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Assessing regional variations in the effect of the removal of user fees on institutional deliveries in rural Zambia.

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ABSTRACT

This paper examines regional differences in the effect of user fee removal in rural areas of Zambia on the use of health institutions for delivery. The analysis uses quarterly longitudinal data covering 2003q1-2008q4. When unobserved heterogeneity, spatial dependence and quantitative supply-side factors are incorporated in the Interrupted Time Series (ITS) design, user fee removal is found to immediately increase aggregate institutional deliveries, although the national trend was unaffected. Drug availability and the presence of traditional birth attendants also influence institutional deliveries at the national level, such that, in the short-term, strengthening and improving community-based interventions could increase institutional deliveries. However, there is significant variation and spatial dependence masked in the aggregate analysis. The results highlight the importance of service quality in promoting institutional deliveries, and also suggest that social and cultural factors, especially in rural areas, influence the use of health facilities for delivery. These factors are not easily addressed, through an adjustment to the cost of delivery in health facilities.

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1. Introduction

Improving maternal health remains a global challenge, particularly for countries in the sub-Saharan Africa region, where maternal mortality averages 680 per 100,000 live births and almost 50% of the approximately 350,000 annual maternal deaths occur (e.g. Hogan et al., 2010; World Health Organisation, 2010). With the increased pressure to achieve the Millennium Development Goal (MDG) 5, reducing the maternal mortality rate by 75% by 2015, there is a need to further facilitate skilled birth assistance and institutional deliveries (e.g. Campbell and Graham 2006; United Nations, 2011; World Health Organisation, 2010). Institutional deliveries and skilled assistance at birth are important factors in reducing deaths arising from complications in pregnancy. It has even been suggested that skilled birth assistance is the single most important factor in preventing maternal deaths (e.g. World Health Organisation, 1999). Although skilled assistance at birth and institutional deliveries are key to the reduction in maternal deaths, only 20-40% of women in developing countries deliver in a health facility (e.g. Limwattananon et al., 2011), while approximately 70% of births among poor women take place at home (e.g. Montagu et al., 2011).

Economically, the limited use of institutional delivery services is expected to relate to demand and/or supply-side barriers that deter women from accessing these services (e.g. Amooti-Kagoona and Nuwaha, 2000; Fournier et al., 2009; Gage, 2007; Gage and Calixte, 2006; Ronsman et al., 2003; Wagle et al., 2004). Supply-side, or health system factors, such as the quality of services, not often captured in household surveys, matter (e.g. Gabrysch and Campbell, 2009). More germane to this analysis, financial barriers, such as user fees, also discourage the utilisation of maternal health services, and possibly increase the use of informal care (e.g. Borghi et al., 2003; Gage, 2007; Nanda, 2002; Stekelenburg et al., 2004). These barriers may have equity implications, as well as health implications. There are more pronounced disparities between rich and poor in the use of delivery services, compared to other health services, as well as significant gaps between the two groups in the utilisation of antenatal care services (e.g. Gwatkin et al., 2004).

User fees for health services were introduced in many developing countries in the late 1980s, with the aim of financing health care, including maternal health care. Advocates supporting healthcare user fees argue that they enhance the efficient allocation of goods and services by targeting the population in need of the good or service, i.e., low valuation consumers are screened (e.g. Oster, 1995). Also, if higher prices are perceived to reflect better quality, user fees could increase demand (e.g. Bagwell and Riordan, 1991), a potential virtuous cycle, at least in terms of revenue generation. However, the removal of user fees could also have negative effects on equity and access (e.g. Creese, 1991; Russell and Gilson 1997; Yates 2009). In an effort to increase healthcare accessibility, by reducing the direct financial cost associated with treatment, most countries in sub-Saharan Africa abolished or reduced user fees for health services, including maternity services and delivery services (e.g. De Allegri et al., 2011; Masiye et al., 2008; Nabyonga et al., 2005; Wilkinson et al., 2001), or exempted certain groups from payment (e.g. Penfold et al., 2007; Witter et al., 2007).

Only limited evidence relating national user fee reforms to women's uptake of maternity services, such as institutional delivery and skilled assistance at birth, is available (e.g. Dzakpasu et al., 2013). Although it has not been possible to examine whether maternal deaths will decrease as a result of a fee reduction, as has been proposed (e.g. Kippenberg et al., 2008; Prata et al., 2004), it has been possible to directly examine institutional deliveries and skilled assistance at birth. Witter et al. (2007) and Penfold et al. (2007) find a 10-36% increase in institutional deliveries in the Central and Volta regions of Ghana, although, when fees were temporarily reinstated, the number of institutional deliveries decreased. Deininger and Mpuga (2005) also report an increase in institutional deliveries after the abolition of user fees in Uganda. Skilled assistance at birth, on the other hand, does not increase following the abolition of user fees (e.g. De Allegri et al., 2011; Tann et al., 2007). Not only is the literature limited, it has generally ignored the role of supply-side factors (e.g. Cheelo et al., 2010; Lagarde, Barroy and Palmer, 2012; Masiye et al., 2008) or limited the analysis to a comparison of average utilisation before and after the policy change (e.g. Masiye et al., 2008). Although panel data has been available, previous studies have not accounted for that structure in the data (e.g. Lagarde, Barroy and Palmer 2012), and, thus, have been unable to control for potential time invariant confounders. For instance, in the reporting of administrative data, which is used here, there could be consistent over- or under-reporting of information. Similarly, within a region there could be consistently more or fewer institutional deliveries, due to region-specific characteristics. Exploiting the information in the panel, allows one to address these concerns. Moreover, estimates obtained from an aggregate analysis often mask important heterogeneities that may warrant further attention.

The context of the policy change in Zambia provides an opportunity to do address the preceding concerns and improve our understanding of the relationship between user fees and maternal health services. User fees were removed in public health facilities in 54 rural districts in Zambia in April 2006 to improve access to health services, particularly for the poor, who mostly reside in rural areas.[2] Prior to the policy change, preventative services, such as: antenatal care, family planning and counselling, were exempt from payment, but not delivery services. In terms of fee setting, health providers in various regions were allowed to set fees in line with locally defined affordability criteria, but the fees were to be approved by the Ministry of Health (Masiye et al., 2008). Delivery fees at public health facilities varied from ZMK10, 000 ($3) to ZMK30, 000 ($9) (e.g. Cheelo et al., 2010; Kamwanga et al., 2002; Stekenlenburg et al., 2004).[3]

[2] Free health services included all aspects of preventative and curative services at Health Posts (HP) and Health Centres (HC), including Hospital Affiliated Health Centres (HAHC). Patients referred to first level hospitals were to be treated free of charge for all services at such facilities. Patients referred further upwards from level one hospitals were exempted from charges. Fee exempt services included: consultation, treatment, admission and diagnostic services (e.g. MOH, 2007).

