032 | 0.060 | 0.535 | 0.593 |
| Household Controls | 0.032 | 0.061 | 0.527 | 0.598 |

(c) ATE, Uninsured Children Aged 48-84 Months

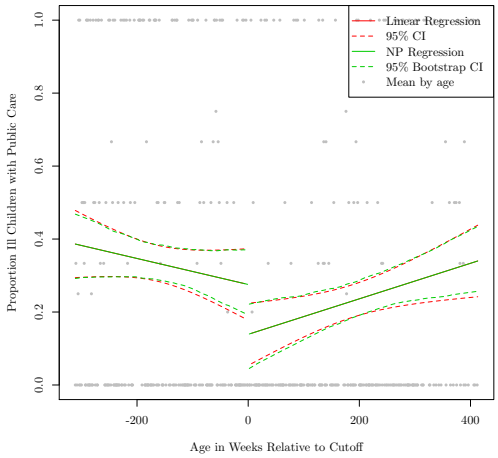
| | Estimate | Std. Error | t value | Pr(>\|t\|) |
|---|---|---|---|---|
| No Controls | 0.081 | 0.093 | 0.864 | 0.388 |
| Child Controls | 0.079 | 0.094 | 0.844 | 0.399 |
| Locale Controls | 0.118 | 0.094 | 1.257 | 0.209 |
| Household Controls | 0.122 | 0.095 | 1.281 | 0.201 |




(a) Estimates over Years




(b) Estimates over Months




(c) Estimates over Weeks

Figure 5: Proportion of Insured Ill Children Receiving Public Care

Table 4: Public Care ATE amongst the Insured: Age Measured in Years

(a) ATE, Insured Children Aged 0-14 Years

| | Estimate | Std. Error | t value | Pr(>\|t\|) |
|---|---|---|---|---|
| No Controls | 0.179 | 0.072 | 2.479 | 0.013 |
| Child Controls | 0.184 | 0.071 | 2.578 | 0.010 |
| Locale Controls | 0.156 | 0.071 | 2.180 | 0.030 |
| Household Controls | 0.158 | 0.071 | 2.224 | 0.026 |

(b) ATE, Insured Children Aged 3-10 Years

| | Estimate | Std. Error | t value | Pr(>\|t\|) |
|---|---|---|---|---|
| No Controls | 0.172 | 0.099 | 1.735 | 0.084 |
| Child Controls | 0.186 | 0.099 | 1.889 | 0.060 |
| Locale Controls | 0.166 | 0.101 | 1.652 | 0.099 |
| Household Controls | 0.183 | 0.098 | 1.858 | 0.064 |

(c) ATE, Insured Children Aged 4-7 Years

| | Estimate | Std. Error | t value | Pr(>\|t\|) |
|---|---|---|---|---|
| No Controls | 0.226 | 0.127 | 1.778 | 0.077 |
| Child Controls | 0.233 | 0.128 | 1.812 | 0.071 |
| Locale Controls | 0.188 | 0.136 | 1.385 | 0.168 |
| Household Controls | 0.167 | 0.138 | 1.214 | 0.227 |

Table 5: Public Care ATE amongst the Insured: Age Measured in Months

(a) ATE, Insured Children Aged 0-168 Months

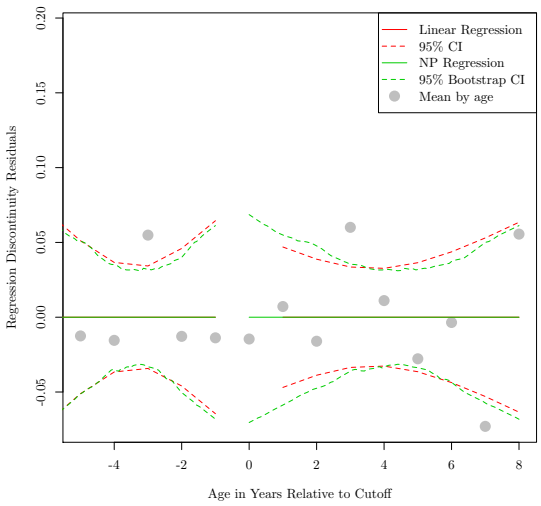
| | Estimate | Std. Error | t value | Pr(>\|t\|) |
|---|---|---|---|---|
| No Controls | 0.159 | 0.067 | 2.365 | 0.018 |
| Child Controls | 0.159 | 0.066 | 2.403 | 0.017 |
| Locale Controls | 0.132 | 0.066 | 1.989 | 0.047 |
| Household Controls | 0.146 | 0.066 | 2.211 | 0.027 |

