Does Abolishing User Fees Lead to Improved Health Status? Evidence from Post-Apartheid South Africa

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Abstract

Whether user fees for basic health services should be charged or abolished for the poor has recently been debated. This study examines the impact on child health status of removing user fees in South Africa. Our main innovation is to exploit plausibly exogenous variation in access to free healthcare, due to the fact that black Africans under apartheid could exercise little political power and residential choice. By looking at *ex ante* similar children, we find substantial improvements in health status among children. Falsification exercises confirm no preexisting trend in the pre-reform period or no treatment effect among non-eligible children in the post-reform period.

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Whether the poor should have free access to basic health services and products or should be charged fees has recently become the subject of contentious debate in the field of development policy. User fees and cost-sharing have been adopted as a primary means to achieve financial sustainability. Further, those who advocate pricing appeal to the theoretical predictions of basic price theory that charging even small fees should enhance the efficient allocation of goods and services because: (1) prices help target the population in need of a good or service, as consumers whose valuation is lower than the given price can be screened out (Oster 1995); and (2) pricing promotes higher usage, as paying a price encourages consumers to use a good or service more due to one of two psychological effects -- that of having already incurred sunk costs (Thaler 1980) or that of believing higher prices correlate to better quality (Bagwell and Riordan 1991).

Recent empirical studies based on randomized controlled trials (RCTs), on the other hand, provide evidence against all these predictions. First, introductions of even small fees dramatically reduced take-up of deworming drugs distributed at Kenyan elementary schools (Kremer and Miguel 2007), of insecticide-treated bed nets (ITNs) among pregnant women in Kenya (Cohen and Dupas 2010), and of water disinfectant among poor households in Zambia (Ashraf, Berry, and Shapiro 2010), leading to little revenue raised to justify administrative costs. Further, these studies show that pricing screens out poor individuals who are in most need of these products, i.e., pregnant women for ITNs, and households with young children for water disinfectant. Moreover, those who are willing to pay higher prices are no more likely to utilize the health products at a higher rate. Overall, these studies provide compelling evidence that "the price is wrong" – cost-sharing is an inequitable and inefficient means of distributing basic health products relative to free distribution (Holla and Kremer 2009; J-PAL 2011).

However, whether user fees should be charged or abolished for health *services* remains an open question with significant international implications. During the 1980s, user fees were adopted in nearly all African countries for patients receiving health services. However, many countries, as well as international organizations, have come to the realization that "to resist the temptation to rely on user fees" (WHO 2008) is a best practice in the effort to achieve universal access to healthcare, as targeted by a Millennium Development Goal.[1] Accordingly, many African countries have removed user fees during the last decade.[2] However, empirically determining the causal impact of free health services on access to healthcare or on health outcomes is impeded by prevailing identification problems that any cross-country studies between countries with and without user fees are severely biased by unobserved heterogeneities across countries, whereas time-series analyses based on the introduction of free services within a country suffer from omitted concurrent changes.[3] Further, such a policy is usually simultaneously introduced at the national level, making it infeasible to exploit variations across time and space within a country. In addition, RCTs, such as randomly providing vouchers to households, do not allow researchers to account for the implications of an important tradeoff between user fees and quality of services or incentives of healthcare providers.[4]

In this paper, we examine the effect of abolishing user fees from health services on nutritional status among poor children in South Africa. Our main innovation is to exploit plausibly exogenous variation in access to free healthcare

[1] For example, Asfaw et al. (2004) show that user fees adopted as a means of health care financing in Ethiopia substantially crowded out the poorest population out of the health care market. Also, James et al. (2005) simulate and conclude that removing user fees comes at a low cost but saves approximately 233,000 deaths annually in children under age 5 in 20 African countries.

[2] Although the scope of application of the abolition varies across countries, user fees have been abolished in South Africa in 1994, Uganda in 2001, Madagascar in 2002, Senegal in 2006, Zambia in 2006, Burundi in 2006, Niger in 2006, Liberia in 2007, Kenya in 2007, Ghana in 2008, Lesotho in 2008, and Sudan in 2008. See Gilson and McIntyre (2005), Meessen et al. (2009), and Yates (2009) for descriptions of each case.

[3] For example, Deininger and Mpuga (2005) and Xu et al. (2005) examine the impact of abolition of user fees in Uganda by comparing health outcomes before and after the policy change in 2001, yet without cross-sectional variation, whose estimates are likely to be biased by unobserved changes, especially because user fees were removed 10 days before an election.

[4] While providing health insurance to the poor has effects similar to eliminating user fees at least partially, it still does not address incentives in supplying quality services. We refer readers interested in the effects of health insurance to Harris and Sosa-Rubi (2009), King et al. (2009), Yates (2009), and Anderson, Dobkin, and Gross (2012).

among children with *ex ante* similar characteristics. Extreme domination by whites over the allocation of resources and limited mobility of black Africans under the apartheid regime created plausibly exogenous variation in availability of clinics across communities unrelated to the characteristics of black Africans, leading to little correlation between the two.

In 1994, the new democratic government implemented a new reproductive policy that removed user fees from healthcare to pregnant women and children under 6 years old. The introduction of free healthcare led to substantial increases in access for black Africans, more than 70 percent of whom previously identified user fees as the major reason for forgoing treatment. Households in communities with clinics (hereafter the "high-treatment region") were affected more by the abolition of user fees because they gained immediate access to health services, whereas those in communities without a clinic (hereafter the "low-treatment region") were less affected by the change, since they continued to have poor access to health clinics or had to wait until clinics were built.[5] This heterogeneity enables us to apply a difference-in-differences (DID) strategy, which compares the changes in nutritional status between the communities with and without health clinics, before and after the policy change.

Our results shed light on immediate and significant returns of free health services as evidenced by improved child nutritional status. The point estimates suggest that free health services increased average weight-for-age z-scores (WAZ) of newborns by 0.64 standard deviations, indicating that pre- and post-natal services are important determinants of health status. Further, free health services helped improve health status among children who were already born in 1993 at low health status and who were at least partially exposed to the policy after 1994. Although their average WAZ was -0.55 in 1993, it increased by 0.57 stand-

[5] Throughout the paper, we use "community" to refer to a census enumerator subdistrict, and "region" to refer to a group of communities defined by whether there was a clinic or not as of 1993.

ard deviations, indicating that free healthcare entirely filled a gap in health status as measured by WAZ between South African children and the reference population. While the magnitudes of the effects are similar when nutritional status is measured by weight-for-height z-scores (WHZ), the positive effects disappear when using height-for-age z-scores (HAZ), and thus our analysis finds effects limited in the short-term period.[6]

Of particular interest is that our findings highlight the potential role of gender differences in the effect of free healthcare. This is more evidence in WAZ; boys attained 0.97 higher WAZ at 0 to 3 years old, and 1.05 higher WAZ at 5 to 8 years old, and these estimates are significantly different from zero. In contrast, the estimated effect for girls is smaller.

The estimated effects might be spurious if there were intrinsic differences between the high- and low-treatment regions. We conduct a number of robustness checks to rule out alternative mechanisms leading to biases in pre- or post-reform period. Specifically, we find little association between child/household attributes and the treatment status in the baseline observations, suggesting no evidence of preexisting trends in the pre-treatment period. Further, we find little, or even negative, treatment effect among non-eligible children aged 6 years old and above as of 1994, indicating the capacity constraint or quality deterioration at existing clin-

[6] Z-scores are calculated by subtracting the median value of the reference population from the observed value and dividing it by the standard deviation value of the reference group for each age and gender. The reference group is typically well-nourished U.S. children. WHO argues that the z-score system is the best and most appropriate system to utilize anthropometric data to measure malnutrition, health, and nutritional status (de Onis and Blössner 1997). Among them, WAZ and WHZ are often considered to capture short-term fluctuations in nutritional status, while HAZ is rather used to measure the nutritional status in the long-term period. The long-term impacts of the health police are beyond the scope of this paper, as we trace health growth only over five years. However, a growing body of literature shows that health status, particularly early childhood nutrition, has substantial long-term and irreversible economic impacts on later outcomes. It has been shown that children who experienced positive (negative) health shocks during early childhood perform better (worse) in school, earn higher (lower) income during young adulthood, and achieve higher (lower) health status and socioeconomics status up to middle age. The literature along with the fetal origins hypothesis is summarized in Almond and Currie (2011). Further evidence on the long-term impacts of early-life health status is found in Alderman et al. (2001), Glewwe, Jacoby, and King (2001), Glewwe and King (2001), Case, Lubotsky, and Paxson (2002), Case, Fertig, and Paxson (2005), Alderman, Hoddinott, and Kinsey (2006), Almond (2006), Bleakley (2007), Dinkelman (2008), Yamauchi (2008), and Maccini and Yang (2009).

ics, and thus bias by unobserved concurrent changes, if any, goes against our estimates.

The rest of the paper is structured as follows. The following section provides policy background, and section II describes the data and summary statistics. The identification strategy and empirical results are presented in section III for newborns and section IV for children already born at the time of the policy reform, and section V discusses validity and robustness checks. Section VI concludes.

