User Fee Abolition in South Africa in 1994 and 1996: Differences-in-Differences

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Abstract

South Africa waived user fees for primary health care, first in 1994, and again, in 1996. Since the 1994 plan focused on young children and older adults, as well as pregnant and nursing mothers, the 1996 change, which waived fees for the remainder of the population, subject to means tests, can be examined via differences-in-differences (DD). DD is applied to a subsample of children, underpinned by a multinomial logit regression of health-seeking behavior amongst ill and injured children. Although the policy provided free primary care to all at public clinics, the results of the analysis do not support the hypothesis that free primary care significantly increased public clinic visits amongst ill and injured children. However, there is strong evidence that ill and injured children were more likely (by 6%) to seek at least some sort of treatment following the change in policy, implying that the policy was indirectly successful.

1 Introduction

President Mandela, of the newly elected democratic government of South Africa, announced that primary healthcare services would be provided without charge at all state facilities. The official policy, enacted June 1, 1994, was for children under the age of six years, pregnant and nursing mothers, and the elderly, as long as they were not currently members of a medical scheme (Wilkinson *et al.* 1997; Leatt *et al.* 2006; South African Government 1994). Additional abolition measures ensued, shortly thereafter. Following an announcement on April 1, 1996, all other South Africans, with a few exceptions (members of a medical scheme, and those living in a household earning less than R100 000 per year)

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were allowed access to primary healthcare at public clinics without user charges (McCoy and Khosa 1996; Leatt *et al.* 2006).

The South African policies have received attention in the literature, although from limited sources of information. Utilization of public health services has increased following the 1994 user fee abolition (McCoy and Khosa 1996; Wilkinson *et al.* 1997; Schneider *et al.* 1997; Schneider and Gilson 1999). However, the effect of the 1996 policy has been less clear. Increases in utilization have been observed (Wilkinson *et al.* 2001; Bayat and Cleaton-Jones 2003), as have decreases (Schneider and Gilson 1999; Wilkinson *et al.* 2001).

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

In response to those criticisms, this research makes use of nationally representative data from South Africa that includes information collected before and after the 1996 user fee abolition. The information is available for both a control group and a treatment group, allowing for the application of difference-in-difference (DD) methods. The control group is young children, who had access to free healthcare before the change, as well as after the change. This control group is compared to a set of older children, who did not have access to free public healthcare until after the 1996 policy announcement. Given that the 1996 policy focuses on those without access to a medical scheme (health insurance), and on those living in households with less than R100 000 (1USD=4.35ZAR on April 1, 1996) in earnings, our treatment and control groups are similarly demarcated. The data is taken from the 1995 and 1998 October Household Surveys (Statistics South Africa, 1995, 1998) and were combined in an effort to consider healthcare-seeking behavior for children, focusing on facility choice.

Although the results show that sick and injured children affected by the policy were more likely to be treated than they were before the removal of user fees, that increase could not be pinned to the use of public clinics, where user fees were abolished. Therefore, contrary to most of the available literature related to user fee removal on the African continent, the results suggest that extending free healthcare to a wider section of the more vulnerable members of society had no statistically significant impact on the use of that healthcare.

2 Background

Preceding 1994, South African health policy was racially stratified; four race groups were classified and segregated, and each had its own health department with varying levels of resourcing. Healthcare expenditure in fiscal year 1992

was R122 per capita in the poorest districts, but R437 per capita in the richest districts (McIntyre *et al.* 1995). Unsurprisingly, the result was racially stratified health outcomes and healthcare utilization. In the ten years between 1988 and 1998, infant mortality rates were 11.4 and 53.6 for whites and for Africans in non-urban settings, respectively (Medical Research Council *et al.* 1999).