(b) ATE, Insured Children Aged 36-120 Months

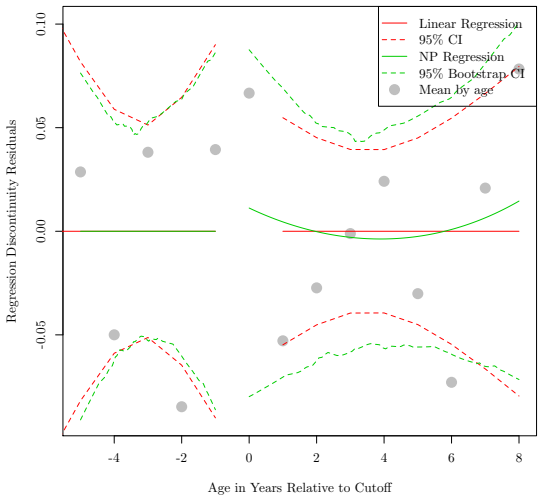
| | Estimate | Std. Error | t value | Pr(>\|t\|) |
|---|---|---|---|---|
| No Controls | 0.158 | 0.078 | 2.025 | 0.043 |
| Child Controls | 0.167 | 0.077 | 2.151 | 0.032 |
| Locale Controls | 0.138 | 0.078 | 1.780 | 0.076 |
| Household Controls | 0.161 | 0.077 | 2.092 | 0.037 |

(c) ATE, Insured Children Aged 48-84 Months

| | Estimate | Std. Error | t value | Pr(>\|t\|) |
|---|---|---|---|---|
| No Controls | 0.083 | 0.110 | 0.751 | 0.453 |
| Child Controls | 0.100 | 0.113 | 0.884 | 0.378 |
| Locale Controls | 0.064 | 0.120 | 0.529 | 0.598 |
| Household Controls | 0.058 | 0.120 | 0.479 | 0.633 |