I. New Reproductive Health Policy in South Africa

The South African apartheid regime was one of the most discriminatory in the world. Geographical segregation by the Bantu Authorities Act of 1951 forced black Africans to move into underdeveloped and infertile areas, called "homelands," where little infrastructure existed. Almost all resources were controlled by the white minority in urban areas without representation from the black majority. This resulted in a fragmented health system; prior to 1994, there was neither a comprehensive health policy nor a central institution to coordinate health plans and practices at the national level. Under these circumstances, black Africans constituted the most disadvantaged and underserved group; their access to health services was constrained by costly out-of-pocket payments for health services as well as by a lack of facilities, doctors, and medicines in the public health sector (Department of Health 1999).

Pressure from the international community accelerated the negotiation toward ending apartheid, culminating in the first democratic election in April 1994. A new government led by President Nelson Mandela focused on eliminating substantial racial and geographic inequalities. Most notably, it included the abolition of user fees from health services to pregnant women and children under 6 years old at public facilities (Republic of South Africa 1994). Free services to pregnant women included pre-and post-natal care from confirmation of pregnancy until 42 days after delivery, and all health services to children under 6 years old became free. The policy was implemented as early as June, 1994.

The free healthcare provision contributed to a significant increase in access to health services among black Africans, who had previously identified the cost of medical services as the major barrier to seeking treatment. A national survey of households conducted in 1993 by the Community Agency for Social Enquiry (CASE) (1995), found “not affordable” to be the most common reason given by black families for forgoing health care (73.8 percent), followed by lack of transportation (11.5 percent). This contrasts with the figures among whites, only 23.2 percent of whom listed cost as a primary constraint. In addition, 90 percent of black Africans did not have health insurance, whereas 76 percent of whites did. Various studies have found that removing user fees substantially improved delivery of pre-natal care to women not previously reached and increased the number of patients under 6 years old (McCoy 1996; Department of Health 1998; Schneider and Gilson 1999; Wilkinson et al. 2001; Cooper et al. 2004; Morestin and Ridde 2009).[7]

However, the benefits of increased access to public health care is fundamentally limited by the continuing financial instability due to the budgetary resource constraint, accompanied by the introduction of free health services, leading to public dissatisfaction with declining quality of services and the low morale of health providers (Gilson et al. 1999). The central issue in financing free health

[7] The dearth of data leads to limited evidence on the types of services that responded to the policy change more than others. The Integrated Nutritional Programme tells us some examples of types of services provided because some of its services overlapped with the services through the free primary care services, including maternal nutrition, breastfeeding assistance, nutrition education, child immunization, growth monitoring, nutritional promotion from infants through adolescents, and micronutrient supplementation. We discuss their importance in Online Appendix V. Other than that, the health system under apartheid was historically biased towards curative services. Available evidence suggests that the increased utilization of health services focus on women receiving prenatal care (McCoy 1996). Wilkinson et al. (2001) shows an increase in curative services and care of children aged under 6 years old i.e., immunization and growth monitoring, in the Hlabisa health district at the time of policy change yet only for a short period. In Online Appendix II, we provide further evidence on the effect of free healthcare services on utilization.

services was to address inequalities in the allocation of budgetary resources across geographical areas. The primary source of the health care funds was generated through the collection of taxes. Prior to 1994, the absence of coordination and integration among as many as eighteen Departments of Health at the central and regional levels created disparities in allocating health resources across and within provinces. The reform in financing policy was necessary to unify the decision process (initially at the Department of Health and later at the Department of Finance) and develop new formula to ensure equity and sustainability of resource distribution to historically under-resourced areas. Initially, the Department of Health adopted the distribution mechanism based on the needs at the provincial levels in an effort to distribute more resources to the poor areas. However, it was later replaced by unconditional grants determined by the Department of Finance, in light of a new fiscal system based on the new Constitution in 1996.[8] However, the inequality in health resource allocations across and within the provinces continued (Gilson et al. 1999).

II. Data Source and Descriptive Statistics

To estimate the impact of free healthcare provision on health outcomes among poor children, we use the data from KwaZulu-Natal Income Dynamic Study (KIDS), a longitudinal household survey in the KwaZulu-Natal province of South Africa.[9]

The first wave in 1993 (hereafter KIDS93) is part of the 1993 Project for Statistics on Living Standards and Development (PSLSD), the first comprehen-

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[8] Previously, the health budgets were centrally determined and reallocated to provinces by the Health Function Committee. However, the new system following the establishment of the new Constitution allowed the provincial governments to determine the allocation of budgets, which they received from the central governments, across sectors. The latter system created some inequalities in health budgets (McIntyre, Baba, and Makan 1998).

[9] The surveys were jointly conducted by the University of KwaZulu-Natal, the University of Wisconsin-Madison, the International Food Policy Research Institute, the London School of Hygiene & Tropical Medicine, and the Norwegian Institute of Urban and Regional Studies. A more detailed description of KIDS is provided in May et al. (2000) and May et al. (2007).

sive household survey in South Africa undertaken in the last half of 1993. To address dramatic changes in South African society since 1994, African and Indian households of the PSLSD sample in the KwaZulu-Natal province were resurveyed in 1998 (hereafter KIDS98). As the largest province in South Africa, KwaZulu-Natal accounted for approximately 9.5 million of the total national population of 40 million in 2001 and shares many characteristics with other former homelands, such as high rates of poverty and lack of basic services. (Klasen 1997; Leibbrandt and Woolard 1999; May et al. 2000). The original sample contains 1,389 households (215 are Indian and 1,174 are Africans), of which 1,178 households (85 percent) were followed up on in 1998. The attrition rate is lower than other related studies and thus of limited concern (Carter and Maluccio 2003).[10]

KIDS provides us with several analytical advantages. First, the household survey contains all relevant information necessary for the analysis: extensive demographic and socioeconomic status information as well as anthropometric data for all children aged 6 years old and below in KIDS93, and for all children aged 12 years old and below in KIDS98. The subsequent main analysis reports estimates based on WAZ, while we provide a companion set of estimates using WHZ and HAZ in Online Appendix IV.[11]

Second, the community survey, which we merge with the household survey using unique identification codes, provides information on the availability of health and other types of infrastructure across communities. In this paper, we fo-

[10] Also in Online Appendix V, we provide evidence that attrition is not systematically correlated with the treatment status.

[11] In the literature, three anthropometric indices are widely used to measure nutritional status: weight-for-age z-score (WAZ), weight-for-height z-score (WHZ), and height-for-age z-score (HAZ). WAZ and WHZ measure short-term fluctuations in nutritional status, while HAZ is associated with the long-term cumulative effects of nutrition such as stunting. In this paper, we use WAZ to focus on short-term effects, because our post-reform period is only four years after the policy implementation and because we focus on early childhood during which height does not differ much. It is in general known that stunted children take much longer to catch up in growth. For example, Bobonis, Miguel, and Puri-Sharma (2006) find evidence consistent with this in that iron supplementation and deworming drugs affect only WAZ among preschool children but not HAZ. Also, Walsh, Dannhauser, and Joubert (2002) also find significant improvements only in WAZ and not in HAZ two years after they implemented a community-based nutrition education program in the Free State and Northern Cape Provinces. Another concern with height is that it is subjected to large measurement error particularly for infants; Glewwe, Jacoby, and King (2001) find that about 52 percent of the total variance in HAZ, even for children at the time of school enrollment, is caused by measurement error. Nonetheless, in Online Appendix IV, we repeat the main analysis using WHZ and HAZ and discuss similarities and disparities in results.

cus on clinics as the primary healthcare facility providing health services to pregnant women and children. This reflects evidence that black Africans visited public clinics for pre-natal services (69 percent) and for initial treatments (40 percent), whereas whites sought private doctors for pre-natal care (79 percent) and for initial treatments (74 percent) in 1993 (CASE 1995; see also Maharaj and Cleland 2005).

Third, the timing of data collection suits the purposes of our study; comparison of the two waves, in 1993 and 1998, captures changes in child health status from just before to after the policy change. In addition, the nature of the data allows us to trace the same individuals across time. This enables us to circumvent identification problems, which we describe below, posed by previous studies, a large majority of which have relied on cross-sectional data.

For the purposes of the present study, the samples are restricted to black Africans, not only because they constitute the majority of the South African population but also because they account for most of the poor. Because the new government aimed at protecting and equalizing opportunities for the most disadvantaged group, black Africans are most likely to have benefited from the new health policy. Further, the samples used in the subsequent analyses are limited to households with children only.

Table 1 presents baseline summary statistics on the individual health status and a variety of household and community characteristics using the sample in KIDS93. We define the treatment intensity based on the availability of health clinics in the community. Each community had either zero (low-treatment region) or one clinic (high-treatment region) as of 1993. Those in the high-treatment regions gained immediate access to health services as soon as the free provision of healthcare began. On the other hand, families in the communities without a clinic needed either to wait until facilities became available, or to travel long distances to receive treatments in different communities. The second column presents figures from the high-treatment region (26 communities), whereas the third column focuses on the low-treatment region (28 communities), and the last column shows the difference in means between the two regions for respective variables.