[3] Unsurprisingly, when formal charges are not levied, indirect levies have been. In the absence of user fees in Ghana and Benin, women have been required to purchase supplies – bleach, to sterilise materials used during delivery, gloves and sanitary pads – when admitted to a health facility for delivery (e.g. Borghi *et al.*, 2003; Kowalewski *et al.*, 2002). Obtaining these supplies could easily delay or prevent the use of delivery services. At the household level, delivery services present additional financial implications, in terms of travel costs, as well as the patient's and the patient's companions' time. Additionally, relatives may have to bring food for the patient, as they await delivery (e.g. Borghi *et al.*, 2003).

Data from these districts is collated over 2003q1-2008q4 to construct a panel of the 9 provinces. We exploit the panel information in the subsequent analysis, and consider potential dynamic effects. The analysis is founded upon an Interrupted Time Series (ITS) design, complemented by a segmented regression analysis, which is adequate, when only retrospective longitudinal data, before and after an intervention, is available. We further disaggregate the data to obtain regional level estimates from Seemingly Unrelated Regressions (SUR), addressing spatial dependence within an error component framework. In contrast to much of the previous literature, the analysis incorporates supply-side factors, including quantitative measures of service quality, to assess the impact of user fee removal on institutional deliveries.

The outline of the rest of the paper is as follows: Section 2 summarises the relevant empirical literature. Section 3 describes the data. The estimation strategy is outlined in Section 4, while the econometric results are presented and discussed in Section 5. Brief concluding remarks are presented in Section 6.

2. Relevant Literature

There is an extensive literature relating health financing policy changes to health service utilisation (e.g Lagarde, Barroy and Palmer 2012; Masiye et al., 2008; Nabyonga et al., 2005; Ridde, 2003; Wilkinson et al., 2001) however, much of that evidence could be biased (e.g. Lagarde and Palmer, 2008). Although a few studies have accounted for specific time series properties and problems (e.g. Lagarde, Barroy and Palmer 2012; Wilkinson et al., 2001), most of the analysis has focussed on a simple comparison of average utilisation before and after a policy change (Ridde, 2003; Nabyonga et al., 2005; Masiye et al., 2008). Even if the analysis has gone beyond a simple comparison before and after the policy change, which is not always the case, the impact of policy on curative and preventative care has been the predominant theme, rather than maternal health services. Evidence from this literature suggests that removing user fees increased access to curative services for the vulnerable groups (e.g. Deininger and Mpuga, 2005; Lagarde, Barroy and Palmer, 2012; Lagarde and Palmer, 2008; Masiye et al., 2008; Wilkinson et al., 2001), led to provider choice substitution (e.g. Koch, 2012) but may have negatively affected service quality (e.g. Lagarde and Palmer, 2008) and utilisation amongst non-targeted groups (e.g. Lagarde, Barroy and Palmer 2012). Also, curative care utilisation increases could have been at the expense of preventative services (e.g. Wilkinson et al., 2001). In Ridde and Morestin's (2011) review of 20 articles, they note that the abolition of user fees has generally had positive effects on the utilisation of health services.

With regard to maternal health services, as would be expected, user fees have had negative effects on utilisation (e.g. Nanda, 2002). Utilisation of antenatal care (ANC) services in Zimbabwe and Tanzania declined with the introduction of user fees. In Ghana, with the introduction of the fee exemption policy on deliveries, the proportion of institutional deliveries increased in the Central and Volta region, and, encouragingly, the increase was higher for women facing the greatest financial barrier to health care and were at the greatest risk of maternal mortality (e.g. Penfold et al., 2007). Asante et al. (2007) provide further evidence of equity improvements; fee exemption policy reduced the overall costs of delivery by 8% to 22%, depending on the type of delivery.

Although user fees do matter, there are other factors affecting institutional deliveries. Gabrysch and Campbell (2009) identify 20 determinants, based on a review of 80 articles. They group determinants into four broad themes: (1) socio-cultural factors, (2) perceived benefit/need of skilled attendance, (3) economic accessibility and (4) physical accessibility. The identified factors influence decision-making at the individual and household level; they also include measures affecting the ability to pay and the role of distance as access obstacles. They suggest that other factors, such as the quality of care, are not easily captured in household surveys, although they are reported as being essential in qualitative studies. Thus, there is a need to examine the effect of supply-side factors, which is done here.

In additon to the factors mentioned by Gabrysch and Campell (2009), the use of ANC services positively affects the utilisation of institutional deliveries and skilled attendance (e.g. Gage, 2007), as does previous delivery at a health facility (e.g. Bell et al., 2003; Stephenson et al., 2006). Essentially, experiences with the health system, especially positive ones, gained through ANC visits or previous deliveries can affect delivery. Similarly, ANC provides opportunities for health workers to recommend a place of delivery, based on pregnancy risk assessments, and women with lower risks may be encouraged to deliver without a skilled assistant. Moreover, ANC attendance breeds familiarity with the health system and health facility; thus, women who seek ANC are more likely to use the same facility for delivery. However, the positive relationship observed between seeking ANC and delivering at a health facility could result from other confounding factors, such as the availability and access to services (e.g. Breen and Ensor, 2011; Gabrysch and Campbell, 2009); the same has been suggested for previous deliveries (e.g. Gabrysch and Campbell, 2009; Stephenson et al., 2006). For instance, the use of ANC or delivery services may indicate the presence of a nearby health facility offering delivery services. In many developing countries, it should be noted, ANC services are provided through outreach services, mobile clinics and small facilities, many of which do not offer delivery services. To address these problems, we include factors to proxy for the availability of health services.

As implied by the previous discussion, quality of care, which covers both the perceived quality and the medical quality of care, is an important factor influencing the choice to deliver at a health facility. Although this implication has been confirmed in Hadley's (2011) qualitative study, few quantitative studies manage to capture the quality of care. Therefore, including such factors, when available, as is the case here, is a necessary addition to the literature.

Finally, alternative delivery options should also be considered, as they are likely to impact on institutional delivery and skilled birth attendance. In the African context, the primary alternative is a traditional birth attendant (TBA), an alternative that may or may not be an appropriate substitute. TBAs may not provide satisfactory assistance, due to low levels of literacy, nonexistant to poor training and limited obstetric skills, all of which negatively affect the delivery process, especially when there are complications (e.g. Garces et al., 2012; Singh et al., 2012). On the other hand, TBAs could be better than nothing, especially if they are properly trained. Although maternal mortality rose after TBAs were banned in Malawi, they then fell, once TBAs were trained and reinstated (e.g. Ana, 2011). There is further evidence that trained TBAs reduce neonatal mortality (e.g. Gill et al., 2012), and are a feasible and affordable option in countries with limited medical skills capital; however, they need an appropriate support network to work effectively (e.g. Stekelenburg et al., 2004).