The new government inherited a fractured health system in need of restructuring. In addition to the racially segregated health departments, there was a system of user charges, although the system did not cover healthcare costs. During the 1992 fiscal year, shortly before fees were abolished, they covered only 4.5% of recurrent expenditure in South Africa (McIntyre 1994), a figure more or less in line with the 5% observed in other African countries (Creese *et al.* 1990). Free health care, covering all services and all children under the age of six, unless they were covered by a medical scheme, was made available at state health facilities, including hospitals, community health centers, clinics, mobile clinics and hospitals, where at least half the expenditure is subsidized by the state (South African Government 1994). The 1996 extension was announced in a budget speech, and, despite limiting coverage to clinics and those from relatively poorer households, was unexpected (McCoy and Khosa 1996; Leatt *et al.* 2006).

In addition to the changes in user charges, the South African government unlocked resources for the health of its citizens. Between fiscal years 1992 and 1998, public resources devoted to the public health sector grew nearly 9% per year (Thomas *et al.* 2000). The fact that increased resources were devoted to the public health sector is important to the analysis for two reasons. The first is that it shows the South African government's commitment to improved health, raising the expectation of some policy success. The second is that it underscores the importance of the DD approach, described below, and the need for observing the treatment and control groups over time.

3 Methodology

The methodology follows the DD estimator (Card and Krueger 1993; Bertrand *et al.* 2004). Unlike in standard DD, based on linear regression, this analysis is subsumed in a multinomial logit regression. The employed structure is dictated by the analysis of healthcare provider choice, and there are four healthcare choice outcomes: No treatment, treatment at a public clinic, treatment at a non-clinic public health facility, and treatment at a private facility. In terms of the policy, since user fees were abolished at public clinics, an increase in public clinic usage is hypothesized. Indirectly, it is also hypothesized that there will be a significant reduction in children not receiving any healthcare, when sick or injured, while the effects at private facilities and non-clinic public facilities should be negligible.

DD analysis is prefaced on fixed-effects panel data estimation, although it can be undertaken with either panel data or repeated cross-section data; the latter is used here. Within DD, it is assumed that there are at least two groups,

and that those two groups can be observed over at least two different points in time; for this analysis, there are exactly two groups and two points in time. The two groups will be referred to as younger (control) children and older (treated) children, because the policy affected these groups differently. The younger children, those under the age of six, are the control group, due to fact that these children had access to free primary health care at public clinics in both 1994 and 1996. On the other hand, children aged six years and older are placed in a treatment group, because these children were not included in the 1994 policy announcement, but were included in the 1996 announcement.

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

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

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

In this analysis, unfortunately, the pre-1996 sample does not have the same characteristics as the post-1996 sample, which, if ignored, could also result in bias. In order to deal with the lack of symmetry across the samples, the DD setting is extended. To account for the asymmetry, time trend effects are included. As there are only two time periods, variable-specific time trends are merely interactions terms based on a time period indicator crossed with each of the control variables. In other words, we control for differences in the subsamples by identifying observations as occurring in one or the other time-period.

Finally, as there are multiple healthcare facility choices, even though the policy benefits are expected only to occur for no treatment and public clinic treatment, the DD representation is set within a multinomial logit framework. However, the parameter estimates from a multinomial regression are not informative about the size of the effect, only the direction. Therefore, effect sizes are calculated from the marginal effect of the DD indicator on each facility choice. The marginal effect of the DD indicator is the estimate of the change in the probability of choosing one of the four facilities. Furthermore, as overall probabilities sum to one, a positive marginal effect for one facility must yield an offsetting decrease, in some way, across the remaining choices.

4 The data

4.1 Data source

Data for the analysis was sourced from two South African October Household Surveys (OHS), one from 1995 and another from 1998. The first of these surveys is the only one available that falls after the initial user fee abolition policy, while the second occurs two years after user fees were abolished for all. Although OHS data also exists for 1996 and 1997, the 1996 survey was deemed too close to the 1996 policy change, while the 1997 survey was missing the necessary health data for analysis.