The most striking fact revealed from the table is that none of the individual or household characteristics, except household size, differs significantly between the two regions. Notably, anthropometric indicators, such as WAZ, WHZ, and HAZ, are similar across the regions, despite the difference in proximity to health clinics, suggesting that children in the high-treatment region were no better situated to utilize health services. This is consistent with the fact that black Africans had severely limited access to healthcare due to high payment costs. Similarities in monthly income and parental educational attainment between households in the low- and high-treatment regions not only indicate that proximity to clinics does not simply proxy rich versus poor areas or urban versus rural areas, but more importantly that black families had limited ability to determine the extent of infrastructure in their communities, both of which provide support to the critical identification assumption that the allocation of clinics among black communities was plausibly orthogonal to family attributes.

There is some evidence that communities in the high-treatment region are more likely to have other types of social facilities, such as hospitals and schools (not shown). Consequently, cross-sectional estimates of the relationship between health facilities and health status may confound unobservable differences in communities and may involve omitted variable bias. Section V discusses our challenge to develop an appropriate counterfactual for health status among children in the absence of policy change.

III. Effect on Newborns

The first question posed in this study is whether free pre- and post-natal care leads to improved health status among newborns. We present our empirical strategy in subsection A, followed by empirical results in subsection B.

A. Empirical Strategy

The econometric framework is established to compare health improvements among newborns between the two regions before and after the policy change. Consequently, we make use of treatment status as defined by two dimensions of variation. The first variation is a function of birth timing. Because user fees were abolished in 1994, any children observed in KIDS93 had not yet been exposed to the policy. On the other hand, all children born after 1994 in KIDS98 were eligible for free health services for their entire lives. Thus, the sample includes children aged 0 to 3 in KIDS93 and the same age group in KIDS98.[12] The second variation across individuals within the same cohort is determined by the residential community: whether the household lived in the high- or low-treatment region as of 1993. This leads to the baseline regression:

$$W_{ihct} = \beta_0 + \beta_1(High_c \times Post_t) + \beta_2 High_c + \beta_3 Post_t + \boldsymbol{X}'_h\beta_4 + (\boldsymbol{X}'_c \times Post_t)\beta_5 + \varepsilon_{ihct} \qquad (1)$$

where W_{ihct} is the weight-for-age z-score of individual i, in household h, in residential community c, in survey year t. $Post_t = 1$ for observations from KIDS98 (post-reform observations), while $Post_t = 0$ for observations from KIDS93 (pre-reform observations). $High_c = 1$ if community c had a clinic in 1993 (the treatment group), and is otherwise equal to 0 (the control group). $\mathbf{X}_h$ is an additional vector of household characteristics, $\mathbf{X}_c$ contains a vector of community character-

[12] Although technically all children across ages in KIDS93 can serve as pre-reform observations, we focus on only the same age groups that were fully exposed in KIDS98. This corresponds to children aged 0 to 3 in 1998, and thus we use only children aged 0 to 3 in KIDS93 as the corresponding pre-reform observations. Moreover, we do not consider children aged 4 in 1998 as the fully-exposed cohort because this cohort was not entitled to prenatal services (See the figure in Online Appendix I that clarifies the timing and exposure of each cohort to the free healthcare policy).

istics, and ε_{ihct} is an unobserved individual disturbance.[13] $\mathbf{X}_c$ is interacted with a post-reform dummy because it takes into account of other types of health facilities in 1993 and immunization campaigns since 1993 that may be correlated with the existence of clinics and that may potentially confound effects on health outcomes in 1998. This helps avert mistakenly picking up an effect by other governmental programs, such as immunization campaigns provided at clinics. The parameter of interest is β_1, which represents the difference-in-differences effect on health improvements among children in the high-treatment region relative to children in the low-treatment region. Since children in the low-treatment region could still receive free health services if they visited a clinic in a neighboring community or once a clinic was built in their community, the estimated impact is likely to be understated.[14]

To further refine variations across cohorts and communities, the main strategy employed is:

(2) $$W_{ihcl} = \delta_0 + \delta_1(High_c \times Post_l) + \boldsymbol{X}'_h\delta_2 + (\boldsymbol{X}'_c \times Post_l)\delta_3 + \eta_c + \psi_l + \varepsilon_{ihcl}$$

where η_c is the fixed effects of a residential community in which the household resided in 1993, and ψ_l is cohort fixed effects defined by the year of birth.[15] $Post_l = 1$ for the post-reform cohorts (cohorts aged 0 to 3 in KIDS98). All standard errors are clustered at the community level. The coefficient of interest, δ_1, is expected to be positive if abolishing user fees led to improved child health status. The effect can be interpreted as causal based on the identification assumption that, after controlling for the birth timing and residential location, the intensity of the

[13] Although it is important to control for these covariates, the key assumption that the availability of clinics is exogenous to black families suggests that β_1 can be estimated even without these controls. However, their inclusion enhances the precision of the estimates. Thus, the main analysis presents the results with and without controls, and similarities in estimates across these specifications provide evidence to support the validity of the identification assumption.

[14] With this identification strategy, we implicitly assume the linearity or moderate concavity, if not high concavity, in the relationship between improved access (i.e., increased number of visits to clinics) and health improvements. However, this may not be the case. For example, it may be plausible to assume a high degree of concavity if there is a high diminishing marginal return to healthcare services, as the number of visits increases. However, since we do not directly observe utilization, we are unable to estimate non-linear impacts. Since children in the low-treatment region were also likely to have experienced some improvements in access, a small increase in utilization may have brought about substantial improvements in health status. In this sense as well, our estimation provides the lower bound of the policy impact.

[15] We use the child's age to identify their birth year, and thus a cohort refers to children at the same age in the survey.

exposure to the policy change is exogenously determined. The potential threat is to confound any other changes between pre- and post-reform periods that are correlated with the availability of clinics and that have direct impacts on health. We discuss these issues in greater detail in Section V.

B. Empirical Results

A simple two-by-two matrix in Panel A of Table 2 illustrates our identification strategy, laid out by equation (1). As described above, we compare the health improvements from children aged 0 to 3 in KIDS93 (pre-reform observations) to the same age group in KIDS98 (post-reform observations), across the regions. Health status is indistinguishable in the pre-reform period between the regions. While health status improved in both regions, the improvement was significantly higher in the high-treatment region by 0.522 in WAZ. Notably, WAZ becomes positive in the high-treatment region, indicating that the gap in mean health status between black children in South Africa and the reference U.S. population is completely filled.

Table 3 presents the regression results.[16] Column (1) presents the benchmark result based on equation (1), which is analogous to Panel A of Table 2. Column (2) provides results from equation (2), where community fixed effects absorb effects through time-invariant unobserved differences across communities, and where cohort fixed effects control for factors common to each cohort as well as the main post-period effect. Consequently, the estimated effects are purged of all characteristics that are permanent in communities or common to cohorts. Controlling for community fixed effects is important because there is some evidence that the high-treatment region is likely to have other sorts of infrastructure, while controlling for cohort fixed effects is crucial, because this was a period of dramatic

[16] In Online Appendix III, we explore health improvements by cohort between 1993 and 1998 without distinction between the high- and low-treatment regions. The findings illustrate that most cohorts affected by the health policy have improved health status over time, relative to non-affected cohorts.

change in the whole society. With these fixed effects, the coefficient of interests increases in magnitude, and the explanatory power increases substantially. Columns (3) and (4) additionally include various household or community characteristics. Note that although these variables are important in themselves, their inclusion does not alter the DID estimates, which provides credibility to the interpretation that the availability of clinics in 1993 is not related to any of these background characteristics. The preferred estimate in column (5) suggests that after controlling for all these variables, removing user fees improved the health status of newborns by 0.64 standard deviation in terms of WAZ. This highlights the particular importance of pre- and post-natal services.

Estimates from other control variables indicate that income and immunization campaigns are strongly associated with health status, though these two variables are known to be endogenous to other factors. The negative impact of the number of other health facilities may appear odd, yet is consistent that communities with initially having more health facilities had built a lower number of health facilities by 1998. We have little evidence that parental education or household size is significantly associated with health status.