The evaluation of user fee abolition and institutional deliveries in Zambia, discussed below, highlights the importance of the aforementioned supply-side factors, such as quality of care, and demand-side factors, such as user fees and alternatives, in influencing the demand for institutional deliveries. A natural experiment in Zambia underpins the identification strategy in the empirical analysis, although additional practical realities must also be considered. The policy change in Zambia, a country consisting of 72 districts in 9 provinces, was implemented under different conditions. For instance, drugs and financing that were to be provided to some districts were not provided successfully (e.g. Carraso et al., 2010), which could have compromised the quality of care in those districts, leading to an exodus of unsatisfied clients into other districts or provinces. Thus, policy implementation potential plays an important role in the analysis, as there could be provincial or district interdependencies in the outcomes. Therefore, the experimental setting analysed is further complemented with tests for cross-sectional dependence, following Pesaran's (2004) CD test, as well as Breush and Pagan's (1980) Lagrange Multiplier (LM) test, and corrections for the identified dependencies. These tests and corrections address a major critique of panel data analysis; cross-sections are unlikely to be independent. Furthermore, quality of care, one of the important supply-side factors, is likely to differ by province and/or district, influencing effect size in these regions. Zellner's (1962) seemingly unrelated regression (SUR), which controls for spatial dependence, is used to estimate the potentially different effect sizes across regions.

3. The Data

3.1 Data source

The study uses routine quarterly data, collected within the Health Management and Information System (HMIS) administered by the Ministry of Health (MOH) in Zambia. The data include quarterly information on the supply and use of a wide range of health services at all public health facilities nationwide, but it is aggregated to the district level. Data from all 54 rural districts, in which user fees were abolished in April 2006, were available; complete data was only available for 46 of the districts.[4] From the district level data, it was possible to compile regional data for the 9 provinces from 2003q1 to 2008q4 (that is T=24 and N=9). Based on the available data and the previously discussed literature, we selected and included six quarterly time series: the proportion of institutional deliveries (ID); average health centre client contacts per day (CC), which measures the staff workload (defined as the total number of patient visits divided by the total number of staff per day); traditional birth attendants per 1000 of the population (TBAs); the proportion of drugs available, based on the percentage of stock-outs of drugs on the essential drug list (DA); the average number of antenatal visits per quarter (ANC); and the population in the province (POP). CC and DA capture the quality of services, while cultural preferences and alternative options are captured by TBAs. A description of the variables is presented in Table 1.

Table 1 about here

3.2 Preliminary analysis

[4] Appendix 1 shows all the districts that abolished user fees in April 2006. Data from the following districts was discarded: Chibombo, Kapiri Mposhi, Serenje, Chienge, Chavuma, Lukulu, Siavonga and Milenge. In these districts, there were multiple missing months of information.

In Panel I of Table 2 we compare the means of the selected variables in the period prior to the abolition of user fees (2003q1-2006q1) to the means in the post-abolition period (2006q2-2008q4). A test of mean differences before and after the policy change finds statistically significant difference for antenatal visits, drug availability and health centre client contact, although these estimates do not control for any trends, prior to the abolition of user fees. Moreover, the standard errors do not account for any within-group correlation. Differences in means for institutional deliveries at the provincial level are presented in Appendix Table B. Panel II of Table 2 shows the correlation between the variables in the system. Institutional deliveries are statistically significantly correlated with the first lag, suggesting persistence and justifying the dynamic specification in the analysis. It is also evident that many of the previously described relationships from the literature hold in this data, at least at the level of correlations. ID is positively correlated with ANC, but negatively correlated with DA and TBAs. However, correlation is not a result that can stand on its own.

Table 2 about here

4. Empirical Methodology

4.1 Properties of the data

4.1.1 Panel unit root tests

To determine the appropriate methodology to be applied to the economic estimation in order to detect and avoid problems of spurious regressions, we first ascertain the time series properties of the data. Several statistics maybe used to test for the unit root in panel data, however, since the panel data set we have is not too long, we implement the Im, Pesaran and Shin (IPS) (2003), which combines the information from the time series and cross-section dimensions, such that fewer observations are needed for the test to have power. In contrast to the Levin, Lin and Chu (2002) test, the IPS (2003) t-bar test is based on the mean augmented Dickey-Fuller (ADF) test statistics and is calculated independently for each cross-section of the panel. Based on Monte

Carlo simulation results, IPS (2030) show that their test has more favourable finite sample properties than the Levine, Lin and Chu test. For a variable y_{it}, the p^{th} order Augmented Dickey Fuller (ADF) regression is given by

$$\Delta y_{it} = a_i + b_i y_{i,t-1} + c_i t + \sum_{j=1}^{p} d_{ij} \Delta y_{i,t-j} + u_{it} \tag{1}$$

where a_i and b_i are panel-specific intercepts and slopes, respectively, t is the time trend and u_{it} are assumed to be normally distributed stochastic terms for cross-section i, although variances are allowed to be heterogenous across panels. Whether or not the lagged dependent variable matters, statistically, underpins the empirical test; the null assumes not, while the alternative allows for correlation within at least one cross-section. In other words, the null

$$H_0 : b_i = 0, i = 1,..., N, \tag{2}$$

is tested against that alternative

$$H_1 : b_i < 0, i = 1,..., N_1; b_i = 0, i = N_1 + 1,..., N, \tag{3}$$

N_1 is such that N_1/N tends towards be a nonzero fixed constant as N goes to infinity. To test the null hypothesis against the alternative, IPS (2003) propose computing the simple average of the t-ratios of the ordinary least squares estimates of b_i in equation (1), i.e.

$$IPS = \frac{1}{N} \sum_{i=1}^{N} \tilde{t}_i, \tag{4}$$

where $\tilde{t}_i$, is the ordinary least squares t-ratio of b_i in the ADF regression in (1). If the errors in the different regressions contain a common time-specific component, IPS (2003) proposes a cross-sectionally demeaned version of the test. As a robustness check, we also calculate the panel unit root tests by Breitung (2000) which is a modification of the Augmented Dickey Fuller statistics that has more power than the IPS (2003) if individual specific trends are included (e.g. Baltagi, 2008).