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

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

As the policy was meant only for those without access to health insurance, and living in households with less than R100 000 in earnings, information on these two variables were needed to limit the sample. Furthermore, given that the initial policy only affected children up to the age of six, this analysis focuses only on children near that age, which was chosen to be up to the age of 11. After limiting the sample, the 1995 sample included 1938 children, while the

1998 sample contained 1168 children. Although not reported here, a previous analysis with children up to the age of 18 was also undertaken, and it led to similar results (thesis reference; cite not included for anonymity). The analysis focuses on the treatment and location, thereof, received by an ill or injured child: (i) an ill or injured child receiving no treatment, (ii) an ill or injured child receiving treatment at a public clinic, (iii) an ill or injured child receiving treatment at a non-clinic public facility, or (iv) an ill or injured child receiving treatment at a private facility.

4.2 Control variables

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

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

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

A number of other variables are included to control for potential opportunity costs of time. We include the distance of the household, in minutes, from the health facility they would most likely use in the event of a need for healthcare; distance is self-reported. Although the distance does not capture resources, it does provide a proxy for the resource use that would be required to treat a sick or injured child. We also include household structure via categorical variables related to the number of children under the age of six (either 0, 1 or more), as well as the number of adults in the household (either 1, 2 or more). We include

two additional measures of adult availability, the proportion of employed adults in the household and the proportion of non-labor force participant adults in the household. We also include the proportion of adults in the household that are union members to control for the ability of employed household members to get time away from work, if a child needs additional attention related to healthcare-seeking activities.

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

The most complex data development process was related to the calculation of earnings. In South African data, it is not uncommon for earnings, either in business or in work, to not be recorded (Simkins 2004). Although a number of options are available, we chose to use values taken from categories listed in follow-up questions. Specifically, if there is no answer to the earnings question, a follow-up question is asked, and in the follow-up, a series of categories or salary ranges are used as prods, in an attempt to uncover earnings information. In this analysis, we used the bottom value in the salary range as the proxy for unreported earnings. Importantly, the choice of bottom, middle, or top of the range did not influence the proportion of children living in households earning more than R100 000 by more than 0.5%, and, thus, using the bottom value from the earnings categories was deemed appropriate.

5 Results

5.1 Data summary

The data is summarized in two tables, in Table 1 and Table A.1, separated by survey year. Table 1 outlines the simple differences in the outcome variables over the survey years. Although no other controls are included, the estimates reported in Table 1 are encouraging, since they suggest increases in the use of public facilities. The robustness of this change will be discussed, below.

Table A.1, on the other hand, describes the data in the two samples. As has been noted, there are a number of differences between the children over the two sample periods, and this must be incorporated into the analysis. To summarize, the data in 1998 is decidedly less urban, and contains fewer Asians and coloured children, but more black children. In 1998, compared to 1995, fewer children are living in the Western Cape, Eastern Cape and KwaZulu Natal, while more children are living in Mpumalanga and Limpopo provinces. More households in

1998 have one child under the age of six, and either one or two adults. Similarly, both the mother and the father are more likely to be alive in the second survey, than the first. Households in 1998 are more likely to live between 30 and 60 minutes from their first-choice health facility, have positive reported income, as well as higher income, own their home, have access to tap water and share a toilet, but less likely to have a flush toilet, than in 1995. However, refuse is less likely to be collected regularly in 1998 than 1995. In 1998, fewer children are living in households, in which, any of the children had gone hungry in the past year. In terms of the adult proportional variables, union membership is higher in 1998, as is the proportion of adults with no formal schooling; however, in the same year, smaller proportions of adults are employed, completed matric or are smokers. These differences across samples led us to control for trends in the control variables within the analysis.

5.2 DD estimates

Two sets of multinomial regressions were estimated. In the first, the variables described in the data section were used to control for potential confounding factors that could influence the choice of healthcare facility. The multinomial logit estimates for this regression can be found in Table B.1. In the second analysis, trends in the control variables were also included. The multinomial logit regression estimates for the second analysis can be found in Table B.2. In order to further limit space, only significant results are presented in Tables B.1 and B.2. The full set of results is available upon request.