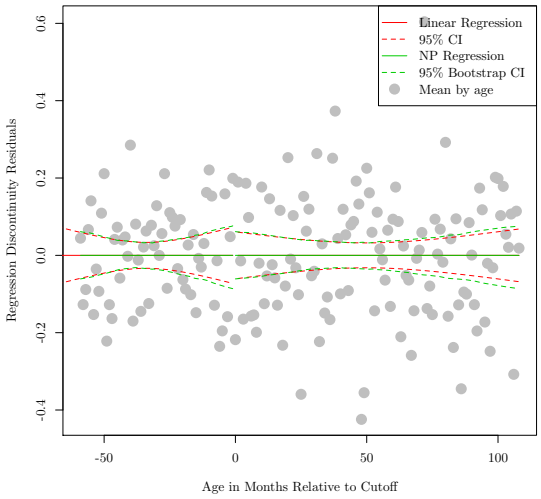



(a) RD Errors for the Uninsured

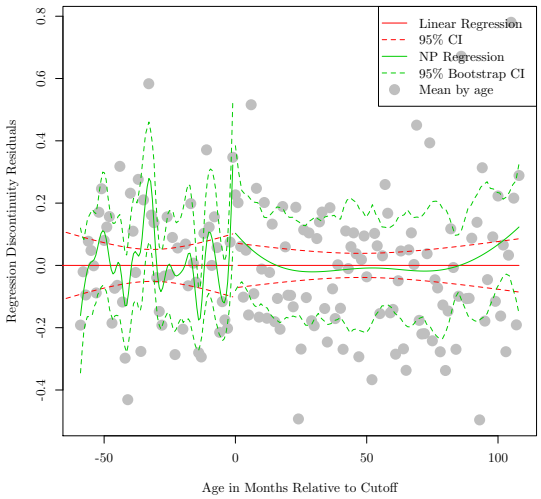



(b) RD Errors for the Insured

Figure 6: Regression Discontinuity Residuals Across the Treshold: Age in Years




(a) RD Errors for the Uninsured




(b) RD Errors for the Insured

Figure 7: Regression Discontinuity Residuals Across the Treshold: Age in Months

# A Summary of Control Variables

Table A.1: Health Facility Choice Summary Data

| | U Young | U Old | I Young | I Old |
|---|---|---|---|---|
| Male Child | 0.497 | 0.483 | 0.544 | 0.463 |
| African Child | 0.785 | 0.710 | 0.402 | 0.358 |
| Coloured Child | 0.126 | 0.155 | 0.125 | 0.123 |
| White Child | 0.044 | 0.065 | 0.412 | 0.443 |
| Western Cape | 0.087 | 0.112 | 0.125 | 0.174 |
| Eastern Cape | 0.207 | 0.188 | 0.125 | 0.098 |
| Free State | 0.087 | 0.063 | 0.115 | 0.063 |
| KwaZulu-Natal | 0.300 | 0.306 | 0.216 | 0.227 |
| Northwest Province | 0.082 | 0.091 | 0.071 | 0.091 |
| Gauteng Province | 0.092 | 0.077 | 0.216 | 0.196 |
| Mpumalanga Province | 0.078 | 0.078 | 0.047 | 0.098 |
| Limpopo Province | 0.026 | 0.030 | 0.041 | 0.025 |
| Urban | 0.476 | 0.496 | 0.845 | 0.834 |
| Med Center > 5km | 0.416 | 0.441 | 0.321 | 0.307 |
| 1 km < Med Center < 5km | 0.358 | 0.355 | 0.395 | 0.406 |
| More than 10 People in HH | 0.128 | 0.121 | 0.030 | 0.038 |
| 8-9 in HH | 0.142 | 0.135 | 0.044 | 0.055 |
| 7 in HH | 0.132 | 0.122 | 0.034 | 0.048 |
| 6 in HH | 0.144 | 0.166 | 0.101 | 0.164 |
| 5 in HH | 0.165 | 0.213 | 0.253 | 0.277 |
| 4 in HH | 0.180 | 0.160 | 0.355 | 0.320 |
| More than 6 Adults in HH | 0.055 | 0.060 | 0.017 | 0.003 |
| 5 adults | 0.083 | 0.061 | 0.017 | 0.018 |
| 4 adults | 0.130 | 0.126 | 0.068 | 0.103 |
| 3 adults | 0.174 | 0.211 | 0.095 | 0.123 |
| 2 adults | 0.462 | 0.431 | 0.747 | 0.690 |
| Child of HH Head | 0.615 | 0.777 | 0.872 | 0.947 |
| Grandchild of Head | 0.345 | 0.187 | 0.118 | 0.050 |
| Head No Education | 0.581 | 0.574 | 0.108 | 0.141 |
| Head Primary Education | 0.304 | 0.314 | 0.270 | 0.302 |
| Head Matric | 0.098 | 0.097 | 0.497 | 0.433 |
| Head NLFP | 0.286 | 0.272 | 0.071 | 0.073 |
| Head Unemployed | 0.063 | 0.075 | 0.017 | 0.018 |
| Head Male | 0.632 | 0.611 | 0.841 | 0.877 |
| HH Income | 2633.0 | 2351.9 | 38350.1 | 47326.1 |