Table 3 presents the results from these two panel unit root tests. Columns (I) and (II) report Breitung (2000) and IPS (2003) test statistics, with and without the trend, respectively. Both tests reject the null of a panel unit root for all the variables, except antenatal visits without a trend. Given cross-section dependence, detected by the tests reported in Table 4, we further consider the sensitivity of the panel unit root test results. Specifically, the Breitung (2000) and IPS (2003) tests augmented with the cross section averages, taking into account cross-sectional dependence, are also examined. The modified Breitung (2000) and IPS (2003) tests address cross-section dependence through demeaning, subtracting cross-section averages from the series (e.g. Levin, Lin and Chu, 2002). It has been shown that ignoring cross-section dependence may cause substantial size distortions (e.g. Baltagi, 2008). The results obtained from the sensitivity tests are not reported, as they are similar to those presented in Table 3. Therefore, we conclude that all the variables are trend stationary.[5]

Table 3 about here

4.1.2 Cross-section dependence test

As mentioned earlier, another problem that might arise in a panel context is the existence of cross-section dependence. In this case, cross-section dependence may be caused by the spill over effects across boundaries of districts due to the abolition of users. In fact, institutional deliveries in one region can be affected by not only a local change in fees charged (due to the abolition of user in the region), but also by similar changes occurring in other regions. In the presence of cross-sectional dependence of the error term, methods that assume cross-sectional independence would results in estimators that inefficient with biased standard errors, causing misleading inference. At a descriptive level, a statistic that captures cross-section dependence is the average pair-wise correlation coefficient:

[5] We also computed the Levin Lin and Chu (2002) and the results obtained were similar to those obtained using the the IPS (2003) panel unit root tests.

$$\bar{\rho} = \frac{2}{N(N-1)} \sum_{i=1}^{N-1} \sum_{j=i+1}^{N} \rho_{ij}, \tag{5}$$

where ρ_{ij} is given by

$$\rho_{ij} = \frac{\sum_{t=1}^{T} \mu_{it} \mu_{jt}}{(\sum_{t=1}^{T} \mu_{it}^2)^{1/2} (\sum_{t=1}^{T} \mu_{jt}^2)^{1/2}} \tag{6}$$

and μ_{it} are the residuals from equation (1), the ADF regression.

Given the potential for problems that might arise from cross-section dependence, we consider a diagnostic test, based on the above pair-wise correlation coefficients, Pesaran's (2004) CD test. Pesaran (2004) addresses shortcomings in Breush and Pagan's LM test, when N is large, and it is based on pair-wise correlations, rather than the square pairwise correlations used in the LM test. The test is robust to non-stationarity, parameter heterogeneity and performs well, even in small samples (e.g. Pesaran , 2004)

$$CD = \sqrt{\frac{2T}{N(N-1)}} \left(\sum_{i=1}^{N-1} \sum_{j=i+1}^{N} \hat{\rho}_{ij} \right) \tag{7}$$

Table 4 reports the average pair-wise correlation coefficient, and the Pesaran (2004) CD test statistic for all the variables, measured in levels. The results indicate the presence of cross-section correlation between the provinces for all the variables. Therefore, the analysis will make provision for this sort of correlation, when evaluating the impact of the policy change on institutional deliveries. Failure to do so may lead to misleading inference, especially if the source of the cross-section dependence is correlated with the regressors (Baltagi, 2008).

Table 4 about here

4.2 Interrupted time series

Although randomised experiments are seen as a gold standard for evaluating health care interventions, they are difficult to implement (e.g. Ranson et al., 2006). An alternative approach for the evaluation of policies, and recognised by the Effective Practice and Organisation of Care Group (EPOC) of the Cochrane Collaboration (Cochrane Effective Practice and Organisation of Care Review Group, 2002), is the interrupted time series (ITS) design (e.g. Cook and Campbell, 1979). In an ITS design, data are collected at multiple periods over time, but these periods must span both before and after the point in time, in which, an intervention or interruption is introduced. These data can be used to detect the intervention effect, parsing it from the underlying secular trend (Ramsay et al., 2003). ITS designs allow researchers, particularly in developing countries, to analyse the impact of interventions using routine data; oftentimes, opportunities to conduct a pre-intervention or baseline survey were not available (e.g. Lagarde, 2011). Routine data on health information, however, is often available, even several years before the intervention. Although an ITS design is superficially simple to implement, it is a powerful quasi-experimental approach for evaluating interventions. In contrast to comparing simple averages before and after intervention, ITS designs allow for the statistical investigation of potential biases in the estimation of the effect of an intervention. Potential sources of bias, which can be addressed in the ITS framework include: the existence of secular trends or non-stationarity, cyclical or seasonal patterns in the outcome variable and autocorrelation (e.g. Cook and Campbell, 1979).

However, there are several threats to the internal validity of ITS results. The lack of a randomised control group within the ITS design, for example, threatens the validity of the study. Another major threat is the (near) simultaneous occurrence of an event in addition to the intervention event to be studied (e.g. Ramsay et al., 2003). In this analysis, there does appear to be such an additional event. Although drug shortages occurred around the same time as the abolition of user fees, the shortages are incorporated in the analysis. Other threats are common to most empirical studies, espectially not having enough data to identify the impact. If the time series of observations is too short, it becomes difficult to detect secular trends (e.g. Crosbie, 1995). As a rule of thumb, detecting a policy impact with 80% power, when the autocorrelation parameter is 0.4, requires 10 pre- and post-intervention data points (ibid.). In this study, the ITS design is analysed using segmented panel data regression. Segmented regression analysis of ITS data allows for the statistical assessment of the intervention effect, both immediately and over time. The analsysis also incorporates factors, other than the intervention, which could explain the observed effect.

4.3 Model specification

As noted above, data is available for nine provinces (N=9), covering the period between 2003q1 and 2008q4 (T=24), while the analyisis is founded upon panel time series modelling, since the time dimension is dominant (e.g. Baltagi, 2008). Due to the strong persistence observed in institutional deliveries, we specify a dynamic panel model, including one lag of the dependent variable, to assess the impact of the abolition of user fees on institutional deliveries.

$$ID_{it} = a_i + bTime_t + cPostslope_{it} + \beta Intervention_{it} + \delta' x_{it} + dID_{i(t-1)} + \varepsilon_{it} \tag{8}$$

where ID_{it} is the vector of institutional deliveries in the $N = 9$ provinces; x_{it} is a vector of explanatory variables and includes time dummies (t1, t2 and t3) to account for the cyclicality observed in the institutional deliveries, a_i is the regional fixed effect; $Time_t$ is a vector of continuous values indicating time from the start to the end of the study period ; $Intervention_{it}$ is a vector of indicators coded 0 for the pre-invention period and 1 for the post-intervention period; $Postslope_{it}$ is a vector of indicators coded 0 up to the last point before the intervention and coded sequentially from 1 thereafter; and ε_{it} is a vector of disturbances.