However, the primary focus of the analysis is the impact of the 1996 policy change, which was estimated as the marginal effect, at the mean of the data, of the DD indicator for each healthcare utilization outcome in the multinomial logit regression. Regardless of whether or not control variable trends were incorporated, the results support the hypothesis that user fee abolition increased the probability that an ill and injured child would utilize at least some healthcare, by between 5.5% and 6.5%. However, even though children were more likely to receive healthcare, the increase is spread over public clinics, public other (than clinics), and private facilities in such a way that the increases are statistically insignificant. For example, the first panel of Table 2 suggests that public clinic usage increased by an insignificant 1.5%, while other public facility usage increased by an insignificant 4%, and private facility usage increased negligibly. Although the results in the second panel of Table 2 suggest a different pattern, all components of the pattern, again, are statistically insignificant.

In summary, the policy has had a positive effect on overall treatment. Overall treatment has increased, and significantly so for those affected by the 1996 user fee abolition i.e. children aged six years and older. However, it has not had the expected effect on public clinic care. Since user fees were removed for primary care received in public clinics, it was expected that the policy would have led to a significant increase in the use of public clinics amongst ill and injured older children, a result that was not realized in the analysis.

5.3 Discussion

The DD analysis points to a statistically significant policy effect, when comparing treatment to no treatment, and, not, as expected, on the use of public clinics, which were subjected to user fee abolition in 1996. Our results generally disagree with the findings previously reported in the user fee literature, although the literature on the effects of curative care utilization following user fee changes is not extensive. In South Africa, for example, only a few studies are available. Curative care is found to increase amongst children under the age of six, following the 1994 policy change, and there was an observed increase in adult curative care services over the 1992-1998 period (Wilkinson *et al.* 2001; Lagarde and Palmer 2008). Recent analysis, making use of more representative data related to the 1994 policy finds increases in curative care utilization in the range of 5-7% (Koch 2012), a figure much lower than the 44.7%, 77.3% and 300% implied by others (Wilkinson *et al.* 1997; Bayat and Cleaton-Jones 2003; McCoy and Khosa 1996).

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

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

Although the sample employed in this analysis is more representative than the samples employed in a number of other studies, data both before and after the policy change contains a large number of observations, and the analysis is based on an empirical model designed to deal with a variety of confounding factors, there are limitations to the analysis. The 1996 policy followed closely

on the heels of an earlier policy abolishing user fees in the public healthcare sector for young children, nursing and pregnant women and the elderly, which was implemented in 1994. It is possible that the time horizon was too short to uncover an effect. It is also the case that the two samples used for the analysis were not as similar as would have been preferred. However, adjusting for sample differences, through the inclusion of additional control variables, did not alter the substantive conclusions.

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

6 Conclusion

South Africa has availed public health facilities to more individuals by waiving user fees for primary health care, first in 1994, and again, in 1996. Since the 1994 plan focused on young children and older adults, as well as pregnant and nursing mothers, the 1996 change, which waived fees for the remainder of the population, subject to means tests, was examined via DD applied to a subsample of young children. Although the policy provided free primary care to all at public clinics, the results of the analysis do not support the hypothesis that free primary care statistically significantly increased public clinic visits amongst ill and injured children. However, there is strong evidence that ill and injured children were approximately 6% more likely to receive at least some treatment following the change in policy, implying that the policy was indirectly successful.

The empirical results do not support the hypothesis that public clinic utilization would increase, following the abolition of user fees in 1996. Given the lack of empirical support for the hypothesis, coupled with the broad support in the empirical literature for utilization increases associated with the 1994 user fee abolition, further abolishing user fees to more of the population was not an effective option for improving the utilization of public health facilities in South Africa. The result further suggests that barriers to accessing health care for this segment of the population is not user fees; rather, there are other barriers that matter. Therefore, in order to improve the utilization of public health facilities, future research should focus on developing a deeper understanding of healthcare access barriers, and develop policies to address those barriers.