With respect to gender, it is widely observed in developing countries that most advantages are enjoyed only among boys.[17] To assess this, the sample focuses only on boys in Panel A of Table 4 and on girls in Panel B. The results reveal that the positive effects found in Table 3 are driven mostly by boys; the point estimates are substantially larger than the previous estimates and are highly significant. On the other hand, the magnitudes of effects, while consistently positive, are

[17] Sizeable literature has documented the gender bias in the effect of social services in developing countries in various contexts. For example, Duflo (2003) finds the effect of a large pension program in South Africa to be much smaller for boys than for girls when the pension recipient is a woman. Qian (2008) finds that sex imbalance and educational attainment responded quickly to changes in sex-specific incomes in the post-Mao era in China, rejecting a hypothesis that husband and wife do not function as a unitary decision maker. Oster (2009) shows increased access to health camps has led to greater increases in health investment among boys than among girls in India. Maccini and Yang (2009) find that the health, education, and socioeconomic statuses of Indonesian women are highly sensitive to economic shocks experienced at birth, consistent with gender bias in the allocation of nutritional resources.

lower for girls and not statistically different from zero. These differential effects cannot be explained by intrinsic biological gender differences, because health status was similar in 1993 between boys and girls (Figure A1). Rather, this is consistent with anecdotal evidence of son-preference in our sample areas. This indicates that even abolishing direct user fees will not necessarily benefit both genders equally.

In a companion set of specifications not summarized here, we also explored the effect using WHZ and HAZ (results shown in Online Appendix IV). The pattern of the findings is similar when using WHZ, confirming that the policy had substantial short-term effects on nutritional status. On the other hand, we find little impact on HAZ either because the policy did not have long-term effects or because by 1998 we had not yet observed long-term effects. See Online Appendix IV for evidence and further discussions on such similarities and disparities in results.

IV. Effect on Already Born Children

The second question in this study is whether free healthcare helps improve health status among children already born at the time of the policy reform. This is of particular interest in development policy, as growing evidence shows that low health status during early childhood has irreversible adverse effects on later health and socioeconomic status. We first describe the empirical strategy analogous to the previous ones, and then present the empirical results.

A. Empirical Framework

The empirical framework is designed to compare improvements in health status from 1993 to 1998 between the high- and low-treatment regions for children who were already born. In doing so, we trace the same cohorts over time in order to minimize effects through changes in the sample cohorts. In particular, we focus on health improvements between children aged 0 to 3 in KIDS93 (pre-reform observations) and children aged 5 to 8 in KIDS98 (post-reform observations).[18] The baseline regression is the same as equation (1). Now, equation (2) becomes;

(3) $W_{ihclt} = \gamma_0 + \gamma_1(High_c \times Post_t) + X'_h \gamma_2 + (X'_c \times Post_t) \gamma_3 + \eta_c + \psi_{lt} + \xi_{ihclt}$.

Because we trace the same cohorts over time, we now interact single year-of-birth cohort dummies (4 cohorts) with the survey years (2 periods), denoted as ψ_{lt}, allowing us to control for the main post-period effect as well as across-cohort heterogeneities not only in the pre-reform but also in the post-reform period. The parameter of interest, γ_1, is interpreted as the average treatment effect of free health services on children already born when the policy was implemented. We probe the validity of identification assumptions in Section V in detail.

B. Empirical Results

Panel B of Table 2 previews the results in the main analysis. As aforementioned, these cohorts had substantially lower health status in 1993; WAZ among the high treatment region was 0.55 standard deviation lower than mean WAZ in the reference group, and that of the low treatment region was 0.41 lower. The finding shows that children in the low-treatment region experienced little improvement during the five years from 1993 to 1998. While children in the high-treatment region experienced improved health status, the magnitudes are smaller than the corresponding figure in Panel A.

The regression-adjusted results, in Table 5, confirm that free healthcare had substantial impacts on improving health status among children who had experienced low health status at birth. The DID estimates suggest that removing user fees led to 0.57 standard deviation greater improvements in WAZ among chil-

[18] Although children aged 4 were at least partially affected by the new health policy, and are accounted for in KIDS98 (children aged 9 1998), we do not include this cohort in KIDS93 simply because this allows us to focus on the same pre-reform cohorts as in Section III. We do not include children at or above 5 years old in KIDS93, because these cohorts were at least 6 years old as of 1994, and thus were not entitled to free health services.

dren. Consistent with the previous findings, boys exhibited greater benefits than girls. Interestingly, the magnitudes of the point estimates are remarkably similar to those among newborns; short-term impacts of healthcare services are not constrained by malnutrition experienced previously. The evidence provides added significance to removing user fees; it not only helps newborns but also partially offsets the poor health status experienced at birth. Panel B and Panel C present similar patterns as for newborns: while the estimates are consistently positive and marginally significant for girls, the magnitudes are substantially higher for boys.

The estimates so far present the average treatment effect of free healthcare services among post-reform observations. To uncover heterogeneities in the treatment effect across post-reform cohorts, we interact the high-treatment region dummy with age-group dummies in 1998 and run the following regression;

$$(4) \qquad W_{ihclt} = \alpha_0 + \alpha_{1l} \sum_l (AgeGroup98_l \times High_c) + \boldsymbol{X}'_h\alpha_2 + (\boldsymbol{X}'_c \times Post_t)\alpha_3 + \eta_c + \alpha_{4k} \sum_k (Cohort98_k) + \epsilon_{ihclt}$$

where *AgeGroup98* is a binary variable taking the value of one for observations in KIDS98 and for respective age group l of 0, 1-2, 3-4, 5-6, 7-8, and 9-11.[19] We also control for all cohort dummies in KIDS98, *Cohort98*, that absorb the main post-period effect. The sample includes all observations from KIDS93 and KIDS98 whose anthropometric information is available (children aged 0 to 6 in KIDS93 and those aged 0 to 11 in KIDS98). The omitted group is observations in KIDS93, and thus each coefficient α_{1l} can be interpreted as an effect of free healthcare on a given age group in the post-reform period. A striking fact presented in Table 6 is that newborn girls under one year of age benefited from healthcare, and did so to a greater extent than boys, and with a high degree of statistical significance, whereas none of the other cohorts among girls show statistically significant effects. This indicates that pre-natal services benefited all chil-

[19] These age groups were constructed, instead of focusing on a single age, to increase precision of the estimates. The findings are similar even when we estimate the age-by-age variation.

dren by the similar magnitudes, regardless of gender, possibly because their gender was not known until birth (and ultrasound tests were not available at that time), yet there are distinct differences in the estimates by gender in subsequent ages. Taken together, the findings suggest that there is at work here a mechanism of unequal intra-household allocation of resources: to wit, boys are more likely to receive medical treatments relative to girls.

Another issue in heterogeneous effects of free health care is related to changes in the distribution of WAZ; whether the mean increase in WAZ was proportionately shared among children at various levels of WAZ. The kernel density figures of WAZ distributions in 1993 and 1998 in Appendix A2 show that children at extremely low WAZ decreased both in the high- and low-treatment regions. On the other hand, the share of children above the mean did not change for overall children in Panel A, although a further investigation at the treatment level reveals that the share has increased more in the high-treatment region. Although these figures appear to indicate that the free health services did not improve health status among malnourished children as much as those who were relatively better nourished, the interpretation requires caution.

Although the z-score classification system is widely recognized as the best practice for population-based assessment, de Onis and Blössner (1997) requires cautious interpretation in its use for individual assessment for two reasons. One is that despite an easy practice of using z-scores to define the cut-off points of overweight or underweight,[20] these definitions are based on the statistical baseline or expected prevalence, leading to designating a certain share of individuals always as malnourished or overweight even if they are truly healthy without impaired

[20] The z-scores above 2 are often labeled as overweight, and those below -2 and -3 as moderate and severe under-nutrition, respectively. For individual assessment, the term "obesity" or "fatness" should not be used, as these terms are often related to health risk, but should be only used in case of adiposity. Centers for Disease Control and Prevention state, "In-depth assessments are required to determine if children and adolescents with BMI-for-age > 95th percentile are truly overfat and at increased risk for health complications related to overweight."

growth.[21] Further, while the cut-off points are often related to health risk among adults, there are no risk related fixed values for children (Flegal and Ogden 2011). Second, the adoption by WHO of the reference group (usually healthy well-nourished US children) for international purposes has been challenged, as it requires the growth patterns of children to be similar across various ethnicities or genetic origins.[22]

For population-based assessment, on the other hand, the mean z-score value is considered as an adequate indicator of nutritional status of the entire population (WHO 1997). For instance, a negative mean z-score value in a given population indicates that most, not only those below the cut-off point but also the entire population, if not all, individuals are at risk of suboptimal health and/or nutritional conditions, and thereby requiring interventions for the entire population, not just individuals bounded by a certain cut-off. Therefore, the finding of an increased mean WAZ at most provides evidence of a reduced gap in nutritional status between the black South African children and the reference group that existed in the pre-reform period without referring specifically to a subset of children determined by the cut-off points.

V. Robustness Checks

The treatment status in any non-experimental study is, by its nature, not randomly assigned, which may lead to endogenous correlations with an error term. In this study, the main analysis thus far is based on the identification assumption that after controlling for the community fixed effects and cohort fixed effects, the intensity of access to free health services is as good as randomly allo-

[21] For example, the recognition of z-scores above 2 to define overweight implies that 2.3 percent of the reference population will always be classified as overweight for all age and sex groups.