Statistical inference from the model in (8), however, might be influenced by spatial correlation or cross-sectional dependence, driven by regional proximity, heteroscedasticty and/or serial correlation (due to the upward trend in institutional deliveries). Therefore, the analysis must consider these issues. Pooled Ordinary Least Squares (POLS) serves as the baseline model; however, Fixed Effects (FE) and Feasible Generalised Least Squares (FGLS) are also considered. In contrast to the POLS estimator, which assumes homogeneity in intercepts and slopes, the FE and FGLS estimators relax this assumption, albeit in different ways. Specifically, FE control for time-invariant omitted variables that differ by province, such as the level of development, health infrastructure and health staff, and, thus, allows for intercept heterogeneity. Moreover, an F-test (Pr>F=0.000) supports the existence of regional fixed effects. The FE method differences out the individual variability across regions based on the idea that within-variation eliminates much of the error variance. Thus, the FE estimator is results in a pooled OLS estimator on the differenced (demeaned) equation and yields unbiased estimates under the assumption of strict exogeniety. In the dynamic specification, the FE estimator with Driscoll and Kraay (1998) standard errors which are robust to moderate levels of cross-sectional dependence in the error term, are implemented[6].However, this adjustment does not correct for the Nickel bias. The inclusion of lags of the dependent variable in the FE regression yields biased estimates, when T is small, although the bias approaches zero as T approaches infinity (e.g. Nickell, 1981). Judson and Owen (1999) find a 20% bias, when T=30, although their coefficients were correctly signed. Although Kiviet (1995)[7] suggests a bias-correction procedure suitable for small T and moderate N (10<N<20), results from the implementation of that procedure are not discussed here.

While the FE estimator assumes random intercepts and homogenous slopes, it also disregards between-variation variation which can yield biased standard errors and consequently incorrect inference. Differently from the FE estimator, the Feasible Generalised Least Squares (FGLS) treats all parameters as random and does not impose any restriction on the error

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[6] Cross-section dependence is often managed through the inclusion of year dummies. In our case, inclusion of time dummies in the FE model – controlling for either cross-sectional dependence or global shocks affecting all provinces simultaneously – may create multicollinearity, due to the presence of indicator and time variables used to capture the policy impact.

[7] We implemented the Kiviet (1995) correction for FE small sample bias, but the results obtained were similar to those obtained without the correction

structure, allowing for autocorrelation within panels, cross-section correlation and heteroscedasticity across the units (Kmenta, 1986). Specifically, the specification takes the form

$$ID_{it} = a_i + bTime_t + cPostslope_{it} + \beta_i Intervention_{it} + \delta' x_{it} + dID_{i(t-1)} + \varepsilon_{it} \quad (9)$$

In the FGLS, the regression disturbances comprise three dimensions: the three separate components are associated with 1) time, 2) space and 3) with both time and space. (e.g. Podesta, 2002). Furthermore, to link the differences in the policy change within regions to characteristics that vary across regions; we adopt Zellner's (1962) seemingly unrelated regression (SUR). The SUR is a system of individual regressions in which cross equation errors are allowed to be correlated and takes account of cross-section or spill-over effects among the regions. Therefore, FGLS can be interpreted as pooled SUR in which estimates represent the average values of the regional coefficients since they vary across regions. By stacking the observations in the t-dimension, the model has the following SUR representation

$$\begin{aligned} &ID_t = a_t + bTime_t + cPostslope_t + \beta Intervention_t + \delta' x_t + dID_{(t-1)} + \varepsilon_t \\ &t = 1, \ldots T \end{aligned} \quad (10)$$

where ε_t is a vector of contemporaneous disturbances, and the rest of the variables have been described, above. With this specification, we allow for heteroscedasticity and correlation across separate equations for each year. The SUR model is best suited for estimation with cross-section dependence, since it captures the correlation in the error terms across cross-sections, especially when $T > N$ (e.g. Baltagi, 2008). It also allows for detailed region-specific analysis.

5. Estimation Results and Discussion

Analysis results are presented, first at the aggregate level, using the models discussed above, and then at the disaggregated level.

5.1 Impact of the policy change at aggregate level

Results of the static and dynamic representations for the three estimators are presented in Table 5. Panel I contains results from a simple model with only ITS controls, and, therefore allows for POLS, FE and FGLS. Panel II, on the other hand, includes additional controls, but focuses on the static representation; therefore, results in panel II also include POLS, FE and FGLS. Finally, panel III contains results from the dynamic model, which includes one lag of the dependent variable; thus, only FE and FGLS results are potentially meaningful.

Table 5 about here

Because the assumptions associated with each set of models in each of the panels differ, it is necessary to whittle down the results to the one result or set of results that is most plausible. Within I, where all of the explanatory variables are based on the ITS design, the assumptions are: (i) there is no persistence in the dependent variable and (ii) no other variables influence institutional deliveries, other than the policy. Within II, assumption (ii) is relaxed, and, within III, both (i) and (ii) are relaxed. Persistence in the dependent variable leads to a preference for results in III, rather than results in either I or II. Additionally, recall that POLS is underpinned by homogeneity of all effects across regions and time, and, therefore, is not valid in a dynamic setting, while FE allows for time-invariant regional differentiation and FGLS allows for unspecified correlation in the errors. In this setting, both autocorrelation – (Pr>F=0.0045) based on a test outlined in Wooldridge (2001) and heteroscedasticity – (Pr>Chi2=0.000) following the modified Wald test outlined in Greene (2003) – are present in the data. Therefore, the FGLS specification is generally preferred. In other words, the results in the last column represent the preferred results.

Therefore, based on the FGLS estimates, we conclude that there was an immediate 1.2% increase, or 3.4% per quarter increase, following the policy change.[8] However, no statistically significant quarter-to-quarter increase in the trend in institutional deliveries could be identified, which means that institutional deliveries did not continue to rise after the immediate increase. However, the lagged dependant variable is postive and statistically significant, suggesting that previous deliveries in the facility affect current institutional deliveries, a result that is consistent with previous literature (e.g. Bell et al., 2003; Nwakoby, 1994). Akin to previous literature, the coefficient on drug availability (DA), which proxies for the quality of services is positively associated with institutional deliveries, whilst the presence of TBAs is negatively associated with institutional deliveries (e.g. Gabrysch and Campbell, 2009) ANC visits, however, are not significantly associated with institutional deliveries and the result differs from previous analyses finding that ANC uptake is highly predictive of institutional delivery (e.g. Gage, 2007). Provincial-level population growth has a negative and statistically significant impact on institutional deliveries, which is expected, since population growth can strain the provision of health services, especially if the supply of healthcare inputs remains fixed.

5.2 Provincial-level analysis of the impact of the policy change

Although the preceding results imply that the elimination of user fees for delivery services increased the use of institutional delivery services, at least at the national level, that analysis could mask impact heterogeneity at the provincial level. We now turn to this consideration, through the estimation of a SUR model. The results of the analysis are presented in Table 6. Panel I contains region-specific regression results. In addition to the SUR estimates, we also report Breusch and Pagan's (1980) LM test of spatial independence. If rejected, the SUR estimator improves the efficiency of the region-specific estimates, through the incorporation of cross-

[8] We obtain the 3.4% increase in facility-based deliveries by dividing the percentage point change by the pre-intervention mean (1.2/0.349/=3.4%).

equation residual correlation. Panel II, in Table 6, describes the degree of correlation between those residuals.