# B Complete Empirical Results for Years Regressions

Table B.1: Uninsured Full Regression Results: Public Care, Age (0-14) in Years

| | Estimate | Std. Error | t value | Pr(>\|t\|) |
|---|---|---|---|---|
| (Intercept) | 0.542 | 0.094 | 5.775 | 0.000 |
| rd.year-6 | 0.053 | 0.054 | 0.996 | 0.320 |
| years | -0.008 | 0.007 | -1.231 | 0.218 |
| sexmale | -0.017 | 0.024 | -0.713 | 0.476 |
| pop.groupcolour | -0.004 | 0.042 | -0.096 | 0.923 |
| pop.groupasian | -0.228 | 0.058 | -3.955 | 0.000 |
| pop.groupwhite | -0.232 | 0.062 | -3.759 | 0.000 |
| hh.relategrandchild | 0.082 | 0.067 | 1.210 | 0.226 |
| hh.relatechild | 0.026 | 0.067 | 0.390 | 0.696 |
| provEC | 0.004 | 0.051 | 0.073 | 0.942 |
| provNC | 0.101 | 0.067 | 1.511 | 0.131 |
| provFS | -0.248 | 0.063 | -3.966 | 0.000 |
| provKZN | 0.057 | 0.051 | 1.124 | 0.261 |
| provNW | 0.049 | 0.060 | 0.814 | 0.416 |
| provGP | -0.012 | 0.059 | -0.204 | 0.838 |
| provMP | 0.051 | 0.063 | 0.817 | 0.414 |
| provLP | 0.100 | 0.085 | 1.171 | 0.242 |
| urbanyes | 0.007 | 0.032 | 0.213 | 0.831 |
| dist.med.L | 0.005 | 0.025 | 0.193 | 0.847 |
| dist.med.Q | 0.035 | 0.022 | 1.632 | 0.103 |
| hh.inc | -0.000 | 0.000 | -0.214 | 0.831 |
| hh.inc.sq | 0.000 | 0.000 | 0.217 | 0.828 |
| adults.L | 0.045 | 0.063 | 0.714 | 0.475 |
| adults.Q | 0.052 | 0.043 | 1.202 | 0.229 |
| adults.C | -0.012 | 0.039 | -0.308 | 0.758 |
| adults^4 | 0.022 | 0.035 | 0.636 | 0.525 |
| adults^5 | 0.019 | 0.032 | 0.585 | 0.558 |
| hh.size.L | 0.055 | 0.051 | 1.078 | 0.281 |
| hh.size.Q | -0.083 | 0.038 | -2.216 | 0.027 |
| hh.size.C | 0.010 | 0.034 | 0.290 | 0.772 |
| hh.size^4 | -0.018 | 0.032 | -0.564 | 0.573 |
| hh.size^5 | 0.009 | 0.031 | 0.277 | 0.782 |
| hh.size^6 | -0.038 | 0.031 | -1.235 | 0.217 |
| hh.ed.L | 0.003 | 0.071 | 0.040 | 0.968 |
| hh.ed.Q | 0.006 | 0.054 | 0.108 | 0.914 |
| hh.ed.C | 0.001 | 0.037 | 0.025 | 0.980 |
| hh.labourstrunemp | 0.092 | 0.055 | 1.681 | 0.093 |
| hh.labouremployed | -0.024 | 0.033 | -0.734 | 0.463 |
| hh.sexmale | 0.000 | 0.030 | 0.007 | 0.994 |
| rd.year-6:years | 0.014 | 0.014 | 0.966 | 0.334 |