[22] de Onis and Blössner (1997) notes, "The reference should be used as a general guide for screening and monitoring and not as a fixed standard that can be applied in a rigid fashion to individuals from different ethnic, socioeconomic, and nutritional and health backgrounds."

cated. Yet, this raises concerns regarding several potential ways in which this identification assumption may be violated. In this section, we formulate falsification tests to probe the robustness of our findings to alternative mechanisms.

A. Pre-Reform Bias

The first issue is related to pre-existing trends in health during the pre-reform period. The key to the identification strategy is that there was no systematic difference in health development between the high- and low-treatment regions, had the user fees not been abolished. Two possible scenarios, however, would give rise to pre-reform bias.

One is that black African families may have exerted influence to site clinics in rich communities, where the residents tend to be healthier to begin with. For example, many previous studies have shown strong correlations between availability of health facilities and health status in cross-sectional or some panel settings, yet these studies simply reflect that people with high income live in areas with better services and more qualified doctors (Das and Hammer 2007), whereas poor areas are more likely to have fewer health facilities, high absenteeism, and unhealthy people (Banerjee, Deaton, and Duflo 2004; Banerjee and Duflo 2006; and Chaudhury et al. 2006). Hence, the availability of health institutions and the degree of access to health care services systematically varies across areas, and treating them as exogenous biases the estimates.

On the other hand, such a mechanism is unlikely in our research context because South Africa was characterized by extreme domination by the white minority over the black majority under apartheid; resources controlled and allocated by whites under apartheid resulted in variation in availability of clinics across communities, unrelated to demand from black Africans, leading to a plausibly orthogonal relation between availability and demand.

Yet, this does not directly rule out the second possible selection mechanism that there were unobservable rules that determined the allocation of health facilities. For example, if the initial allocation of clinics under apartheid had been endogenously determined by whites, i.e., targeting the building of facilities in communities where black families were somehow more (less) responsive to health improvements, then the DID estimates would lead to upward (downward) bias. We present two pieces of evidence that limit the scope of both cases.

First, the previously shown Table 1 explores whether the observed characteristics of black African children and households are associated with the availability of health facilities, sharing the same spirit of a balancing test, conducted in Case and Deaton (1999). If black families could have exerted extensive power to allocate health facilities in their neighborhoods, or if there was an allocation rule endogenous to family background, there should be a significant difference in the mean values between the two regions. Table 1 reveals that none of the observed child and household characteristics is systematically different with the availability of clinics, except household size. Importantly, the availability of clinics was correlated neither with income level, nor with parental educational level.[23]

Second, we conduct a falsification test to look for evidence of pre-existing health trend. Panel A of Figure 1 illustrates the visual evidence. Using observations from KIDS93, we plot average WAZ across ages between children in the high- and low-treatment regions. It is evident that health status is not only similar in level but also similar in trend across ages. Yet, Panel B illustrates a clear gap in health status when the policy was implemented in 1994 among eligible cohorts. In Panel B, the sample includes children under 12 years old, whose anthropometric information is available, from KIDS98. The vertical dashed line separates those who were eligible and not eligible to the policy. Because children greater than 9

[23] Our finding is consistent with Case and Deaton (1999), also shows little association between socioeconomic characteristics among black families and school resources, such as pupil-teacher ratios, during the same period and estimate the effect of class-size by simply looking at an OLS correlation between the two.

years old in 1998 were not entitled to the free healthcare, we do not see substantial differences in health status between the two regions. However, a sharp increase in WAZ is depicted only in the high-treatment region at the time of the reform. This graphically demonstrates that only children in the high-treatment region benefited at the timing when the policy was initiated.

The corresponding regression results in Table 7 confirm what we see in Panel A. Here, we compare children aged 0 to 3 with children aged 4 to 7, both from KIDS93, and thus neither cohort had yet been affected by the policy.[24] The DID term is consistently small in magnitude and is not statistically different from zero across various specifications. Negative signs indicate that, if anything, children in the high treatment region were falling behind in health. This is either because the whites did not select the locations for clinics in response to local health status, or because the blacks did not benefit from clinics, even if there was one.

Taken together, the findings suggest that the allocation of clinics among black African communities was to some degree unsystematic or arbitrary. We have engaged in extensive discussions with local economists and doctors, but the determinants behind the initial allocation of clinics remains unclear.[25] However, even if there was some systematic rule, this appears not to invalidate the identification strategy, as evidence suggests that children in the two regions would stay on a similar trend in the absence of the policy change.

[24] Note that, although both cohorts are "pre"-treatment observations, we still use the term "post" to refer to children aged 4 to 7, just to be consistent with the rest of the paper.

[25] Likewise, Case and Deaton (1999) also questioned the rules determining the allocation of teachers across schools in 1993 with South African academics, the then Ministry of Education, and the Department of Education and Training, yet they also note, "While there were guidelines on class sizes at both primary and secondary levels, these were not closely observed. Much of the variation in pupil-teacher ratios appears to have been generated by a (largely Afrikaner) bureaucracy that exercised its own discretion, but was not very responsive to the variation in needs that followed outflows of teachers or inflows of pupils." Note that the non-existence of an endogenous rule is critical in cross-sectional studies like Case and Deaton (1999), while the parallel trends assumption is rather important in panel studies like our study.

B. Post-Reform Bias

The second issue is related to omitted variable bias in the post-reform period. One of the challenges in evaluating social policies is that there may be several other changes happening concurrently, making it difficult to disentangle their effects. The new health policy in South Africa is no exception; there may be unobserved changes in other policies or infrastructure in the post-apartheid era, specifically in the areas with clinics, not accounted for in this study. This would be the case if the high-treatment region had been more likely to receive further investments, based on some unknown rule that caused an initial clinic to be located there in the first place. Then, the treatment status simply proxies further investments in the post-apartheid era, and the main results presented so far may be picking up their effects. Similarly, the DID estimates would be overstated if improvements in the quality of medical services (i.e., range of medical services, availability of drugs or equipment, workload of physicians, number of beds, or interactions between doctors and patients) had been the main contributors to child health improvements, leading to a different policy implication, i.e., the government should allocate more resources to improving the quality of services, rather than removing user fees and simply expanding access to health services.

We formulate a falsification test to look for such evidence by tracing health improvements among children who were 6 years old and above as of 1994. In particular, we compare cohorts aged 5 to 7 in KIDS93 (the pre-reform cohorts) with cohorts aged 10 to 12 in KIDS98 (the post-reform cohorts). These cohorts comprise a valid counterfactual to children under 6 years old in the absence of the new health policy, because they are close in age yet were not entitled to free health services due to the arbitrary age limit at 6 years old, while these cohorts have been affected by any other sorts of changes. If any unobserved changes had first-order effects, the coefficient of DID estimates is hypothesized to be positive.

The results in Table 8 show that improvements in child health were lower in the high-treatment region than in the low-treatment region. The coefficient is negative and marginally significant. In addition, the coefficients are essentially unchanged over various controls, indicating that changes in the post-apartheid period still appear to be less correlated with household and community characteristics. This suggests that, if anything, children with initial access to clinics were at a disadvantage with respect to improving health status in the post-reform period. This is consistent with patients' perceptions that the quality of services deteriorated since 1994, reflecting the fact that eliminating user fees led to capacity constraints, i.e., staff shortages and patient overcrowding, accelerated by the existing labor agreements that restricted transferring staff from one place or facility to another, all of which contributed to lowering staff morale and raising barriers to the provision of high-quality service (McCoy 1996; Fonn et al. 1998; CASE 1999; Gilson et al. 1999; McIntyre and Klugman 2003; Gilson and McIntyre 2007).

The results in this falsification test also reflect a goal of post-apartheid policies that disproportionately targeted hitherto deprived and underserved areas. It has already been shown that communities with better access to clinics were also likely to have access to other sorts of infrastructure (e.g., hospitals, schools, etc.), and equalizing such geographical and racial disparity was one of the priorities of the new government. Hence, if anything, the high-treatment region was *less* likely to receive further investments. Table A1 supports this assertion in that more new clinics were built in communities in the low-treatment region between 1993 and 1998.[26]

[26] Despite this fact, we still refer to areas with a clinic in 1993 as the high-treatment region, in contrast to Duflo (2001) who refers to the districts in Indonesia where the greater number of schools was constructed as the high-treatment region. This is because the data report only the number of clinics available in respective survey years but no information on exactly when each clinic was built or started provision of services. This makes it difficult to assess how many clinics were available when each cohort was born. Rather, the government reported that the Clinic Upgrading and Building Programme, initiated as a Presidential Lead Project in 1994, did not bring about noteworthy outcomes at the early stage. Advancement was made only after August 1995, when a Fast Track Clinic Building Process was initiated to accelerate the construction and upgrading of clinics, particularly in rural areas (Department of Health 1995). Further evidence shows that construction of clinics was substantially slower in the KwaZulu-Natal province due to political instability and violence (Cameron 1996;

In summary, there is little scope for the omitted variables bias–these variables must be correlated with the treatment status and the timing of the policy change, and affect health outcomes *only* for children under 6 years old. To the best of our knowledge, the government did not provide any other child support programs comparable to the free healthcare policy during this period.[27] It is worth noting that, although quality of care appears not to have played a role, its importance should not be understated, as the interactions between quantity and quality should promote further health improvement. The negative estimated effects for the older children suggest that the surge in demand, along with the issues unsolved on the supply side, i.e., poor financial management to support and improve the quality of health services, may have created some negative externality effects for older children and adults. Indeed, these may partially or entirely offset the gains from the young children. While our findings still highlight the importance of having access to health services (quantity-side benefits), it is clear that quality side benefits should have been enhanced. The interaction between quantity and quality of health services and its impact on health status constitute an interesting topic for future research.