Table 6 about here

As might be expected in any country, and possibly moreso in a developing country, policy impacts are not estimated to be the same across all regions. The results in Panel I suggest that the abolition of user fees led to a statistically significant and immediate negative reduction in institutional deliveries in two provinces. In the Copperbelt and Western provinces, that reduction was 8.3 percentage points (17% with reference to the baseline) and 4.7 percentage points (13% with reference to the baseline) per quarter respectively. However, post-intervention, there was a trend increase in institutional deliveries, quarter-on-quarter, in four of the nine provinces (ranging from 1.9% in Luapula to 6.8% in Lusaka), although Central province experienced a relatively small, but statistically significant, decrease in institutional deliveries (0.7 percentage point (2.3%) overtime. In addition to the previously uncovered differences across regions, cross-sectional independence is rejected (Pr=0.0249).

Although the reduction in institutional deliveries is unexpected, one can speculate that the reduction in user fees could possibly have increased the utilisation of other health services, which, in turn, had a negative effect on institutional deliveries. For instance, the Copperbelt province is a relatively urban province and a hub for health care professionals; therefore, individuals from the surrounding regions often travel to the province to seek care. In fact, provincial level correlations between the residuals, described in the correlation matrix presented in Panel II, indicate a relatively high degree of correlation between the Copperbelt and other regions. It might also have been expected that provinces close to each other would exhibit relatively larger degrees of dependence; that is not entirely true. For instance, about 5 provinces, namely, Luapula, Northern, Northwestern, Southern and Western provinces have a relatively high positive correlation with the Copperbelt province, but not necessarily with regions that are close to them (see map of Zambia in appendix). Potentially, there is spatial autocorrelation, which needs to be addressed further, using spatial techniques taking into account the distance between regions.

Since user fees are not the only factors determining facility delivery, and, therefore, the relative importance of fees relative to other barriers (such as quality of care) is likely to vary from province to province, these results are not unexpected. Furthermore, some provinces may have greater capacity to deal with an increase in utilisation, and, thus, maintain the quality of care provided. In support of the previous hypothesis, there is evidence that the drugs and financing that were meant to be provided, ostensibly to help health centres deal with the hoped-for influx in deliveries, were not successfully delivered to all districts and facilities (e.g. Carraso et al., 2010; Cheelo et al., 2010).

5.3 User fees and traditional birth attendants

User fees were removed to increase access to health services and improve health outcomes particularly for the poor in rural communities. The abolition of user fees reduces the financial cost of treatment, and is expected to increase utilisation rates. Studies in Zambia and other developing countries have uncovered increases in the use of health services by some population groups, after the removal of user fees (e.g. Lagarde et al., 2012; Masiye et al., 2008; Nabyonga et al., 2005; Wilkinson et al., 2001), and our results at the national level are consistent with that in literature. Moreover, the findings highlight the importance of quality of services in encouraging institutional deliveries at the national level but there are important variations at the regional level.

The national level findings suggest that TBAs statistically significantly reduce institutional deliveries. At the regional level, a similar result is found for the Copperbelt province, while in regions such as the Eastern, Luapula and Lusaka provinces the association is positive. The positive association suggests that harnessing the potential of TBAs by possibly providing the right environment for them to operate could lead to an increase in TBAs are trained to carry-out deliveries and are advised to refer more complicated cases to higher levels of care. Within Zambia, the involvement of trained TBAs in the delivery process remains an important strategy, particularly in rural areas, where health worker scarcity is a problem (Stekelenburg et al., 2004). Furthermore, trained TBAs can reduce perinatal deaths, neonatal deaths and stillbirths (e.g. Ana, 2011; Gill et al., 2012), although others argue that TBAs offer poor obstetric services (e.g. Garces et al. 2012, Singh et al., 2012). Between 1970 and 1990, the World Health Organisation promoted TBA training, as one strategy to reduce maternal and neonatal mortality; however, there is insufficient evidence to establish the potential for TBA training to improve peri-neonatal mortality (e.g. Sibley et al., 2012). Given that a larger share of women in Zambia were assisted by TBAs in 2007 (23.5%) compared to 2001/2 (11.7%) (e.g. Central Statistical Office, 2009), the role of TBAs cannot be ignored. Similarly, the scarcity of health personnel in low-income countries, especially personnel focussed on women's health, means that non-institutional deliveries will continue to play a significant role in health service provision (e.g. Limwattananon et al., 2011). Thus, reducing maternal mortality may require the implementation of interventions, which are country-specific and include TBAs, due to differences in the local contexts.

Additionally, although user fees were not a significant source of revenue to the health facilities, due to routine costs, they were a flexible form of income used by health facilities in rural areas to support TBAs. For example, user fees were often redistributed to TBAs, in the form of tokens of appreciation, to encourage women to deliver at health facilities (e.g. Cheelo et al., 2010). Fees were also used to purchase cleaning agents (bleach) and food for inpatients (ibid). However, with the abolition of user fees, TBA support has been significantly reduced; thus, the incentive for TBAs to encourage woman to deliver at health institutions has also reduced. Moreover, following the abolition of fees in Ghana, Benin and Zambia, women are reportedly required to bring bleach, gloves and syringes with them, when delivering at a health facility (e.g. Borghi et al., 2003; Carrasso et al., 2010). Additional requirements, such as these, act as a barrier to the utilisation of delivery services, and may even exceed the levels of the abolished user fees.

Such barriers are problematic. In comparison, TBA delivery costs are reportedly affordable, especially for the poor, because the payments are negotiable, both in amount and timing, while in-kind payments are also accepted (e.g. Amooti-Kaguma, 2000).

6. Conclusion

This research investigates the impact of user fee abolition on institutional deliveries using a panel of 9 Zambian provinces covering the period 2003q1 to 2008q4. Different models are estimated to address heterogeneity, autocorrelation, heteroscedasticity and cross-section dependence within a panel data context. After the econometric issues have been addressed, the aggregate results provide strong evidence that the abolition of user fees had an immediate positive impact on institutional deliveries. However, the increase was not sustained via an increase in the trend, and the increase was economically small, 1.2% or 3.4%, depending on how the increase is measured. The aforementioned aggregate increase could not be ascribed to an increase in any particular region. Instead, immediate decreases were uncovered in some regions, while trend increases were uncovered in other regions. In other words, the aggregate results mask interesting regional heterogeneity. From a policy perspective, that heterogeneity is likely to be important; motivations for non-institutional delivery might be social or cultural, factors not easily altered through reductions in the direct cost of delivery. Similary, although it was not possible to consider indirect charges, as data was not available, anecdotal evidence suggests that facilities have followed a cost-shifting strategy, and that strategy could account for the economically small user fee impacts estimated here.

In addition to user fee effects, the analysis also identified a TBA impact and a quality of service impact. At the aggregate level that TBAs are associated with a reduction in institutional deliveries; however, at the regional level, the impact was more varied. In some provinces, the association was positive, while in others it was negative. With respect to service quality, a strong positive impact was uncovered at the national level, a result that carried over to a number of provinces. Again, the aggregate results masked interesting regional heterogeneity.