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Table 1. Descriptive Analysis of Outcome Variables

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

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

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

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

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

Table A.1. Descriptive Statistics of Control Variables

VARIABLES	1995	1998	VARIABLES	1995	1998
Survey Year	0.000	1.000	Father Alive	0.881	0.914***
	(0.00)	(0.00)	(=1 if father is known to still be alive)	(0.01)	(0.01)
Treated Group	0.429	0.404	Mother Alive	0.973	0.986*
(=1 if child is at least six years old)	(0.01)	(0.01)	(=1 if mother is known to still be alive)	(0.00)	(0.00)
DD Indicator	0.000	0.404	Distance to Medical Facility: 15-30 mins	0.336	0.343
(=1 if age >=6 & year=1998)	(0.00)	(0.01)	(=1 if household lives 15-30 minutes away)	(0.01)	(0.02)
Urban	0.503***	0.440	Distance to Medical Facility: 30-60 mins	0.195	0.277***
(=1 if lives in urban locale)	(0.01)	(0.02)	(=1 if household lives 30-60 minutes away)	(0.01)	(0.02)
African	0.788	0.867***	Distance to Medical Facility: >60 mins	0.157	0.169
(=1 if black african)	(0.01)	(0.01)	(=1 if household lives at least 60 minutes away)	(0.01)	(0.01)
White	0.030	0.025	Positive Reported Income	0.221	0.590***
(=1 if white)	(0.00)	(0.01)	(=1 if household has reported earnings)	(0.01)	(0.02)
Asian	0.040***	0.021	Own Home	0.468	0.807***
(=1 if Asian)	(0.01)	(0.01)	(=1 if household lives in a home they own)	(0.01)	(0.01)
Coloured	0.141***	0.086	Access to Tap Water	0.585	0.759***
(=1 if mixed race)	(0.01)	(0.01)	(=1 if have tap water in or near house)	(0.01)	(0.02)
No Children <6	0.204	0.188	Access to Flush Toilet	0.405**	0.360
(=1 if no children under 6 in household)	(0.01)	(0.01)	(=1 if have a flush toilet in or near house)	(0.01)	(0.02)
One Child <6	0.424	0.485***	Share Toilet	0.138	0.165**
(=1 if one child under 6 in household)	(0.01)	(0.02)	(=1 if have to share toilet with neighbors)	(0.01)	(0.01)
Two or more <6	0.373**	0.327	Access to Phone	0.170	0.166
(=1 if two children under 6 in household)	(0.01)	(0.02)	(=1 if own or rent phone)	(0.01)	(0.01)
One Adult	0.109	0.152***	Refuse Regularly Collected	0.460***	0.394
(=1 if only one adult in household)	(0.01)	(0.01)	(=1 if refuse is regularly collected)	(0.01)	(0.02)
Two Adults	0.401	0.419***	Log Income	2.058	4.823***
(=1 if two adults are in the household)	(0.01)	(0.02)	(= natural log of reported earnings + 1)	(0.11)	(0.15)
Two + Adults	0.489	0.430	Log Income Squared	19.361	41.358***
(=1 if more than two adults in household)	(0.01)	(0.02)	(= squared value of log income)	(1.08)	(1.44)
Western Cape	0.084**	0.059	Prop Adults in Union	0.040	0.043***
(=1 if lives in Western Cape)	(0.01)	(0.01)	(= proportion of adults belong to labour union)	(0.00)	(0.00)
Eastern Cape	0.208***	0.130	Prop Adults NLFP	0.231	0.288
(=1 if lives in Eastern Cape)	(0.01)	(0.01)	(= proportion adults not participating in work)	(0.00)	(0.01)
Northern Cape	0.046	0.057	Prop Adults Employed	0.366***	0.158