Table B.2: Uninsured Full Regression Results: Public Care, Age 3 to 10 in Years

| | Estimate | Std. Error | t value | Pr(>\|t\|) |
|---|---|---|---|---|
| (Intercept) | 0.779 | 0.135 | 5.781 | 0.000 |
| rd.year-6 | 0.007 | 0.080 | 0.088 | 0.930 |
| years | 0.011 | 0.021 | 0.509 | 0.611 |
| sexmale | -0.060 | 0.036 | -1.672 | 0.095 |
| pop.groupcolour | -0.046 | 0.061 | -0.746 | 0.456 |
| pop.groupasian | -0.286 | 0.089 | -3.226 | 0.001 |
| pop.groupwhite | -0.192 | 0.088 | -2.175 | 0.030 |
| hh.relategrandchild | -0.008 | 0.102 | -0.081 | 0.935 |
| hh.relatechild | -0.075 | 0.102 | -0.734 | 0.463 |
| provEC | -0.101 | 0.076 | -1.334 | 0.182 |
| provNC | 0.028 | 0.102 | 0.272 | 0.786 |
| provFS | -0.299 | 0.090 | -3.335 | 0.001 |
| provKZN | -0.022 | 0.075 | -0.292 | 0.770 |
| provNW | -0.059 | 0.088 | -0.668 | 0.504 |
| provGP | 0.005 | 0.085 | 0.058 | 0.953 |
| provMP | -0.084 | 0.094 | -0.896 | 0.370 |
| provLP | -0.041 | 0.122 | -0.335 | 0.738 |
| urbanyes | 0.029 | 0.048 | 0.592 | 0.554 |
| dist.med.L | -0.003 | 0.037 | -0.090 | 0.928 |
| dist.med.Q | 0.008 | 0.032 | 0.238 | 0.812 |
| hh.inc | -0.000 | 0.000 | -0.184 | 0.854 |
| hh.inc.sq | 0.000 | 0.000 | 0.073 | 0.942 |
| adults.L | 0.028 | 0.097 | 0.287 | 0.774 |
| adults.Q | -0.071 | 0.064 | -1.111 | 0.267 |
| adults.C | 0.122 | 0.059 | 2.074 | 0.038 |
| adults^4 | -0.013 | 0.053 | -0.248 | 0.805 |
| adults^5 | 0.041 | 0.048 | 0.852 | 0.394 |
| hh.size.L | 0.089 | 0.077 | 1.164 | 0.245 |
| hh.size.Q | 0.000 | 0.056 | 0.002 | 0.998 |
| hh.size.C | 0.006 | 0.051 | 0.119 | 0.905 |
| hh.size^4 | -0.002 | 0.046 | -0.041 | 0.968 |
| hh.size^5 | 0.003 | 0.045 | 0.074 | 0.941 |
| hh.size^6 | -0.031 | 0.045 | -0.687 | 0.492 |
| hh.ed.L | 0.094 | 0.096 | 0.974 | 0.330 |
| hh.ed.Q | -0.008 | 0.075 | -0.106 | 0.916 |
| hh.ed.C | -0.027 | 0.054 | -0.489 | 0.625 |
| hh.labourstrunemp | -0.046 | 0.084 | -0.554 | 0.580 |
| hh.labouremployed | -0.038 | 0.049 | -0.771 | 0.441 |
| hh.sexmale | 0.013 | 0.046 | 0.274 | 0.784 |
| rd.year-6:years | -0.038 | 0.039 | -0.985 | 0.325 |