Although the conclusions are reasonably general, there are some limitations worthy of further analysis. For example, the routine data used in the analysis could either be too sporadic or too frequent. Quarterly data provides less data than monthly data, and might hide trends in utilisation; however, both quartery data and monthly data might also be too noisy for the identification of trends. Another concern is that routine data neither provides information about non-users nor provides socio-economic and related characteristics of users. Specific information about users and non-users is potentially useful in exploring response heterogeneity, e.g. user fee abolition may have affected poor women differently than non-poor women. Unfortunately, the observed increase could reflect better recording, rather than actual increases in institutional deliveries. Additionally, the routine data only covered public health facilities in Zambia. Thus, the observed changes in utilisation might not be a reflection of all health facilities in the country, since most private health facilities are not, yet, incorporated into the HMIS.

Future research, which is likely to complement this research, should seek to link individual-level data, from household surveys, to facility-level data. Such a link between the supply and demand sides of the market could underpin an analysis of the impact of the policy change on maternal health-seeking behaviour. Recent Demographic and Health Survey data include geographic positioning system (GPS) information, which can be used to tie sample clusters to routine data. Data tied together in this fashion could be used for the analysis of mother's health or child health, relating user fee abolition policies to maternal health outcomes and, subsequently, to child health outcomes. Finally, future research could consider modelling other types of spatial correlation which gives more importance to the distance between regions.

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Appendix A: List of 54 districts where user fees were removed for the entire district on 1st April 2006.

Luapula Province	Western Province	North Western Province
Kawambwa	Kaoma	Kabompo
Chiengi	Lukulu	Kasempa
Milenge	Kalabo	Mwinilunga
Mwense	Senanga	Chavuma
Nchelenge	Sesheke	Zambezi
Samfya	Shangombo	Mufumbwe

Northern Province	Southern Province	Lusaka Province
Chinsali	Gwembe	Luangwa
Isoka	Itezhi-tezhi	Chongwe
Kaputa	Kalomo	Kafue
Chilubi	Kazungula	
Luwingu	Monze	
Mpika	Namwala	
Mporokoso	Siavonga	
Mpulungu	Sinazongwe	
Mungwi		
Nakonde		

APPENDIX B: Means of institutional deliveries at provincial level

	Before	After	Diff
Central	0.305	0.314	0.009
se(mean)	(0.008)	(0.007)	(0.012)
Copperbelt	0.476	0.387	-0.088**
se(mean)	(0.019)	(0.013)	(0.024)
Eastern	0.315	0.369	0.054**
se(mean)	(0.012)	(0.018)	(0.021)
Luapula	0.307	0.382	0.075***
se(mean)	(0.005)	(0.013)	(0.013)
Lusaka	0.296	0.356	0.060***
se(mean)	(0.009)	(0.020)	(0.021)
Northern	0.275	0.283	0.007***
se(mean)	(0.006)	(0.005)	(0.008)
N/Western	0.503	0.525	0.022***
se(mean)	(0.011)	(0.021)	(0.023)
Southern	0.278	0.291	0.014
se(mean)	(0.007)	(0.012)	(0.014)
Western	0.370	0.363	-0.006
se(mean)	(0.013)	(0.009)	(0.016)

$^{+}$ Diff gives the difference between before and after the intervention (After-before).

Table 1: Description of the variables

Variable	Description	N	T
ID	Proportion of institutional deliveries	9	24
ANC	Average antenatal Visits	9	24
CC	Average health centre client contact	9	24
TBAs	Number of traditional birth attendants per 1000 of the population	9	24
DA	Proportion of drugs available	9	24
POP	Population in province (transformed to logs)	9	24

Table 2: Data summary

		ID	ANC	DA	CC	TBAs	POP$
Panel I: Means and standard errors							
All	Mean	0.355	2.935	0.722	25.728	0.241	11.617
(n=216)	Se	(0.006)	(0.022)	(0.007)	(0.497)	(0.004)	(0.016)
Before	Mean	0.347	3.112	0.745	24.310	0.248	11.570
(n=117)	Se	(0.008)	(0.025)	(0.010)	(0.632)	(0.005)	(0.022)
After	Mean	0.364	2.714	0.696	27.061	0.233	11.672
(n=99)	Se	(0.008)	(0.025)	(0.011)	(0.767)	(0.006)	(0.024)
Diff+	Mean	0.016	-0.386*	-0.049*	3.098*	-0.015*	0.102
	Se	(0.012)	(0.035)	(0.014)	(0.972)	(0.008)	(0.032)
Panel II: Correlation Matrix							
	ID	1					
	ANC	0.219*	1				
	DA	-0.121*	0.095	1			
	CO	-0.032	-0.208*	-0.387*	1		
	TBAs	-0.139*	0.184*	0.503*	-0.217*	1	
	POP	-0.378*	-0.378*	-0.149*	0.442*	-0.397*	
	ID_1	0.830*	0.219*	-0.170*	0.013*	-0.388*	1

Notes: Panel I shows the cross regional means and standard errors of the seven variables. Standard errors are reported in parentheses. Panel II shows the cross regional average correlations among all the variables. * denotes significance at 5% level. + Diff gives the difference between before and after (After-before). $ Population has been transformed to log values.

Table 3: Breitung and Im Pesaran and Shin (IPS) unit root test statistics

	(II)Breitung t-stat		(III)IPS Wtbar	
	Without trend	With trend	Without trend	With trend
ID	-2.778*	-3.414*	-3.320*	-3.774*
ANC	0.035	-3.535*	-1.547	-3.777*
CC	-4.204*	-4.212*	-3.090*	-4.380*
TBAs	-2.890*	0.228	-2.367*	-2.882*
DA	-2.896*	-1.621*	-2.761*	-3.341*
POP	1.567	-2.672*	-0.607	-4.812*
ID_1	-2.685*	-1.878*	-3.237*	-3.690*

Notes: * indicates that the test is significant at the 5% level.

Table 4: Cross section dependence of all variables

Variable	$\bar{\rho}$	CD test
ID	0.331	9.72*
ANC	0.731	21.49*
CC	0.382	11.24*
TBAs	0.156	4.58*
DA	0.229	6.74*
POP	0.998	29.32*
ID_1	0.367	10.56*

Note: $\bar{\rho}$ and Pesaran (2004) CD test are computed as in (6) and (7) respectively. * indicates that the coefficient is significant at 5% level.