(=1 if lives in Northern Cape)	(0.01)	(0.01)	(= proportion of adults that are employed)	(0.01)	(0.01)
Free State	0.077	0.066	Prop Adults Matric	0.073*	0.066
(=1 if lives in Free State)	(0.01)	(0.01)	(= proportion of adults with matric certificate)	(0.00)	(0.00)
KwaZulu-Natal	0.288***	0.216	Prop Adults No School	0.312	0.384***
(=1 if lives in KwaZulu-Natal)	(0.01)	(0.01)	(= proportion of adults without any schooling)	(0.01)	(0.01)
Northwest	0.101	0.111	Children Hungry (past year)	0.572***	0.437
(=1 if lives in Northwest Province)	(0.01)	(0.01)	(=1 if children ever went hungry in past year)	(0.01)	(0.02)
Gauteng	0.089	0.081	Prop Adult Smokers	0.129***	0.101
(=1 if lives in Gauteng Province)	(0.01)	(0.01)	(= proportion of adults that are smokers)	(0.00)	(0.00)
Mpumalanga	0.079	0.132***			
(=1 if lives in Mpumalanga Province)	(0.01)	(0.01)			
Limpopo	0.027	0.148***			
(=1 if lives in Limpopo Province)	(0.00)	(0.01)	Observations	1,938	1,168

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

Table B.1. Multinomial Regression Results without Year Interactions

VARIABLES	Model 2			Model 3			Model 4		
	Pub Clinic	Pub Other	Private	Pub Clinic	Pub Other	Private	Pub Clinic	Pub Other	Private
Survey Year	-0.339**	-0.995***	-0.506***	-0.198	-1.032***	-0.486***	-0.243	-1.013***	-0.371*
	(0.15)	(0.17)	(0.17)	(0.18)	(0.19)	(0.18)	(0.20)	(0.21)	(0.22)
Older Children	-0.671***	-0.671***	-0.665***	-0.674***	-0.674***	-0.658***	-0.705***	-0.692***	-0.639***
	(0.15)	(0.15)	(0.17)	(0.15)	(0.15)	(0.17)	(0.15)	(0.15)	(0.17)
Free Care Older Children	0.296	0.387*	0.275	0.311	0.415*	0.280	0.315	0.394*	0.291
	(0.21)	(0.22)	(0.24)	(0.21)	(0.22)	(0.24)	(0.21)	(0.22)	(0.24)
Free State	-1.054***	-1.078***	0.322	-1.123***	-1.087***	0.318	-1.111***	-1.081***	0.306
	(0.37)	(0.34)	(0.34)	(0.37)	(0.35)	(0.35)	(0.37)	(0.35)	(0.35)
Northwest	0.479	0.002	0.676*	0.503	0.080	0.767**	0.512	0.080	0.751**
	(0.35)	(0.33)	(0.36)	(0.35)	(0.34)	(0.37)	(0.35)	(0.34)	(0.37)
Mpumalanga	0.345	-0.272	0.845**	0.376	-0.215	0.892**	0.383	-0.189	0.867**
	(0.35)	(0.34)	(0.36)	(0.36)	(0.35)	(0.37)	(0.36)	(0.35)	(0.37)
Urban	-0.039	0.190	0.414***	-0.047	0.214	0.149	-0.084	0.189	0.169
	(0.14)	(0.15)	(0.16)	(0.21)	(0.22)	(0.23)	(0.21)	(0.22)	(0.23)
Asian	-1.107**	-0.122	0.461	-0.997**	-0.180	0.314	-0.866*	-0.154	0.309
	(0.45)	(0.34)	(0.33)	(0.46)	(0.35)	(0.34)	(0.47)	(0.35)	(0.35)
White	-1.232**	0.109	0.961***	-0.998*	0.019	0.795**	-0.766	0.040	0.758**
	(0.50)	(0.36)	(0.34)	(0.53)	(0.39)	(0.37)	(0.55)	(0.40)	(0.38)
Two Adults	-0.350*	-0.144	-0.368*	-0.345*	-0.184	-0.379*	-0.312	-0.181	-0.423*
	(0.19)	(0.21)	(0.22)	(0.20)	(0.21)	(0.22)	(0.20)	(0.22)	(0.23)
Two + Adults	-0.509***	-0.522**	-0.692***	-0.458**	-0.552***	-0.688***	-0.395*	-0.529**	-0.689***
	(0.19)	(0.21)	(0.21)	(0.19)	(0.21)	(0.22)	(0.21)	(0.23)	(0.24)
Med Facility: > 60 mins	-0.568***	0.114	-0.372	-0.618***	0.123	-0.321	-0.635***	0.147	-0.305
	(0.20)	(0.21)	(0.23)	(0.21)	(0.21)	(0.23)	(0.21)	(0.21)	(0.23)
Own Home				-0.299**	-0.159	-0.143	-0.291**	-0.148	-0.151
				(0.14)	(0.14)	(0.15)	(0.14)	(0.14)	(0.15)
Toilet Shared				-0.072	0.061	0.337*	-0.070	0.075	0.328*
				(0.18)	(0.18)	(0.19)	(0.18)	(0.18)	(0.19)
Prop Adults Matric							-1.936***	-0.321	0.693
							(0.57)	(0.49)	(0.48)
Observations	3,106			3,106			3,106		