Table B.3: Uninsured Full Regression Results: Public Care, Age 4 to 7 in Years

| | Estimate | Std. Error | t value | Pr(>\|t\|) |
|---|---|---|---|---|
| (Intercept) | 0.939 | 0.238 | 3.939 | 0.000 |
| rd.year-6 | 0.043 | 0.117 | 0.371 | 0.711 |
| years | 0.001 | 0.070 | 0.021 | 0.983 |
| sexmale | -0.034 | 0.058 | -0.579 | 0.563 |
| pop.groupcolour | 0.021 | 0.108 | 0.193 | 0.847 |
| pop.groupasian | -0.264 | 0.140 | -1.885 | 0.060 |
| pop.groupwhite | -0.224 | 0.139 | -1.611 | 0.108 |
| hh.relategrandchild | -0.124 | 0.191 | -0.650 | 0.516 |
| hh.relatechild | -0.161 | 0.183 | -0.881 | 0.379 |
| provEC | -0.117 | 0.123 | -0.952 | 0.342 |
| provNC | -0.013 | 0.171 | -0.076 | 0.940 |
| provFS | -0.275 | 0.146 | -1.887 | 0.060 |
| provKZN | 0.037 | 0.124 | 0.295 | 0.768 |
| provNW | 0.013 | 0.146 | 0.086 | 0.931 |
| provGP | 0.122 | 0.137 | 0.895 | 0.372 |
| provMP | -0.025 | 0.147 | -0.167 | 0.867 |
| provLP | 0.056 | 0.176 | 0.320 | 0.749 |
| urbanyes | -0.021 | 0.078 | -0.267 | 0.790 |
| dist.med.L | 0.030 | 0.061 | 0.490 | 0.625 |
| dist.med.Q | -0.018 | 0.053 | -0.343 | 0.732 |
| hh.inc | 0.000 | 0.000 | 0.278 | 0.781 |
| hh.inc.sq | -0.000 | 0.000 | -0.144 | 0.886 |
| adults.L | -0.096 | 0.153 | -0.629 | 0.530 |
| adults.Q | -0.026 | 0.099 | -0.265 | 0.791 |
| adults.C | 0.071 | 0.098 | 0.724 | 0.470 |
| adults^4 | 0.030 | 0.091 | 0.328 | 0.743 |
| adults^5 | -0.004 | 0.081 | -0.044 | 0.965 |
| hh.size.L | 0.205 | 0.121 | 1.694 | 0.091 |
| hh.size.Q | -0.048 | 0.094 | -0.517 | 0.605 |
| hh.size.C | 0.025 | 0.085 | 0.301 | 0.763 |
| hh.size^4 | 0.058 | 0.074 | 0.786 | 0.433 |
| hh.size^5 | 0.010 | 0.074 | 0.138 | 0.890 |
| hh.size^6 | -0.022 | 0.072 | -0.311 | 0.756 |
| hh.ed.L | 0.216 | 0.158 | 1.368 | 0.172 |
| hh.ed.Q | 0.077 | 0.124 | 0.620 | 0.535 |
| hh.ed.C | 0.127 | 0.087 | 1.456 | 0.147 |
| hh.labourstrunemp | -0.167 | 0.129 | -1.293 | 0.197 |
| hh.labouremployed | -0.028 | 0.080 | -0.344 | 0.731 |
| hh.sexmale | -0.049 | 0.071 | -0.689 | 0.491 |

Table B.4: Insured Full Regression Results: Public Care, Age (0-14) in Years

| | Estimate | Std. Error | t value | Pr(>\|t\|) |
|---|---|---|---|---|
| (Intercept) | 0.101 | 0.120 | 0.838 | 0.402 |
| rd.year-6 | 0.158 | 0.071 | 2.224 | 0.026 |
| years | 0.016 | 0.009 | 1.797 | 0.073 |
| sexmale | 0.009 | 0.034 | 0.260 | 0.795 |
| pop.groupcolour | -0.035 | 0.060 | -0.588 | 0.557 |
| pop.groupasian | -0.153 | 0.073 | -2.094 | 0.037 |
| pop.groupwhite | -0.105 | 0.045 | -2.323 | 0.020 |
| hh.relate.L | 0.103 | 0.160 | 0.640 | 0.522 |
| hh.relate.Q | -0.116 | 0.109 | -1.066 | 0.287 |
| provEC | 0.124 | 0.067 | 1.865 | 0.063 |
| provNC | 0.362 | 0.099 | 3.648 | 0.000 |
| provFS | 0.267 | 0.074 | 3.630 | 0.000 |
| provKZN | 0.131 | 0.058 | 2.253 | 0.025 |
| provNW | 0.116 | 0.075 | 1.539 | 0.124 |
| provGP | 0.059 | 0.058 | 1.028 | 0.304 |
| provMP | 0.037 | 0.077 | 0.480 | 0.631 |
| provLP | -0.062 | 0.106 | -0.585 | 0.559 |
| urbanyes | -0.021 | 0.052 | -0.396 | 0.692 |
| dist.med.L | 0.043 | 0.033 | 1.331 | 0.184 |
| dist.med.Q | 0.013 | 0.029 | 0.444 | 0.657 |
| hh.inc | 0.000 | 0.000 | 2.524 | 0.012 |
| hh.inc.sq | -0.000 | 0.000 | -2.568 | 0.010 |
| adults.L | -0.054 | 0.161 | -0.338 | 0.735 |
| adults.Q | 0.039 | 0.118 | 0.335 | 0.738 |
| adults.C | -0.030 | 0.106 | -0.288 | 0.774 |
| adults^4 | 0.079 | 0.089 | 0.882 | 0.378 |
| adults^5 | -0.181 | 0.065 | -2.770 | 0.006 |
| hh.size.L | -0.065 | 0.094 | -0.686 | 0.493 |
| hh.size.Q | -0.012 | 0.071 | -0.165 | 0.869 |
| hh.size.C | 0.016 | 0.067 | 0.245 | 0.806 |
| hh.size^4 | -0.040 | 0.059 | -0.689 | 0.491 |
| hh.size^5 | -0.061 | 0.062 | -0.986 | 0.324 |
| hh.size^6 | -0.008 | 0.055 | -0.150 | 0.881 |
| hh.ed.L | -0.122 | 0.055 | -2.210 | 0.027 |
| hh.ed.Q | -0.050 | 0.042 | -1.206 | 0.228 |
| hh.ed.C | 0.042 | 0.031 | 1.326 | 0.185 |
| hh.labour.L | -0.079 | 0.060 | -1.326 | 0.185 |
| hh.labour.Q | 0.144 | 0.109 | 1.326 | 0.185 |
| hh.sexmale | 0.001 | 0.070 | 0.008 | 0.994 |
| rd.year-6:years | -0.019 | 0.019 | -0.980 | 0.328 |