C. Alternative Mechanisms

The effects presented so far suggest that removing user fees was a driving factor contributing to health improvement for a segment of the targeted population. Evidence in the falsification test above highlights the fact that the mechanisms through which free health services improved child health status pertain to children under 6 years old, excluding any other changes that also affected slightly

Khan, Lootvoet, and Vawda 2006); even the first democratic election did not take place until 1996. These pieces of evidence lead us to assume that clinics observed in 1998 are more likely to have been built in later years, namely close to 1998, and thus areas with a clinic in 1993 should have benefited more from the policy. It is also worth noting that even if clinics were built earlier, the results estimate only the lower bound of the effects.

[27] In Online Appendix V, we provide further evidence on alternative policies and mechanisms that may confound our results. In particular, we discuss the Integrated Nutritional Programme (INP) in greater detail.

older children. This subsection considers several alternative mechanisms that may have generated improvements in health status.

Migration. The presumption that black families had limited influence over the initial allocation of health facilities does not rule out the possibility that they could still migrate to places nearby clinics. Yet, this was not the case for blacks prior to the fall of apartheid. As noted, under apartheid, millions of black Africans were forced into "homelands." They had little freedom to choose where to live and their mobility was severely restricted, making it difficult to migrate to areas with better health infrastructure. However, the demise of apartheid freed black Africans from these constraints, which could have led to a change in the composition of the sample population in the high- and low-treatment regions. To avoid this geographical self-selection bias after 1994, we identify households according to the communities in which they resided in 1993, instead of according to their current communities. Both are highly correlated, as approximately 95.3 percent of the households in KIDS98 still lived in the same community as in 1993, yet the former is predetermined and not endogenous to selection into the high-treatment region. Nonetheless, we repeat the main analysis using only the samples who did not migrate by 1998, shown in Online Appendix V. The main results are unchanged, suggesting that migration itself cannot explain the main results.

Health Education. Another possibility is that health improvement is due not to health services *per se* but to the health education provided at clinics. It is widely observed in developing countries that parents lack basic knowledge or information on sanitation and child health. For example, CASE (1995) shows that 23 percent of black African households used unpiped water, such as from a river, stream, or dam, and 89 percent of them drank such water without boiling it in 1993. Clinics and other public health facilities serve as important conduits for the dissemination of basic health information to the public through educational campaigns. While it is difficult to isolate the effects of such a channel, two aforementioned pieces of evidence lend support to the argument that health education does not appear to be a key factor in health improvement. One is that the magnitude of effects was much larger for boys. Since implementing sanitary behaviors, e.g., boiling water or washing hands, is by and large costless, and thus entails no pressure to exclude girls, the gender skew does not support the impact through health education. Furthermore, there was no effect on non-eligible cohorts in the post-treatment period. Even if gender bias existed in sanitary controls, this falsification test would still show positive effects if health education were a key determinant of health improvement. Therefore, health education cannot explain the positive association between the availability of clinics and health improvements.

Fertility. Another possible mechanism whereby improved health status could have been achieved is through changes in black African women's fertility behaviors in response to free prenatal services. The changes could have been either positive or negative. Fertility could have increased if women took advantage of the policy and became pregnant more frequently. It could have decreased if women, taking into account a decrease in infant mortality due to improved prenatal care, lowered the number of births. The reason why this mechanism could potentially bias the results is that it shifts the distribution of health status among newly born babies; with increased fertility, the estimates provide lower bounds since less healthy babies, who would not have been born alive previously, were born alive now. With decreased fertility, the estimates are contaminated by effects of intra-household decisions on fertility. In fact, however, extensive studies show that there has not been any change in fertility (McCoy 1996; Schneider and Gilson 1999). Further, we find evidence that the number of births since 1993 is not associated with the treatment status in 1998, suggesting that fertility is unlikely to generate a bias in the analyses.

VI. Conclusion

This paper examines the impact of removing user fees from healthcare on the health status of poor children in South Africa. To this end, we exploit plausibly exogenous variation in the exposure to the new reproductive health policy in the post-apartheid period, determined by the initial availability of a clinic in the community in 1993. This quasi-experimental design provides a rare opportunity to study a population among whom the treatment status is orthogonal to *ex ante* characteristics among households and child health status, as the allocation of clinics was made rather arbitrarily by whites, creating variation in access to health facilities yet similar household characteristics across communities.

A comparison of the changes in WAZ before and after the policy was implemented sheds light on substantial short-term improvements in nutritional status not only of newborns but also of children who were already born at poor health status when the policy was initiated. However, as noted above, free healthcare had the most important influence on boys' health status, indicating that simply removing user fees may not necessarily benefit both genders equally.

Further, a companion set of results confirms the short-term effect of free health services using WHZ, yet no evidence on the long-term effect was found using HAZ. The long time span effect of free health services thus remains unanswered.

We provide two pieces of evidence that the results are not driven by inappropriate identification assumptions. First, we confirm that there is little systematic difference in preexisting trends among observations in the pre-treatment period. Second, we find no effect on non-eligible cohorts (children aged 6 years and above as of 1994) in the post-treatment period, suggesting no other changes in policies and society should confound the effect.

These results present several important policy implications for other developing countries contemplating the abolition of user fees. First, removing user fees is effective in improving child health status through increased access to and utilization of health services in an environment where poor households face significant budget constraints. This is contrary to the argument often put forth on behalf of user fees that even the poor will bypass free social services because they are assumed to carry no value. Our finding is in line with the recent RCTs evidence showing that free distribution is an efficient means of distributing basic health products relative to cost-sharing.

Second, increased access to health services is an important determinant of better health outcomes. Quality of health services in developing countries is often perceived as poor, and thus whether increased access helps improve health status has been questioned. This paper provides evidence that children who had better access to health services achieved improved nutritional status to a significantly greater degree than those with less access. Further, our findings show that simple access to health services can, under certain circumstances, compensate for earlier deprivations or setbacks over the short-run. Given the growing evidence regarding the long-term effects of health status during early childhood, the totality of benefits must be assumed to be even greater.

Third, free health services are often challenged by a potential tradeoff between quantity and quality of services. Our study supports the assertion that the quality of health services appears to have deteriorated, due to poor financial management, leading to lower health status among older children in the high-treatment region. However, the net benefits were still positive and significant for children who received free healthcare. It is still an open question whether these benefits came at a cost to older children and adults who experienced lowered quality of services.

Lastly, while the free provision of health services removes a budget constraint and leads to increased access, it still does not necessarily solve the equity issue.[28] Gender bias in allocation of nutritional resources has been debated in the context of development. Evidence is somewhat mixed with regard to whether women are more or less sensitive to shocks, depending on whether the shock is positive or negative and whether the impacts we examine are short-term or long-term.[29] The pattern of our finding contrasts with literature showing that women are more prone to positive shocks, while it is in line with literature showing that gender bias leads to greater investments in men when men are also initially subject to constraints. For example, Oster (2009) argues that a simple increase in the overall level of access to social programs is not sufficient to achieve social goals; indeed such access may not only fail to benefit women but may even lead to greater gender inequality. Understanding both the direct and indirect costs to households of treating girls and how precisely gender bias tends to restrict the utilization of medical resources will be important areas of investigation to assure that future interventions are designed to also benefit girls.

[28] See also Nanda (2002) for the impact of user fees on women's utilization of health care services in Africa.
[29] Sizable evidence shows that allocating nutritional resources to women is more prone to result in negative impacts than allocation of the same resources to men (Dreze and Sen 1989; Das Gupta 1987; Behrman 1988; Behrman and Deolalikar 1990; Jayachandran 2009; Maccini and Yang 2009), while there is some evidence against the existence of such gender bias (Levine and Ames 2003). On the other hand, evidence shows that women are more responsive to positive shocks (Rose 1999; Duflo 2003; Qian 2008).