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

Table B.2. Multinomial Regression Results with Year Interactions

VARIABLES	Model 2			Model 3			Model 4		
	Pub Clinic	Pub Other	Private	Pub Clinic	Pub Other	Private	Pub Clinic	Pub Other	Private
Year	0.519	0.005	-2.622*	0.139	-0.088	-3.541**	0.472	0.177	-3.130*
	(1.40)	(1.31)	(1.58)	(1.47)	(1.38)	(1.68)	(1.55)	(1.42)	(1.76)
Group	-0.616***	-0.630***	-0.766***	-0.632***	-0.642***	-0.789***	-0.641***	-0.660***	-0.754***
	(0.15)	(0.16)	(0.19)	(0.15)	(0.16)	(0.19)	(0.16)	(0.16)	(0.19)
Treated	0.154	0.280	0.528*	0.195	0.310	0.617**	0.118	0.281	0.558*
	(0.25)	(0.27)	(0.29)	(0.25)	(0.27)	(0.30)	(0.25)	(0.27)	(0.30)
Free State	-1.063***	-0.647*	0.512	-1.112***	-0.717*	0.563	-1.099***	-0.698*	0.532
	(0.40)	(0.36)	(0.38)	(0.40)	(0.38)	(0.39)	(0.40)	(0.37)	(0.39)
Northwest	0.472	0.442	0.823**	0.471	0.485	0.881**	0.502	0.489	0.869**
	(0.37)	(0.35)	(0.39)	(0.38)	(0.36)	(0.40)	(0.37)	(0.36)	(0.40)
Urban	0.060	0.242	0.355*	0.134	0.343	0.175	0.124	0.345	0.247
	(0.17)	(0.18)	(0.20)	(0.24)	(0.25)	(0.28)	(0.24)	(0.25)	(0.28)
Asian	-1.076*	0.060	0.871**	-0.904	0.045	0.719*	-0.894	0.017	0.665
	(0.57)	(0.40)	(0.40)	(0.58)	(0.41)	(0.41)	(0.60)	(0.41)	(0.42)
White	-1.785***	0.096	1.038**	-1.519**	0.036	0.877*	-1.437**	0.023	0.828
	(0.66)	(0.43)	(0.44)	(0.69)	(0.46)	(0.48)	(0.73)	(0.48)	(0.50)
One Child <6	-0.223	-0.310	-0.453*	-0.221	-0.308	-0.501**	-0.183	-0.268	-0.480*
	(0.23)	(0.22)	(0.25)	(0.23)	(0.22)	(0.25)	(0.25)	(0.25)	(0.27)
Two + Adults	-0.586**	-0.598**	-0.676**	-0.524**	-0.599**	-0.651**	-0.545*	-0.594*	-0.652**
	(0.27)	(0.28)	(0.31)	(0.27)	(0.28)	(0.31)	(0.29)	(0.30)	(0.33)
Med Facility: 30-60 mins	-0.108	0.143	-0.575**	-0.141	0.110	-0.499*	-0.155	0.121	-0.484*
	(0.21)	(0.23)	(0.26)	(0.22)	(0.23)	(0.27)	(0.22)	(0.23)	(0.27)
Med Facility: > 60 mins	-0.608***	0.085	-0.389*	-0.654***	0.100	-0.341	-0.653***	0.131	-0.326
	(0.20)	(0.21)	(0.23)	(0.21)	(0.21)	(0.23)	(0.21)	(0.22)	(0.23)
Own Home				-0.275*	-0.099	-0.238	-0.229	-0.068	-0.254
				(0.16)	(0.16)	(0.18)	(0.16)	(0.16)	(0.18)
Toilet Shared				-0.033	0.092	0.366*	-0.027	0.113	0.355*
				(0.18)	(0.18)	(0.19)	(0.18)	(0.18)	(0.19)
Prop Adults Matric							-1.896***	-0.311	0.704
							(0.58)	(0.49)	(0.49)
Year*Black	-0.416	0.046	0.872*	-0.397	0.201	1.111**	-0.681	-0.035	1.062*
	(0.68)	(0.58)	(0.50)	(0.71)	(0.61)	(0.54)	(0.74)	(0.60)	(0.55)
Year*One Child Under 6	-0.145	0.029	1.076***	-0.159	0.049	1.163***	-0.289	0.082	1.122**
	(0.36)	(0.37)	(0.41)	(0.36)	(0.38)	(0.41)	(0.38)	(0.40)	(0.44)