Table B.5: Insured Full Regression Results: Public Care, Age 3 to 10 in Years

| | Estimate | Std. Error | t value | Pr(>\|t\|) |
|---|---|---|---|---|
| (Intercept) | 0.061 | 0.191 | 0.317 | 0.752 |
| rd.year-6 | 0.183 | 0.098 | 1.858 | 0.064 |
| years | -0.008 | 0.026 | -0.303 | 0.762 |
| sexmale | -0.010 | 0.045 | -0.214 | 0.830 |
| pop.groupcolour | -0.019 | 0.079 | -0.247 | 0.805 |
| pop.groupasian | -0.173 | 0.095 | -1.811 | 0.071 |
| pop.groupwhite | -0.085 | 0.063 | -1.345 | 0.180 |
| hh.relate.L | 0.305 | 0.309 | 0.985 | 0.326 |
| hh.relate.Q | -0.173 | 0.201 | -0.863 | 0.389 |
| provEC | 0.102 | 0.090 | 1.126 | 0.261 |
| provNC | 0.173 | 0.139 | 1.245 | 0.214 |
| provFS | 0.353 | 0.100 | 3.523 | 0.000 |
| provKZN | 0.106 | 0.078 | 1.364 | 0.173 |
| provNW | 0.007 | 0.104 | 0.066 | 0.947 |
| provGP | -0.010 | 0.075 | -0.137 | 0.891 |
| provMP | -0.041 | 0.112 | -0.369 | 0.712 |
| provLP | -0.193 | 0.133 | -1.451 | 0.148 |
| urbanyes | 0.000 | 0.071 | 0.002 | 0.998 |
| dist.med.L | 0.008 | 0.043 | 0.194 | 0.846 |
| dist.med.Q | 0.011 | 0.039 | 0.283 | 0.777 |
| hh.inc | 0.000 | 0.000 | 1.411 | 0.159 |
| hh.inc.sq | -0.000 | 0.000 | -1.650 | 0.100 |
| adults.L | -0.119 | 0.214 | -0.558 | 0.577 |
| adults.Q | 0.238 | 0.164 | 1.451 | 0.148 |
| adults.C | -0.170 | 0.151 | -1.124 | 0.262 |
| adults^4 | 0.214 | 0.119 | 1.801 | 0.073 |
| adults^5 | -0.289 | 0.091 | -3.183 | 0.002 |
| hh.size.L | -0.350 | 0.131 | -2.679 | 0.008 |
| hh.size.Q | 0.121 | 0.098 | 1.234 | 0.218 |
| hh.size.C | 0.089 | 0.093 | 0.958 | 0.339 |
| hh.size^4 | -0.143 | 0.085 | -1.690 | 0.092 |
| hh.size^5 | 0.113 | 0.086 | 1.316 | 0.189 |
| hh.size^6 | -0.127 | 0.072 | -1.760 | 0.079 |
| hh.ed.L | -0.116 | 0.074 | -1.579 | 0.115 |
| hh.ed.Q | -0.114 | 0.057 | -2.016 | 0.045 |
| hh.ed.C | 0.066 | 0.043 | 1.549 | 0.122 |
| hh.labour.L | 0.050 | 0.081 | 0.615 | 0.539 |
| hh.labour.Q | 0.229 | 0.164 | 1.397 | 0.163 |
| hh.sexmale | -0.046 | 0.096 | -0.483 | 0.630 |
| rd.year-6:years | 0.030 | 0.049 | 0.602 | 0.548 |