Table 5: Estimation results: OLS, FE, RC, and FGLS estimates of the impact of the abolition of user fees on institutional deliveries

	(I)Indicators only			(II)Static			(III)Dynamic	
ID	POLS	FE	FGLS	POLS	FE	FGLS	FE	FGLS
Inter	-0.014	-0.014	0.001	-0.004	-0.006	-0.003	0.009	0.012**
Postslope	0.005	0.005**	0.006**	0.004***	0.005***	0.005**	0.004**	0.001
Time	-0.001	-0.001	-0.001	0.002**	-0.008	0.002**	-0.007***	-0.000
t1	-0.020	-0.020**	-0.020***	-0.009	-0.039	-0.013***	-0.015*	0.010**
t2	-0.014	-0.014**	-0.014***	-0.010	-0.029	-0.011***	0.001	0.030***
t3	0.035*	0.035***	0.035***	0.029***	0.019	0.032***	0.047***	0.072***
ANC				0.050***	0.048***	0.038***	0.015	0.013
DA				0.156***	0.182***	0.067**	0.143***	0.039**
CC				-0.002	-0.002	0.000	-0.002**	0.000
TBAs				-0.096	-0.077	-0.048*	-0.003	-0.035*
POP				-0.048	1.149	-0.152***	0.797***	-0.022*
ID_1							0.470***	0.848***
Constant	0.354***	0.354***	0.342***	0.711	-13.062	1.940***	-9.088***	0.224
F-test (Pr>F)	0.017	0.000		0.000			0.000	
Wald test (Pr>Chi2)			0.000			0.000		0.000

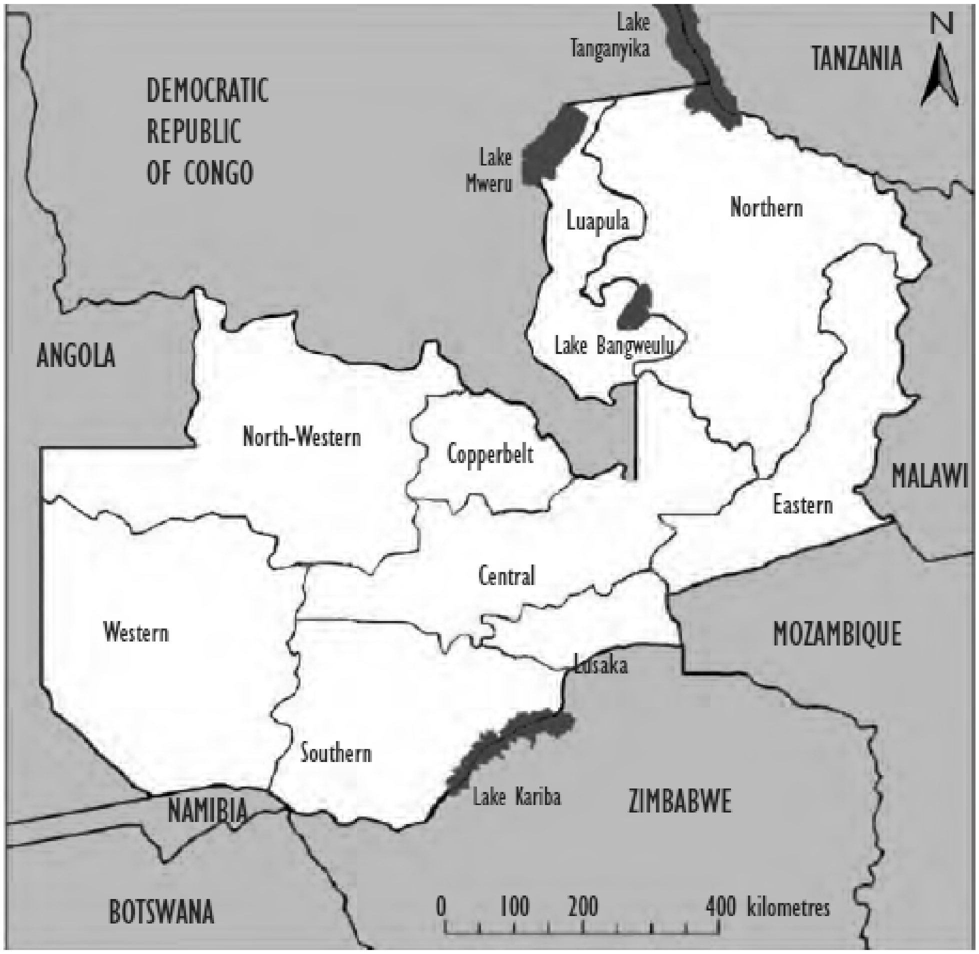
Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 6: Seemingly Unrelated Regression Estimates: Provincial level

	Central	Copperbelt	Eastern	Luapula	Lusaka	Northern	N/Western	Southern	Western
Panel I: Provincial estimates of the impact of the abolition of user fees on institutional deliveries									
Inter	0.044	-0.083***	-0.004	0.003	-0.031	-0.017	-0.003	0.018	-0.047**
Postslope	-0.007**	0.012***	0.004	0.006***	0.020***	0.001	0.006	-0.001	0.019***
Time	-0.000	-0.011***	0.005**	-0.001	0.000	0.002	0.002	-0.002	-0.003
t1	0.016	-0.072***	-0.011	-0.004	0.036**	-0.001	-0.013	0.001	-0.010
t2	0.016	-0.073***	0.015	0.014	0.041**	0.011	-0.008	0.006	0.010
t3	0.065***	0.008	0.065***	0.033***	0.021	0.028***	0.054**	0.028***	0.054***
ANC	-0.043	-0.063***	0.052	0.006	0.032	0.021	0.055	-0.100**	0.052*
DA	0.067	-0.248***	0.175**	0.217***	0.116	-0.005	0.129	0.185***	0.474***
CC	-0.001	-0.001	-0.003***	-0.006***	-0.006**	0.000	-0.004*	-0.006***	0.004*
TBAs	-0.100	-0.153***	0.264***	0.171***	0.352***	0.032	0.032	0.045	0.159
Lpop	0.024	0.115***	-0.019	0.012*	-0.023	0.016	0.026	0.041***	-0.020***
ID_1	0.500***	-0.341***	0.474***	0.391**	0.707***	-0.061	0.059	0.214	-0.096
Breush_Pagan test of independence: chi2(36) = 54.464, Pr = 0.0249									
Panel II: Correlation Matrix of residuals									
Central	1								
Copperbelt	0.431	1							
Eastern	-0.0862	0.0727	1						
Luapula	0.1857	0.4216	0.0954	1					
Lusaka	0.0786	-0.2938	0.3061	-0.2045	1				
Northern	0.0156	0.4126	-0.1628	0.1367	-0.2732	1			
N/Western	-0.2096	0.3159	0.1979	0.3097	0.0121	-0.1191	1		
Southern	0.2738	0.4985	-0.0193	0.2414	-0.2064	0.3901	0.041	1	
Western	0.1398	0.3188	-0.4521	-0.2136	0.2000	0.3318	-0.1129	0.1505	1

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Panel I shows the SUR estimates for each province. Panel II shows the cross-provincial correlation matrix

Figure 1: Map of Zambia showing provinces



Source: CSO, TDRC, UNZA, Macro International, 2009