Year*Two Adults	-0.212	-0.108	-0.558*	-0.273	-0.139	-0.564*	-0.220	-0.025	-0.576*
	(0.26)	(0.28)	(0.29)	(0.27)	(0.28)	(0.29)	(0.28)	(0.29)	(0.30)
Year*Western Cape	-0.168	1.485**	0.665	-0.014	1.558**	0.581	0.000	1.629**	0.455
	(0.67)	(0.64)	(0.71)	(0.70)	(0.65)	(0.72)	(0.70)	(0.66)	(0.73)
Year*Eastern Cape	0.393	0.251	-0.239	0.698*	0.437	0.056	0.590	0.401	-0.009
	(0.37)	(0.40)	(0.43)	(0.38)	(0.41)	(0.45)	(0.39)	(0.42)	(0.44)
Year*Mpumalanga	1.205***	0.978*	0.912*	1.182**	0.981*	0.992**	1.110**	0.978*	0.974*
	(0.46)	(0.50)	(0.48)	(0.47)	(0.51)	(0.49)	(0.48)	(0.52)	(0.50)
Year*Med Facility:30-60 mins	0.005	0.086	0.793**	0.018	0.143	0.808**	0.073	0.131	0.812**
	(0.28)	(0.30)	(0.33)	(0.29)	(0.30)	(0.33)	(0.29)	(0.30)	(0.33)
Year*Access to Tap Water				0.884***	0.635*	0.678	0.963***	0.684*	0.763*
				(0.34)	(0.38)	(0.44)	(0.34)	(0.38)	(0.44)
Year*Prop Adults Employed							-1.855*	-2.103*	-1.044
							(1.05)	(1.12)	(1.01)
Year*Children Hungry (past year)							0.762***	0.376	0.277
							(0.25)	(0.27)	(0.29)
Observations	3,106			3,106			3,106		

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