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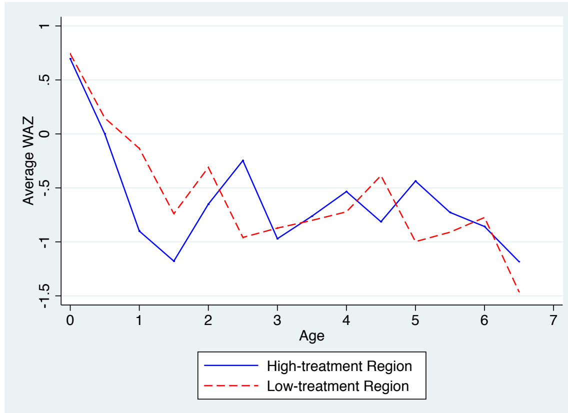
Panel A. KIDS93



Panel B. KIDS98

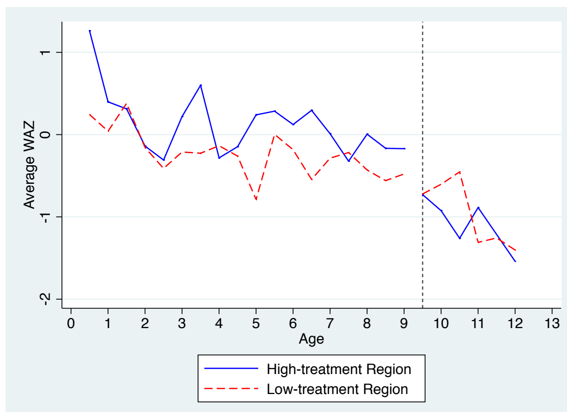


FIGURE 1: AVERAGE WAZ BY AGE AND REGION

Notes: The figure plots the mean value of WAZ by age and region. WAZ below -6 and above 5 are removed as outliers, as these numbers are seen as biologically implausible. The sample contains children aged 0 to 7 in KIDS93 in Panel A, and children aged 0 to 12 in KIDS98 in Panel B. Ages are calculated from ages in month, from which we denote age 0 month to 6 months as 0 year old, 6 months to 12 months as 0.5 year old, and so forth. The dashed line in Panel B indicates the timing of policy change; cohorts to the right are not affected by the policy, whereas cohorts to the left are partially or fully affected by the policy. The dashed line is drawn at age 9.5 years old since the policy started in June 1994, and most of the samples in KIDS98 were surveyed from March to May in 1998. Then, according to our calculation of ages, children at exactly 6 years old at the time of policy change are 9.5 years old in KIDS98.

Source: Author's calculation from the KIDS93 and KIDS98

TABLE 1–DIFFERENCES AT BASELINE BY TREATMENT STATUS

Variables	High	Low	Diff.
Panel A: Child characteristics			
Age	3.28	3.39	-0.104
	[1.60]	[1.64]	(0.114)
WAZ	-0.581	-0.522	-0.060
	[1.63]	[1.57]	(0.113)
WHZ	0.216	0.208	0.008
	[1.58]	[1.66]	(0.117)
HAZ	-1.106	-1.105	-0.001
	[1.71]	[1.68]	(0.121)
Observations	350	481	
Panel B: Household characteristics			
Monthly household income	1037.36	1038.61	-1.25
	[984.66]	[1150.63]	(99.25)
Father's education	1.96	2.00	-0.041
	[3.18]	[3.13]	(0.289)
Mother's education	4.60	4.30	0.300
	[3.54]	[3.49]	(0.322)
HH size	8.64	9.99	-1.351***
	[3.49]	[4.31]	(0.365)
Observations	212	270	
Panel C: Community characteristics			
No. of other types	0.462	0.036	0.426***
health infrastructure	[0.81]	[0.189]	(0.158)
Immunization	1.12	1.04	0.08
campaign	[0.33]	[0.19]	(0.072)
Observations	26	28	

Notes: The table provides summary statistics of variable means in KIDS93. The sample is restricted to children under age 7 whose anthropometric data are available. Observations are at the individual level for Panel A, the household level for Panel B, and the community level for Panel C, whose maximum number of observations are reported in the last row in respective panel (the number of observations for individual variable may be lower due to missing values). Other types of health infrastructure include hospital, dispensaries, and maternity home, and immunization campaign measures the number of immunizations campaign since 1993 at the community level. The second column uses the sample from the high-treatment region, whereas the third column focuses on the low-treatment region, and the last column estimates the differences in means between the two regions under the null hypothesis that the difference is equal to zero. Standard deviations are in square brackets and standard errors are in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

TABLE 2–DIFFERENCE-IN-DIFFERENCES MATRIX IN MEANS OF HEALTH STATUS BY PERIOD AND TREATMENT STATUS

Panel A: Effect on Newborns

	Weight-for-age z-score		
	High	Low	Diff.
Post-reform:	0.321	-0.069	0.391
Aged 0 to 3 in KIDS98	(0.152)	(0.074)	(0.169)
Observations	[212]	[288]	
Pre-reform:	-0.545	-0.414	-0.131
Aged 0 to 3 in KIDS93	(0.161)	(0.115)	(0.197)
Observations	[246]	[325]	
Difference	0.867	0.345	0.522
	(0.193)	(0.137)	(0.235)

Panel B: Effect on Already Born Children

	Weight-for-age z-score		
	High	Low	Diff.
Post-reform:	0.045	-0.345	0.390
Aged 5 to 8 in KIDS98	(0.129)	(0.070)	(0.147)
Observations	[283]	[341]	
Pre-reform:	-0.545	-0.414	-0.131
Aged 0 to 3 in KIDS93	(0.161)	(0.115)	(0.197)
Observations	[246]	[325]	
Difference	0.590	0.069	0.521
	(0.225)	(0.121)	(0.253)

Notes: Each entry in cell reports the mean value of WAZ in the corresponding type of the regions (high vs. low) and periods (pre- vs. post-reform). Robust standard errors are reported in the parentheses, and the number of observations in each cell is provided in the square brackets.

TABLE 3–EFFECTS OF FREE HEALTHCARE ON NEWBORNS

	Dependent Variable				
	Weight-for-age z-score				
Variables	(1)	(2)	(3)	(4)	(5)
High*Post	0.522**	0.571**	0.515**	0.692**	0.638**
	(0.235)	(0.256)	(0.245)	(0.288)	(0.275)
Post	0.345**				
	(0.137)				
High	-0.131				
	(0.197)				
Father's education			0.015		0.016
			(0.019)		(0.019)
Mother's education			-0.014		-0.014
			(0.017)		(0.017)
HH size			-0.014		-0.014
			(0.010)		(0.010)
Log(income)			0.154**		0.155***
			(0.058)		(0.056)
Immunization				0.902***	0.925***
				(0.313)	(0.310)
No. of other health facilities				-0.321*	-0.330*
				(0.185)	(0.187)
Observations	1071	1071	1071	1071	1071
R^2	0.04	0.2	0.21	0.20	0.21
Household characteristics	No	No	Yes	No	Yes
Community characteristics	No	No	No	Yes	Yes
Cohort FE	No	Yes	Yes	Yes	Yes
Community FE	No	Yes	Yes	Yes	Yes

Notes: This table reports the estimates of the effect of free healthcare on newborns. The sample is children aged 0 to 3 from KIDS93 (pre-reform cohorts) and KIDS98 (post-reform cohorts). The observations are at the individual level. Robust standard errors are in the parentheses. The dependent variable is WAZ, and WAZ below -6 and above 5 are removed as outliers, as these numbers are seen as biologically implausible. All specifications in columns (2)-(5) include birth-year-cohort fixed effects and community fixed effects.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

TABLE 4–EFFECTS OF FREE HEALTHCARE ON NEWBORNS BY GENDER

		Dependent Variable Weight-for-age z-score			
Variables	(1)	(2)	(3)	(4)	(5)
Panel A: Boys					
High*Post	0.640**	0.741**	0.697*	1.012**	0.969**
	(0.303)	(0.361)	(0.348)	(0.417)	(0.407)
Observations	551	551	551	551	551
R^2	0.06	0.29	0.30	0.30	0.31
Panel B: Girls					
High*Post	0.390	0.374	0.323	0.430	0.384
	(0.275)	(0.311)	(0.298)	(0.362)	(0.344)
Observations	520	520	520	520	520
R^2	0.02	0.22	0.24	0.22	0.24
Household characteristics	No	No	Yes	No	Yes
Community characteristics	No	No	No	Yes	Yes
Cohort FE	No	Yes	Yes	Yes	Yes
Community FE	No	Yes	Yes	Yes	Yes

Notes: The sample is the same as in Table 3, focusing on boys in Panel A and on girls in Panel B.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

TABLE 5–EFFECTS OF FREE HEALTHCARE ON ALREADY BORN CHILDREN

		Dependent Variable			
	Weight-for-age z-score				
Variables	(1)	(2)	(3)	(4)	(5)
Panel A: All					
High*Post	0.521**	0.544**	0.484*	0.638*	0.566*
	(0.253)	(0.267)	(0.248)	(0.334)	(0.316)
Observations	1195	1195	1192	1195	1192
R^2	0.02	0.16	0.17	0.16	0.18
Panel B: Boys					
High*Post	0.642*	0.842**	0.781**	1.127**	1.049**
	(0.360)	(0.386)	(0.359)	(0.489)	(0.462)
Observations	611	611	610	611	610
R^2	0.03	0.25	0.26	0.26	0.27
Panel C: Girls					
High*Post	0.385	0.329	0.263	0.314	0.238
	(0.252)	(0.262)	(0.251)	(0.286)	(0.276)
Observations	584	584	582	584	582
R^2	0.01	0.18	0.20	0.18	0.20
Household characteristics	No	No	Yes	No	Yes
Community characteristics	No	No	No	Yes	Yes
Cohort*Period FE	No	Yes	Yes	Yes	Yes
Community FE	No	Yes	Yes	Yes	Yes

Notes: This table reports the estimates of the effect of free healthcare on children already born when the policy started. The sample is children aged 0 to 3 in KIDS93 (pre-reform cohorts) and children aged 5 to 8 in KIDS98 (post-reform cohorts). Panel A uses all observations, whereas panel B uses only boys and panel C uses only girls. The observations are at the individual level. Robust standard errors are in the parentheses. All specifications in columns (2)-(5) include community fixed effects to replace the treatment dummy in specification (1) and cohort*period fixed effects to replace the post dummy in specification (1).