Table B.6: Insured Full Regression Results: Public Care, Age 4 to 7 in Years

| | Estimate | Std. Error | t value | Pr(>\|t\|) |
|---|---|---|---|---|
| (Intercept) | -0.005 | 0.242 | -0.020 | 0.984 |
| rd.year-6 | 0.097 | 0.151 | 0.643 | 0.522 |
| years | -0.058 | 0.088 | -0.652 | 0.516 |
| sexmale | -0.001 | 0.072 | -0.019 | 0.985 |
| pop.groupcolour | 0.016 | 0.120 | 0.137 | 0.892 |
| pop.groupasian | -0.153 | 0.155 | -0.988 | 0.325 |
| pop.groupwhite | -0.010 | 0.098 | -0.104 | 0.918 |
| hh.relate.L | 0.125 | 0.339 | 0.370 | 0.712 |
| hh.relate.Q | -0.212 | 0.276 | -0.767 | 0.444 |
| provEC | 0.118 | 0.140 | 0.842 | 0.401 |
| provNC | 0.136 | 0.241 | 0.567 | 0.572 |
| provFS | 0.464 | 0.168 | 2.753 | 0.007 |
| provKZN | 0.170 | 0.120 | 1.421 | 0.158 |
| provNW | 0.074 | 0.142 | 0.518 | 0.605 |
| provGP | 0.018 | 0.125 | 0.147 | 0.883 |
| provMP | 0.390 | 0.182 | 2.149 | 0.033 |
| provLP | 0.015 | 0.222 | 0.069 | 0.945 |
| urbanyes | 0.069 | 0.119 | 0.582 | 0.562 |
| dist.med.L | -0.057 | 0.067 | -0.850 | 0.397 |
| dist.med.Q | 0.092 | 0.065 | 1.429 | 0.155 |
| hh.inc | 0.000 | 0.000 | 0.613 | 0.541 |
| hh.inc.sq | -0.000 | 0.000 | -0.788 | 0.432 |
| adults.L | 0.244 | 0.333 | 0.732 | 0.466 |
| adults.Q | -0.278 | 0.239 | -1.160 | 0.248 |
| adults.C | 0.310 | 0.166 | 1.874 | 0.063 |
| adults^4 | -0.138 | 0.124 | -1.116 | 0.266 |
| hh.size.L | -0.354 | 0.232 | -1.531 | 0.128 |
| hh.size.Q | -0.001 | 0.196 | -0.004 | 0.997 |
| hh.size.C | 0.256 | 0.179 | 1.429 | 0.155 |
| hh.size^4 | -0.169 | 0.154 | -1.098 | 0.274 |
| hh.size^5 | 0.071 | 0.144 | 0.495 | 0.621 |
| hh.size^6 | -0.047 | 0.115 | -0.409 | 0.683 |
| hh.ed.L | 0.076 | 0.122 | 0.628 | 0.531 |
| hh.ed.Q | -0.219 | 0.087 | -2.511 | 0.013 |
| hh.ed.C | 0.151 | 0.066 | 2.282 | 0.024 |
| hh.labour.L | -0.031 | 0.142 | -0.221 | 0.826 |
| hh.labour.Q | 0.352 | 0.209 | 1.689 | 0.093 |
| hh.sexmale | -0.043 | 0.162 | -0.263 | 0.793 |