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

TABLE 6–HETEROGENEOUS EFFECTS OF FREE HEALTHCARE BY AGE GROUP AND GENDER

		Dependent variable		
		Weight-for-age z-score		
Age in KIDS98	Yrs. of exp.	(1)	(2)	(3)
0	1	1.185**	1.140*	1.849***
		(0.527)	(0.633)	(0.625)
1-2	2-3	0.305	0.835**	-0.161
		(0.259)	(0.411)	(0.296)
3-4	4-5	0.498	0.919**	0.084
		(0.338)	(0.441)	(0.354)
5-6	5-4	0.682**	1.229**	0.159
		(0.307)	(0.463)	(0.294)
7-8	3-1	0.336	0.670	0.024
		(0.291)	(0.446)	(0.282)
9-11	1-0	0.018	0.209	-0.160
		(0.227)	(0.342)	(0.233)
Samples:				
Boys		Yes	Yes	No
Girls		Yes	No	Yes
R^2		0.14	0.17	0.19
Observations		2,396	1,212	1,184

Notes: This table reports the coefficients of the interaction term between the high treatment region and each age group in KIDS98, using the equation (4). The sample includes all observations in KIDS93 and KIDS98 whose anthropometric information is available.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

TABLE 7–ADDRESSING PREEXISTING TRENDS

	Dependent Variable				
	Weight-for-age z-score				
Variables	(1)	(2)	(3)	(4)	(5)
High*Post	-0.217	-0.145	-0.157	-0.145	-0.157
	(0.221)	(0.253)	(0.256)	(0.253)	(0.256)
Observations	821	821	820	821	820
R^2	0.01	0.19	0.20	0.19	0.20
Household characteristics	No	No	Yes	No	Yes
Community characteristics	No	No	No	Yes	Yes
Cohort FE	No	Yes	Yes	Yes	Yes
Community FE	No	Yes	Yes	Yes	Yes

Notes: This table aims to address pre-existing trends in health status between the two sets of the regions during the pre-treatment period, using observations from KIDS93. The pre-reform cohorts refer to children aged 0 to 3 in KIDS93 and the "post"-reform cohorts refer to children aged 4 to 7 in KIDS93, while none of the cohorts was yet affected by the policy. Coefficient in Column (1) is based on the equation (1), and all other columns report the coefficient of the interaction term based on the equation (2).

TABLE 8–EFFECT OF FREE HEALTHCARE ON NON-AFFECTED CHILD HEALTH STATUS

	Dependent Variable Weight-for-age z-score				
Variables	(1)	(2)	(3)	(4)	(5)
High*Post	-0.513*	-0.444	-0.479*	-0.479	-0.506
	(0.272)	(0.366)	(0.365)	(0.422)	(0.421)
Observations	308	308	308	308	308
R^2	0.01	0.23	0.24	0.23	0.24
Household characteristics	No	No	Yes	No	Yes
Community characteristics	No	No	No	Yes	Yes
Cohort*Period FE	No	Yes	Yes	Yes	Yes
Community FE	No	Yes	Yes	Yes	Yes

Notes: This table reports estimates of the effect of free healthcare on children among the non-eligible cohorts. The sample is children aged 5 to 7 in KIDS93 (pre-reform cohorts) and children aged 10 to 12 in KIDS98 (post-reform cohorts). Note that because only children less than 6 years old as of 1994 are entitled to free healthcare, even the post-reform cohorts were not yet exposed to the policy.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

APPENDIX

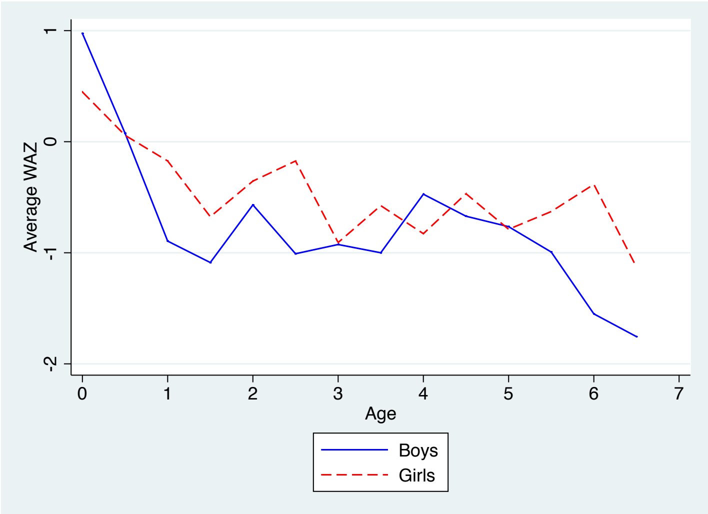
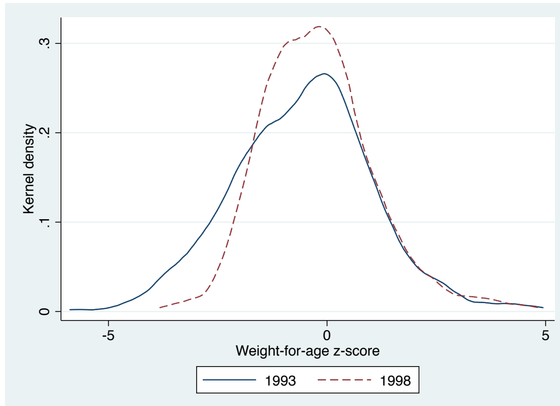


FIGURE A1: AVERAGE WAZ BY GENDER IN 1993

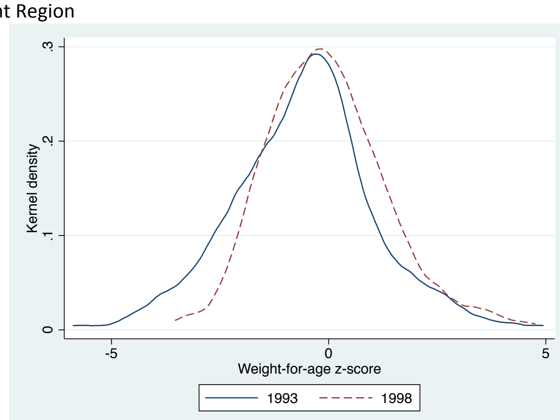
Notes: The figure plots the mean value of WAZ by age and gender for children in KIDS93. The mean difference in WAZ between boys and girls is 0.190 with the standard error of 0.130 Additional comments are the same as Figure 1.

Source: KIDS93

Panel A: All Children



Panel B: The High-Treatment Region



Panel C: The Low-Treatment Region

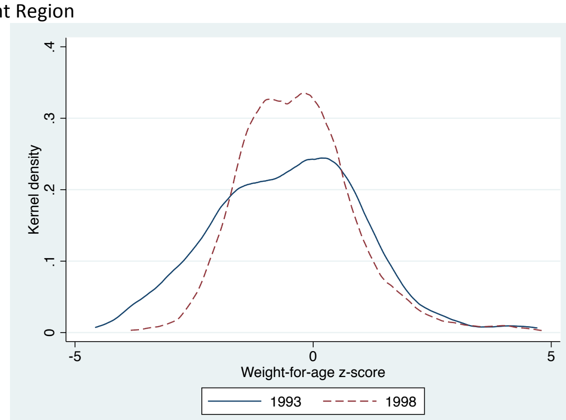


FIGURE A2: DISTRIBUTION OF WAZ

Notes: These figures plot the kernel density of WAZ distribution in 1993 (solid blue line) and in 1998 (dashed red line). The sample uses all children whose anthropometric values are available in respective survey.

TABLE A1–CONSTRUCTION OF NEW CLINICS

Variables	Coeff.
High	-0.879***
	(0.225)
Constant	1.071***
	(0.088)
Observations	54
	0.24

Notes: This table compares the differences in the number of constructions of new clinics between the two types of the region. The dependent variable is the number of clinics built between 1993 and 1998, and the observation is at the community level.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.